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MEDICAL AND PHYSIOLOGICAL

COMMENTARIES.

BY

MARTYN PAINE, M. D. A. M.

——Vitæ tam vires quam actiones expono. — *Biblia Naturæ.*
Morborum quoque te causas et signa doceo. — *Virgil.*

IN TWO VOLUMES.

VOL. II.

NEW-YORK:
COLLINS, KEESE & Co. 254 PEARL-STREET.
LONDON:
JOHN CHURCHILL.
1840.

W
P146m
1844
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PRINTED BY
Hopkins & Jennings,
No. 111 Fulton-st. N Y

CONTENTS OF VOL. II.

I.

PAGE

PHILOSOPHY OF ANIMAL HEAT. . . . 9

II.

PHILOSOPHY OF DIGESTION. . . . 77

APPENDIX

On spontaneous generation. 123

III.

THEORIES OF INFLAMMATION. . . . 142

APPENDIX

On the state of the circulation in fever. 209

IV.

PHILOSOPHY OF VENOUS CONGESTION.

SECTION I. — First observations upon phlebitis. — Venous congestion early regarded as an important condition of disease. — Opinions of authors as to the nature and causes of venous congestion. 216

SECTION II. — Armstrong's doctrines considered. — The lungs, in their relation to the doctrines of obstruction and remora. — Mechanical impediments to the venous circulation not a cause of disease. 230

SECTION III. — Supposed obstructions to the venous circulation farther considered. — Debility and relaxation of the veins. — Original case. . . 256

SECTION IV. — Inductions from the effects of remedies in venous congestion. 268

SECTION V. — Inductions from the causes of venous congestion. . . 276

SECTION VI. — Inductions from the symptoms. 281

SECTION VII. — Morbid anatomy of the veins. — What constitutes inflammation. — All tissues liable to this disease. — Hypothetical doctrines considered. 299

SECTION VIII. — Organization of the veins, &c. 331

SECTION IX. — Analogies supplied by varix, hypertrophy of the veins, &c. — The exciting and pathological causes of varix, and venous hypertrophy. — Morbid products of varix. — Analysis of a case of venous hyper-

PAGE

trophy, from Cruveilhier. — The doctrine of spontaneous formations in the current of venous blood. 337

Section X. — The veins possess the power of dilating actively. . . 379

Section XI. — On the powers which circulate the blood. . . . 398

Section XII. — Analogies resumed. — The doctrine of venous circulation applied to the philosophy of venous congestion. — The mechanical rationale of the operation of bloodletting, &c., in venous congestion. — Louis' cases of "the typhoid affection." — The complications of venous congestions with idiopathic fever. The modifying, and other influences of the former condition. — Pus. — Active phlebitis variously considered in its relations to venous congestion. — Products found in congested veins. — Typhus fever. — Humoral experiments. — Phlebitis and venous congestions, induced by irritating injections into the veins, manifest some of the phenomena of typhus fever. — Puerperal fever. — Puerperal, and endemic erysipelas. — Endemic at the New-York Alms-house. — Phlegmasia dolens constituted by venous or lymphatic inflammation. — Purpura hemorrhagica. — Scurvy. — Venous inflammation according to the nature of the remote causes. — Venous congestion as induced by narcotic poisons, carbonic acid gas, alcohol, &c. — Congestive asthma. — Modifications of venous inflammation farther considered. — The philosophy of the predisposing causes of disease. — Contagion. 427

Section XIII. — Condition of the various tissues of a congested organ. — Incipient seat of venous congestion. 514

Section XIV. — Venous congestion passes readily into inflammation of other tissues. — The rationale considered. 519

Section XV. — Active and passive inflammation and congestion. — No fundamental distinction. 524

Section XVI. — Philosophy of spontaneous hemorrhage, and its application to the pathology of venous congestion. 546

APPENDIX I.

Demonstration of the disease. 569

APPENDIX II.

The importance of analogy and principles in medicine. 574

APPENDIX III.

Cold, as a cause of venous congestion. 590

APPENDIX IV.

Pathology of erysipelas. 603

APPENDIX V.

Pathology of tubercle and scrofula. 608

PAGE

APPENDIX VI.

Melanosis, animal pigments, and the philosophy of adventitious growths. . 637

V.

COMPARATIVE MERITS OF THE HIPPOCRATIC AND ANATOMICAL SCHOOLS. 641

VI.

ON THE PRINCIPAL WRITINGS OF P. CH. A. LOUIS, M. D. 679

ERRATA.

Page 102,	thirty-seventh line from top,	for *its acid,*	read	it is acid.
103,	tenth "	*analysts,*	"	analysis.
108,	seventeenth "	*glutinous,*	"	gluttonous.
139,	twenty-third "	*caninomatous,*	"	carcinomatous.
178,	thirty-fourth "	*εναμτιων*	"	*εναντιων.*
696,	ninth "	*renders tubereles,*	"	renders the presence of tubercles.
700,	thirty-ninth "	*thickened,*	"	thinned.
703,	thirty-seventh "	*imperfectly described,*	"	such imperfectly described.
708,	twelfth "	*mucous,*	"	serous.
720,	fifteenth "	*ought not to outweigh,*	"	ought to outweigh.
"	thirty-first "	*Vol. I p.* 198,	"	Vol. ii. pp. 36, 450.

" To note (3) Vol. ii. add 450.

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PHILOSOPHY OF ANIMAL HEAT.

"We must allow the bodies of living animals and vegetables to form an original cause of heat, as much beyond our power of explaining as the source of the sun's heat."— *Moore's Medical Sketches.*

"It is most probable that the power of generating heat in animals arises from a principle so connected with life, that it can, and does, act independently of circulation, sensation, and volition; and is that power which preserves and regulates the internal machine." — *Hunter's Observations on certain Parts of the Animal Economy.* (1)

"The extrication of caloric is a phenomenon exactly analogous to those of which the general capillary system is the seat."

"The disengagement of caloric is always subordinate to the state of the vital forces."

"The state of respiration has no influence upon the actual heat of the body."

"When we place on one side all the phenomena of animal heat, and on the other the chemical hypothesis, it appears to me so inadequate to their explanation, that I think every methodical mind can refute it without my assistance." — *Bichat's General Anatomy applied to Physiology and Medicine.* (2.)

SECTION I.

Bichat, however, was clearly mistaken in the foregoing sentiment. Vital physiologists, almost by common consent, have abandoned the subject of animal heat to the chemists. It is the only function of life, in which the forces of chemistry appear to the former to have any participation; but such has been the interference of the laboratory with every other, that chemistry has acquired plenary indulgence in this branch of physiology. The only connection, however, which has been reasonably shown, of the forces of chemistry with the process of respiration, relates to the decomposition of the air. From this, and from the

(1) P. 91, 1786. (2) Vol. i, art. 1, s. 9.

necessity of respiration to animal temperature, in common with the other products of vital actions, has followed the induction, that the generation of heat is a chemical process.

In the first place, however, it is by no means certain that the decomposition of the air is not effected by the forces of life. Analogy is strongly in favour of this conclusion; since we observe in all the secreted products from the blood, that decompositions, and recombinations, more or less resembling the results of chemical actions, are effected by the vital forces. The decomposition of the air takes place, also, through the medium of a highly organized tissue; and whatever may be said of *endosmose* and *exdosmose*, the analogy will not hold, since they apply to dead matter, as we have endeavoured to show in another place. Every function, too, of the mucous membrane of the lungs is strictly vital. Analogy should be allowed to be very strong in this case, as well as in the generation of animal heat. We think that we shall have proved in our different essays, by the showing of chemistry itself, that the forces of physics have no connection with any other function or product of organized matter; and it would, therefore, be a very strange anomaly in nature, if the decomposition and absorption of air, the excretion of carbon, and the elaboration of heat, were not vital processes.

Again, respiration is less essential to the production of animal heat, than to many other living processes. It is intimately concerned, indeed, with every function of life; and we might, therefore, equally argue, that every function is the direct result of respiration, and is of a chemical nature.

Bichat, following the path of his great pioneer, has briefly, but ably, refuted the chemical theories of animal heat. Still they live and flourish as ever. One of the latest interpretations of this subject is set forth in the following typographical manner:

"There is a perpetual *deposition*, by the capillary system, of *new matter*, and decomposition of the *old*, all over the frame, influenced by the nerves: in other words, the galvanic influence of the nerves, which occasions these depositions and decompositions, keeps up a slow combustion. In this decomposition there is a continual disengagement of carbon, which mixes with the blood returning to the heart at the time it changes from scarlet to purple; this *decomposition*, being effected by the *electric agency* of the *nerves*, produces constant extrication of caloric: again, in the lungs that carbon is thrown off and united with oxygen, during which *caloric* is again *set free;* so that we have in the Lungs a Charcoal Fire constantly burning, and in the Other Parts a Wood Fire, the one producing *carbonic acid gas*, the other *carbon;* the *food supplying*, through the circulation, the vegetable or animal *fuel*, from which the *charcoal*

is *prepared* that is *burned* in the *lungs*. It is thus that the animal heat is kept up." (1)

We believe something like the foregoing to be the prevailing doctrine; at least, it has been often endorsed as a lucid exposition of the subject. Thus, also, another distinguished physiologist, Professor Elliotson:

"The evolution of animal heat is a mere instance of combustion in the extreme vessels, — the union of carbon and oxygen, (taking place in these vessels) being always attended by an increase of temperature; and we may equally abstain from troubling ourselves about relative capacities for caloric." Again, our author says, "whether the theory be correct or not, the production of animal heat must be as evidently a chemical process, as changes of temperature among inanimate bodies." (2)

(1) Billing's First Principles of Medicine, p. 21, 1838.

(2) Is this conclusion justified by any remarkable analogies between the "production" of animal heat, and the "changes of temperature among inanimate bodies"? What, too, can our author mean by saying, that "the fact of local heats above the temperature of the general mass of blood proves that heat is evolved by *local* processes"? Our author also says that "*habit* has a great influence on the *calorific powers* of animals." And how shall we explain the effect of the nervous influence?

Notwithstanding the well ascertained fact, that as much carbonic acid is evolved by the starving, as by the feasting, subject, — whilst the temperature of the former is always reduced; and although the quantity of carbonic acid evolved is exceedingly variable in all at different hours of the day, and without any intelligible cause, this doctrine of the absorption of oxygen, — and its combination in the extreme vessels with carbon, is not only made by many the safe foundation of the origin of the unvarying animal temperature, but it is assumed as the basis of the modifications that are supposed to be generated by differences in external temperatures. Thus, it is said, that "in high temperatures we have less necessity for the evolution of heat; in low temperatures, more. Accordingly, in the former, the arterial blood remains arterial, — is nearly as florid in the veins as in the arteries, and the inspired air is less vitiated; in low temperatures, the venous blood is extremely dark, and the inspired air more vitiated.

Our author, however, states that "Sir A. Cooper placed a puppy and a kitten, some weeks old, nearly to the mouth in iced water, till they died; and that the blood of the lips, nose, toes, mesentery, and left side of the heart, was of a fine vermilion hue." (*a*) It is also a well known fact, that if there be any difference in the colour of the blood in different climates, it is darker in the tropical than in the northern regions.

Nevertheless, the decomposition of the air, the absorption of oxygen, its combination with carbon, and the consequent evolution of animal heat, are made to depend on the mere contingencies of the varying states of the external temperature, — and for the philosophical reason that the body requires more heat in low than it does in high atmospheric temperatures. But whilst we admire the simplicity of such a convenient law, we think we discover something in other laws of the animal body, and something in those of chemistry itself, to contradict the hypothesis; and when we contend for an equal or even greater "evolution of heat" in high than in low tempe-

(*a*) Elliotson's Human Physiology, Part 2, pp. 238, 242, 239, 240, 1837. See our Vol. I. p. 49.

It is our purpose to examine the connection of respiration with animal heat, and to show its essential dependence on other causes by the experiments of chemical philosophers. We shall endeavour to sustain the conclusions of Hunter and Bichat, although we see much to admire in the ingenious observations of those who depart from the doctrine of the vitalists. But, it is the ingenuity of mind, not its adherence to nature, which imparts, in our estimation, the merit that may belong to other hypotheses. And here their advocates have an apparent intellectual superiority over others who are alone concerned in forming their deductions from the phenomena of nature. The proofs, which the latter enjoy, are so plain and intelligible, that one seems scarcely deserving praise who does but follow their indications. And this, we apprehend, is one reason why so little respect is now paid by many to the system of nature as expounded by Hippocrates, Bacon, Sydenham, Hunter, Bichat, and other kindred spirits. On the contrary, however, artificial hypotheses, to be at all plausible, often require the highest efforts of genius; and this is at once conspicuous in the management of facts, and the arrangement of assumptions, so that they shall borrow, as it were, a light from the facts. We have only to adduce the subject before us in illustration of our principle. To have inferred the dependence of animal heat upon vital actions from the plain phenomena of life, from the analogies supplied by all other formations of the body, from the forces upon which it depends resisting all those causes which extinguish heat in inorganic matter, and from a like coincidence in respect to the operation of all chemical and physical forces upon the living organism, would constitute but little merit, compared with that complex system by which the chemical doctrine has found its way to popular favour. Just so it is with the more extended plan of subverting the most sublime department of nature, where everything is carried on by forces unknown in the inorganic world, and of substituting the laws of that world from which the former appears so remarkably exempt. The chemical hypothesis of respiration is only a part of the great plan of subversion; that of digestion is another; in a word, the spontaneous

ratures, from greater vascular excitement, &c. in the former, we explain the continued uniformity of heat on the ground, especially, that the redundancy is carried off by increased perspiration.

generation of animals sums up the whole. Who could acquire any eclat by simply maintaining that the gastric juice performs digestion in virtue of its vital forces, and thus, also, bestows the first act of vitalization; that the vessels then take up and complete the work of animalization; that the secretions depend upon the same forces, and so on? On the other hand, however, it requires a power not inferior to that of the Creator Himself, to bring about a single one of the foregoing results by the agency of the chemical or any other physical forces. The temptation, therefore, is very great to wander from the luminous path of nature. But, when this path shall have become universally abandoned, as at former eras, he will receive and deserve the greatest meed of praise, who, like Bacon, shall restore the Hippocratic method of induction. At present, however, there are some, who, as it seems to us, adhere with great tenacity to the path of nature; and until this is lost, the discovery of which we speak cannot, of course, happen; and when it comes, there will be but few to enjoy the honour. But, when the light shall have once more been diffused, and great principles again established, and new avenues to honour must be struck out, — and seeing, also, that our chemical hypotheses of life will be as unprofitable to our successors as alchymy to us, what new systems may be invented, we will scarcely presume to foretel. All that we can say of them is, they will make a great noise in the world, will triumph for awhile, and again have their exit. *Sic ibimus, ibitis, ibunt.* It is plain, therefore, should the rest of the world endure as long as it has hitherto, according to the geologists, there will be only now and then one whose name will be imperishable. Or if, like Paracelsus, and others not to be named, a straggler should gain a niche, he will be rather cast there by the eccentricity of his orbit, than by any of those movements that are at all subject to calculation. "*Sic itur ad astra.*"

There is no author more justly entitled to our respect and confidence in all that he has disclosed respecting the dependence of animal heat upon respiration, than Dr. Edwards; and none who has more ably defended the doctrine relating to respiration. He lays down four fundamental points:

1. "The oxygen which disappears in the respiration of atmospheric air is wholly absorbed. It is afterwards conveyed, wholly, or in part, into the current of circulation."

Here, at the onset of the process, chemistry is at fault. The induction of Dr. Edwards is mainly contradicted by Allen and Pepys, (1) and is only partially confirmed by Dupretz, (2) Dulong, Collard de Martigny, (3) Le Gallois, (4) Delaroche, (5) Coutanceau, Lavoisier, Nysten, Hassenfratz, La Grange, and others.

The quantity, also, of carbonic acid expired is exceedingly variable at different hours of the day, without any apparently modifying cause. (6) This shows its existence in the blood, independently of the experiments of Magnus, and others, who ascertained its presence in the ratio of five cubic inches to a pound of venous blood. (7) Here, then, the doctrine of a chem-

(1) Philos. Trans. 1808, 1809, 1829.

(2) Gmelin, Chimie, t. 4, An. de Chim. et Phys. t. 26.

(3) Journ. de Phys ol. 1830. (4) Annal. de Chim. et Physique, t. 4, p. 115.

(5) Journ. de Physique, t. 77.

(6) Prout, in Thomson's Annals of Philosophy. vol. ii. p. 328; and many others.

(7) Dr. Davy says, that "the results of his experiments appear to prove that blood does not absorb oxygen when fresh;" so clearly, indeed, that "he does not consider it incumbent on him to controvert farther the commonly received opinion." "I thought it necessary," he adds, "to reconsider the evidence on the subject, and to make farther experiments on many doubtful points; and the conclusions to which the inquiry has led me are, briefly, that the scarlet colour which venous blood acquires on exposure to the air, is not owing to any chemical change from exposure; and that it is very doubtful if the difference of colour between venous and arterial blood depend on the action of air." (*a*) Again, Dr. Christison, in his critical review of Dr. Davy's experiments, remarks, that the absorption of oxygen, in the human species, is sometimes greatly less than at others,—and this, too, in cases of disease where the general temperature of the body is much exalted. (*b*)

We take no farther part, at present, in these discrepancies of opinion amongst philosophers equally able, and ornaments of their age, otherwise than to show that the exposition of the subject belongs rather to the physiologist than to the chemist. True, we have no doubt of the decomposition of the air, and the absorption of oxygen, by the lungs, and that this particular fact may be advantageously shown by the chemist, although, as we have seen, we have, in some distinguished instances, his own authority that it cannot be shown. But we may now remark, that this does not prove that the decomposition and absorption are not vital processes. Nor is it any proof to the contrary, that atmospheric air is decomposed by the carbon of blood after its abstraction from the body, any more than other analogous changes which take place in dead matter. This subject we have examined fully in our essays on the Vital Powers, and Digestion; and in other places.

Since the foregoing was written, we have met with Dr. Davy's experiments upon the blood, recorded in the London Philosophical Transactions for 1838. They are entirely contradictory of his first series of 1829, as it respects the absorbtion of oxygen. The only farther comment we shall now make is that of adducing these conflicting results, as well as those obtained at different times by Allen and Pepys, and others, to sustain our facts and conclusions, as expressed in our essays on the *Vital Powers*, the *Humoral Pathology*, and *Digestion*, against the pretensions of organic chemistry. The subject before us is the most simple connected with that pursuit, whilst the

(*a*) Edin. Med. and Surg. Journ., 1830, p. 246. (*b*) Ibid. 1831, p. 94—103.

ical formation of carbonic acid in the lungs, by the combination of the carbon of the blood with the oxygen of the air, is invalidated, and the elimination of carbonic acid from the blood placed, in another aspect, upon its true ground of an excretory process. The foregoing fact is also farther shown by the experiments of Spallanzani and Edwards on kittens, which exhaled carbonic acid when deprived of oxygen. Even the skin possesses the function; for Spallanzani(1) found carbonic acid in the atmosphere of a torpid animal after its respiration had entirely ceased. Edwards subjected frogs to hydrogen gas; when he found that the extrication of carbonic acid was as great in 8 h. 30 m. as in 24 hours when atmospheric air was respired. Spallanzani obtained the same result upon snails. Dr. Edwards, therefore, draws the conclusion that,

"Carbonic acid is not formed at once in the act of respiration by the combination of the oxygen of the air with the carbon of the blood, but is entirely the product of exhalation." "Nothing," he adds, "appears to me to prevent our admitting the principle as general."

same distinguished observers arrive at different times at exactly opposite conclusions; nor is there any satisfactory reason assigned for the discrepancies. So, in respect to another simple constituent of the blood, free carbonic acid. Christison, Tiedemann, Gmelin, Müller, Mitscherlich, and others, deny that it can be extricated by the air pump, whilst Sir H. Davy, Brande, Home, Magnus, Bischoff, Bertuch, &c., found it evolved in considerable quantities. Dr. Davy denied its presence in the blood in 1818, but affirms its existence in a majority of instances, in 1838. This is undoubtedly true, since the experiments of Magnus, particularly, are affirmative, are too simple to be liable to deception, and cannot, therefore, be set aside by negative results.

In another able paper, (*a*) Dr. Davy "is disposed to think that carbonic acid acts a very important part in the economy of life," and inclines to the crude speculations of Dr. Stevens in supposing it to be "connected, when in excess, if not in the production of particular diseases, at least with their modification and progress," &c.

As to experiments, intended to illustrate the generation of animal heat, by combining atmospheric air with extra-vascular blood, or by other analogous processes, they are clearly too abstracted from physiological principles to justify any notice. It is remarkable that Dr. Davy affirmed, at one time, that there is little or no change of colour on agitating venous blood with atmospheric air, and that the volume of air "is not changed, nor its composition altered." He sometimes employed a pound of blood, and a flask of two or three gallons. (*b*) Yet, at a subsequent time, (*c*) this able philosopher concludes that oxygen is not only absorbed, but that the evolution of animal heat depends upon a condensation of the gas.

We have stated many absolute self-contradictions like the foregoing, in relation to organic chemistry. Which statement shall we receive as true? Are we not right in our conclusions as expressed in vol. i. p. 676?

(1) Memoria della Respiraz. &c., and Littera al Sig. Senebier, &c. t. v. pp. 7, 43.

(*a*) Ibid. April, 1839. (*b*) Ibid. 1830, p. 243. (*c*) Philos. Trans. 1838.

Gertanner,(1) Collard de Martigny,(2) Bichat,(3) Dr. Davy,(4) Coutanceau and Nysten,(5) Fodera, Dutrochet, Broughton, Allen and Pepys, and others, have shown the elimination of carbon to be an excretory process. Nor will carbon alone decompose atmospheric air or unite with oxygen at a lower temperature than 400° Fh.; nor does a chemical union take place between oxygen and carbonic acid gas.

The chemists having thus abandoned this old entrenchment, we shall take possession, and fortify it with the forces of life. We have shown, however, in another article, that there is still a fondness for the ancient error; and that a kindred doctrine has been elaborated under the disguise of *endosmose* and *exdosmose*.

Before going farther, we may say, that in having employed, as we shall continue to do, the established phraseology of chemical science, we have assigned many reasons in our first volume, as we shall others in our essay on Digestion, for believing that every product of the animal system, including the excrementitious, is differently combined in its elements from such as result from the agency of the chemical forces; that, what we find in our test glasses and crucibles has been really different before, or at the time of, its elimination from the body. Chemical changes may accrue in excrementitious substances immediately after their elaboration; and the ultimate combination may be uniform, where, as in carbonic acid, only two elements are concerned. This will hardly be doubted by such as believe this exact substance to be chemically formed in the blood, in the midst of so many other imputed constituents. The carbonic acid which is converted by vegetables into sap, wood, &c., becomes utterly different from itself or its elements, till it is again reclaimed by the chemist. Nevertheless, however great may be the opposition of the vital to the chemical forces, there is a certain parallel between them; and we think, in no other part of nature, do we see such an astonishing display of Unity of Design as in the constitution of these different, conflicting, yet analogous, powers. Each is concerned in compounding and decompounding the elements of the same matter, and the products of each consist of wholly dif-

(1) On the Laws of Irritability.

(2) Journ. Complément. t. xxxvi. p. 225, t. xxxvii. p. 168; and Journ. de Physiol. t. x.

(3) General Anat. vol. iii. p. 101. Also, Sur la Vie, &c. p. 389—393.

(4) Edin. Med. and Surg. Journ. vol. xxxiv. p. 243; and Philos. Trans. 1823.

(5) Revision des Doctrines Physiol., etc.

ferent combinations. Yet, when the organic forces cease their operation, their combinations being derived from the elements about which the forces of chemistry are equally concerned, these may instantly begin their destructive process, and establish changes according to their peculiar laws. The ground of the analogy is therefore obvious, whilst the powers are as distinct as man is from vegetables, or reason from instinct.

"2. The oxygen," continues Dr. Edwards, "is replaced by exhaled carbonic acid, which proceeds wholly, or in part, from that which is contained in the mass of blood.

"3. An animal breathing atmospheric air also absorbs azote. This is likewise conveyed wholly, or in part, into the mass of the blood.

"4. The absorbed azote is replaced by exhaled azote, which proceeds wholly, or in part, from the blood." (1)

Here, again, is another discrepancy at the threshold. Allen and Pepys saw no diminution of nitrogen. Davy, Pfaff, and Coutanceau found the volume diminished; whilst Nysten, Despratz, Berthellet, and Collard de Martigny, ascertained an increase.

It is also a fundamental principle with Dr. Edwards, that "respiration and animal heat stand related as cause and effect." And here, again, is another important disagreement at the opening of this subject. "Dr. Holland lays it down as an axiom, that animal heat is in the inverse ratio to the quantity of blood exposed to oxygen in the lungs; and opposes the opinion of Dr. Edwards, that it is in the direct ratio to the quantity of oxygen consumed." (2) But we shall see that this conclusion is contradicted by facts.

The experiments of Dr. Edwards contemplate a great variety of objects connected with animal heat; but they have all a direct bearing upon the question which relates to its essential cause. We shall use these experiments, especially, in forming our conclusions. We have made many ourselves, but shall only state a few of the most direct and important, which may be easily repeated.

It is remarked by Smellie, that the natural heat of the hibernating animals is generally above that of man; but during torpidity, it descends to 30°, or 40°, Fh. It is also remarkable, that, although the ordinary cold of winter keeps them in a torpid state, yet exposure to a much lower temperature than that

(1) Edwards, on the Influence of Physical Agents on Life, pp. 158, 239. Tr.
(2) Dr. Edwards on the Influence, &c. p. 471, *notes*.

to which they are commonly subjected, rouses them from it, and establishes the natural heat.(1)

De Sassy (2) has particularly investigated this subject. He exposed the hedgehog, the marmot, and dormouse, in their torpid state, and at a temperature of 39° Fh. to a northern air at 25°. In two hours, the temperature of the dormouse had risen to 97°; that of the other animals was a little more gradual in its return. Nearly the same result has happened by exposing these animals, in their torpid state, to a temperature of 12° Fh.

We cannot rest satisfied with simply admiring the benevolence of this provision and its final cause. Did the diminished temperature of the air continue to produce its original effect in the ratio of its decrease, it is obvious that the life of the animal would be soon extinguished. But, on the contrary, the increasing cold of the atmosphere restores the natural temperature of the animal.

On what part of the system is the primary action of the cold exerted in rousing the hibernating animal from its state of torpor? The organic forces are everywhere in an exceedingly prostrated state, and seem hardly in a condition to be stimulated into active operation by the cause which had laid them prostrate. We naturally turn, therefore, to the nervous system as the medium upon which the first exciting impression is produced, and through which it is transmitted to the brain. This organ of mystery then takes, as it were, the alarm, and sheds abroad its influence over those instruments which are everywhere destined for the evolution of heat. (See Vol. I. pp. 474—480, 568—572.) In an instant, too, its own temperature becomes exalted, and its other functions are, in consequence, started into action. To appreciate the influence which is now exercised by the brain over the languid actions of life, it is only necessary to advert to the well known control which the cerebral influence exerts over the organs of circulation and other important viscera,

(1) Philosophy of Natural History, ch. 12, p. 298. — See, also, Hunter's Observations on certain Parts of the Animal Economy, Exp. 16, p. 99; — Hunter's Principles of Surgery, ch. 8; — Reil's Archiv. für Physiol. t. 12, p. 293; — Meckel's Archiv. für Physiol. t. 3; — Gilbert's Annalen, Bd. 40, u. 41; — Prunelle, Recherches sur les phenom. et sur les causes du Sommeil hivernal; — An. du Mus. t. 18; — Philosoph. Trans. 1832; — Spallanzani, sulla Respirazione Mem. Terza, t. 5, s. 9, 10, 11, 12; — Pallas, Reeve, Mangili, &c.

(2) Recherches Exp. anatom. sur la Physique des Animaux mammif. hibernans, 1808.

when sudden impressions are made upon the brain.—(Vol. I. p. 157, et seq.)

The subsidence of heat in the hibernating animals is the most extraordinary of all the attendant phenomena; and from the nature of the cause, undoubtedly forms a principal element in the various changes that take place during the accession of torpor. It may be said, too, if the generation of animal heat be wholly a vital process, its reduction in the hibernating animals would probably be attended, at all its stages, with a corresponding failure of all other functions; and this is exactly the fact. When, therefore, in passing from the state of torpor to that of high animation, the respiration becomes increased, and the circulation accelerated, it seems to be probable that the principle which developes the latter function is first in operation; especially since the ratio in the increase of heat far surpasses that of the respiratory movements, which, at most, are but feeble till the heat is established.

It is stated by Dr. Edwards, that "the elimination of the temperature and the respiratory movements take place at the same time under the influence of cold, and that cold is the cause of both phenomena." (1) This is true in a general sense; but what are the facts which analysis developes? There is no principle in physiology which at all explains the operation of cold in diminishing the respiratory movements, till it has first reduced the temperature of the surface, and diminished the actions of the vascular system. Whilst the natural temperature remains, the respiratory movements will be in no respect diminished; and this is abundantly shown by the respiration of animals during the coldest weather in high northern latitudes. It is therefore evident, that the diminution of respiration in hibernating animals is subsequent to the diminution of heat, and that they stand, so far, in this relation as cause and effect. Accordingly, it is apparent, that the reduction of temperature depends essentially on other causes than diminished respiration, however this may ultimately contribute to its decline, as it does to that of all organic processes. The converse of this must be equally true; and when heat, therefore, is restored, the first step in the process is an increased action of the capillary system, by which an evolution of heat is immediately started; and then begins an increase of the respiratory movements. "We can always hasten respiration,"

(1) Op. Citat. p. 157.

says Bichat, "by making an animal suffer; but the accelerat'on of the pulse is always prior to that of respiration, which appears to be determined by it."(1)

Heat is peculiarly a vital stimulus. Its action is directly upon the powers of life. Its influence is exerted upon the actions of the sanguiferous capillaries, not upon the function of respiration, any more than it is upon the peristaltic movements of the intestines. Its abstraction from the surrounding medium of certain animals operates as a removal of a necessary stimulus to those actions; and hence the decline of temperature, according to the diminution of capillary action. This action being necessary to the specific functions of respiration, digestion, &c., these, also, decline in a corresponding manner; and, as these again, are necessary to capillary action, and sanguification, their subsidence hastens the decline of the capillary functions. A circle of influences is thus established, which is constantly taking its departure more or less, from the diminished temperature. But it also belongs to the constitution of hibernating animals, that an excessive degree of cold shall become a stimulus to the powers of life; and although we believe its primary influence is mainly exerted upon the organic forces through the nervous influence, it is still a philosophical induction that its action is also directly upon those forces. (See Vol. I. pp. 474—480, 568—572.)

In the hamster, whose pulse, in its natural state, amounts to 150 in a minute, it is, when the animal is torpid, reduced to 15. The natural pulsations of the heart in dormice can scarcely be counted; but as soon as they begin to pass into the torpid state, the pulse is gradually reduced to 30, 20, 16, and becomes, finally, imperceptible from feebleness.(2) Who shall assume that the diminished temperature is owing directly to diminished respiration, and not to that of the organs of circulation? Should we not equally argue, that the coincident suspension of perspiration, of the secretion of bile, of urine, &c., are alike owing to diminished respiration? So, also, *vice versa*, when the phenomena of respiration and circulation return, we may fairly infer that their restoration is especially dependent on a change in the calorific function. For here again, also, there is no conceivable principle which will explain the operation of cold, at a temperature far below that of the torpid animal, in reproducing the res-

(1) General Anatomy, vol. i. p. 418. (2) Smellie, Op. Citat. p. 298.

piratory movements, but that of a primary impression upon the *vires vitæ*, whose first result is an evolution of heat. For here, too, as in the case where a diminution of temperature must be antecedent to diminished respiration, it cannot be consistently supposed, that respiration precedes the evolution of heat. If the former affirmation may be made, the latter conclusion must equally follow.

It is true, that hibernation, and the torpor which is induced by cold in other animals, are so far different, that one is designed for preservation, and the other is destructive of life. It is a difference which grows out of constitutional peculiarites. But the final causes supply no light upon the subject. The efficient cause is cold in either instance; and the same principles, although in a modified way, are equally concerned in the phenomena. If what we have stated as to the philosophy of animal heat in its relation to hibernating animals be true, it is equally true of all other animals. The differences do not arise from different fundamental laws.

It is evident, however, that the peculiarity in the constitution of hibernating animals is only true of them in its most extended sense. There are many other animals which approximate them in their feeble powers of maintaining temperature; and others, again, which sustain an intermediate relation to the most perfect of the warm-blooded vertebrata. These gradations carry us so connectedly up from one extreme to the other, that we are almost insensible of differences as we pass along.

"The high temperature which seems to characterize the mammalia and birds does not belong to them exclusively, since examples of it are found among insects; and on the other hand, among the mammalia themselves there are species, which, at certain periods, present the principal phenomena of cold blooded vertebrata; and, lastly, a great number of non-hibernating mammalia and birds, in the early periods of their life, show, as far as the phenomena of heat is concerned, a strong resemblance to the cold blooded animals." (1)

Thence we infer *a fortiori*, that what is so remarkably conspicuous in the torpid hibernating animals is only the result of a law that prevails throughout the animal kingdom. And if we now look at the facts and excellent philosophy of Dr. Edwards, we shall find that what reason suggests in this case is confirmed by observation. By a series of experiments he ascertained, that the temperature of certain warm blooded animals varies according to the seasons; ranging, from winter to summer, from 105.4°

(1) Edwards, Op. Citat. p. 237.

to 110.7° Fh. "Hence," he says, "I judged that man, also, would experience variations of temperature under the influence of seasons, if not to the same extent, at least within appreciable limits." This induction appears to be well ascertained by observation. (1)

Now, to say that the foregoing phenomenon depends on a difference in respiration in summer and winter, or on the quantities of oxygen absorbed, would not only be an assumption, but opposed to experience, and to the philosophy which is founded upon physiological analogies. The reduction of temperature, in these instances, is owing, as in the hibernating animal, to the abstraction of the vital stimulus of heat from the surrounding atmosphere. The calorific organs are less excited than in summer, and, consequently there is a diminished production of heat in winter. The excess of heat goes off by perspiration in the former season. (2) Just so it is with some other secretions, especially the perspirable matter. Besides, on another occasion, as we shall see, it is equally assumed that a frosty air is best calculated to evolve animal heat. This is true, under particular conditions of the forces of life, as we have seen especially in regard to hibernating animals. But it only cöoperates with the former fact in showing that the process is wholly a vital one.

The principle, therefore, upon which all these phenomena depend, appertains to the vital constitution of animals, is fundamental in their economy, and is everywhere of a common nature. For reasons and facts already stated, and others which are yet to appear, there can be no other intelligible connection of this principle with the function of respiration, than what subsists between respiration and all other functions.

We have spoken of the probable controlling influence of the brain over the generation of animal heat, under particular circumstances. It may be more difficult to arrive at the extent of this influence in the natural state of the animal system. Like other secreted products, animal heat is, doubtless, primarily dependent upon the organic powers. These are variously influenced by varying conditions of the cerebral and ganglionic systems, and, of course, their actions, and the products of the secerning system, over which they preside, are affected in a corresponding manner, whilst they are also modified by the direct

(1) Edwards, Op. Citat. p. 257. (2) See note forward.

action of foreign agents. In the perfectly natural state, there is reason to suppose that the brain may have but little connection with the phenomena, but may become powerfully instrumental in modifying the powers, and actions, and products of life, when unusual conditions exist, or when unusual impressions are transmitted to the brain. We shall see that analogy, as supplied by the vegetable kingdom, affords a presumptive evidence, that the brain may have no active participation in the elaboration of heat, in the natural condition of the body, whilst this induction is strengthened by what is known of other secreted products in both of the animated kingdoms. Still, in respect to the animal kingdom, the mere existence of the cerebral system, its remarkable properties and susceptibilities, and its intimate connection with all parts of the organization, is, *prima facie*, conclusive that it determines important influences upon the *vires vitæ*, and that its presence is indispensable to the integrity of every function. This has been experimentally ascertained in relation to many; and that unusual, or sudden impressions that are not unnatural, as the operation of the passions, for instance, may be extensively and profoundly propagated from the brain to other organs. It has been fully demonstrated that the natural condition of the secretions depends upon the integrity of the nervous connection betwixt the secerning organs and the brain; whilst it has been equally shown that the organic functions, and all vascular action, may be immediately and powerfully influenced by impressions made upon the brain. (See Vol. I. p. 157—179.)

Assuming, then, that animal heat is also a secreted product, it would philosophically come under the common law; and should it appear from experiment, that animal heat depends more or less upon the presence of the brain, and may be influenced through it, the physiological analogy betwixt heat and other secreted matters will be quite apparent; whilst it will explain the remarkable effect of a low atmospheric temperature in developing heat in the torpid hibernating animal, and thus conduct us to the philosophy of the operation of other causes in modifying animal temperature.

Now we have never seen the experiments of Sir B. Brodie, (1)

(1) Philosophical Trans. 1811, 1812; and in Edin. Jour. vol. viii. p. 447. Also, Brande's Manual of Chemistry; Thomson's Chemistry; and Earle, in Med. Chir. Trans, vol. vii. p. 173; who consider Brodie's experiments subversive of the chemical doctrine.

Chaussat, (1) and Emmert, (2) satisfactorily contradicted. We need scarcely say, that they ascertained that when the action of the heart is maintained by artificial respiration in decapitated animals, the heat of the body cooled down rapidly to an equilibrium with the surrounding air. It is true, Le Gallois, (3) Marshal Hall, (4) and Philip, (5) have arrived at results more or less different; but in none of the instances do we think that the experiments of the former physiologists are affected. It has been also suggested to us by an eminent philosopher to whom this essay has been submitted, that the experiments by Dr. Hale conflict with Sir B. Brodie's, and we shall therefore assign our reasons for thinking otherwise, in a note below. (6) We may say, also, that

(1) Mém. Sur L'Influence du Sys. Nerv. sur Chaleur Animale.

This author shows, that injuries of the nervous system produce corresponding affections of the temperature and the functions of secretion.

(2) Meckel's Archiv. vol. i. p. 184.

(3) Le Gallois, Expériences sur la Principe de la Vie; whose observations upon this subject are very indefinite, and liable to obvious exceptions.

(4) Lon. Med. and Phys. Jour. vol. xxxii. 1834. See Brodie, Ibid. p. 295.

(5) Experimental Inquiry into the Laws of the Vital Functions, p. 212,—220.

If these experiments do not sustain the conclusions of Brodie, they neutralize any proof they may supply against them. We shall see, however, that it is a doctrine with Philip, "that the maintenance of animal temperature is a function of the nervous system." This abstracts the question entirely from chemistry; as we believe Dr. Philip, and all others, must abandon the supposed "identity of the nervous influence and galvanism," and allow it to be purely vital. But, it is manifest that "the maintenance of animal heat" is not "a function of the nervous system," since, as we shall see, there is an absolute coincidence betwixt the phenomena of animal and vegetable heat.

(6) Sir B. Brodie, in replying to Dr. Hale, states that "he had reason to believe that he had conducted his experiments with all the caution and attention to minute circumstances, which every physiologist knows to be absolutely necessary in such investigations, to enable us to arrive at any accurate conclusions." (*a*) It is, therefore, *prima facie*, highly probable, that Sir Benjamin arrived at accurate results, where nothing but care, skill, and integrity were necessary. We take it that no one will deny either of these qualifications to this admirable man. Upon these principles, therefore, his experiments should stand, until they are unequivocally shown, by different observers who may be equally entitled to confidence, to have been imperfectly conducted. But Brodie's experiments have been confirmed by other physiologists in Europe. It appears, also, that Dr. Hale deviated in his experiments from those of Brodie, to which, as the latter affirms, "they bear only a certain degree of analogy." Dr. Hale, in reply, (*b*) "doubts not that Sir B.'s experiments were perfectly correct in all their details. Still it does not follow that similar results will be obtained from somewhat similar experiments, *where some circumstances are different.*" It is this "difference in circumstances" which makes the difference in the results obtained by these distinguished observers, and which farther inclines us to confide in Sir Benja-

(*a*) London Med. and Phys. Jour. 1814. (*b*) New England Med. and Surg. Jour. 1816, p. 25.

Müller thinks, that "Brodie's experiments are, for the main point, convincing;" (1) (p. 23, *note*,) whilst it is well said by Magendie, "Remember, — once again let me warn you of this — that one well observed fact cannot overturn another of similar character. If such appear to be the case, rest assured that our intelligence is not sufficiently enlightened to comprehend the relation." (2)

In connection with the foregoing result is another not less important, viz: that in decapitated animals the temperature went down more rapidly when artificial respiration was maintained, than when it was not; thus showing, also, that the tendency of the air when admitted to the lungs is to depress the temperature of the body. This effect, however, is always more than compensated in the natural state of the subject by the part which atmospheric air performs in the process of sanguification, &c.

min's accuracy. If there be only a division of the spinal marrow, and the brain be left undisturbed, as in Dr. Hale's experiments, that organ will continue to exert a powerful influence upon the actions of life, whilst the circulation is maintained. (*a*) Dr. Hale admits, too, that there was, in another respect, "a very considerable reason for the difference in our results." This deviation from Sir B.'s experiments consisted in the use of rabbits by one, and of cats and dogs by the other. To show what this difference may possibly be, Müller states that "the influence of respiration on the heart's action, in dogs, seems to be greater than that of the nervous system," whilst in frogs "the brain and spinal marrow are much more necessary to the maintainance of the heart's action than respiration." (*b*) (See our vol i. pp. 650, 697, 698, &c.)

Dr. Hale also observes, "it may, perhaps, be an *inaccuracy of language* to call his experiments a *repetition*" of Brodie's. The question then arises, will a "repetition" of Brodie's experiments upon dogs and cats afford results contradictory to those of Brodie upon rabbits? There must certainly be a general coincidence, if the experiments be in all respects alike conducted; and since Sir B.'s were attended by very definite results, we may expect that the coincidence will be found to happen. Whatever fundamental law may exist in one species of the warm blooded animals undoubtedly exists in all others. There is probably no specific law peculiar to certain species, although the laws be subject to peculiar influences and modifications in different species. It is such modifications, natural and accidental, which give rise to many opposite local results which might lead to the belief that they are determined by opposite laws. The dog digests better when he is lying down and asleep; but man, when he is erect and awake. Magendie starved an ass with boiled rice, whilst he fattened a cock upon the same. There is an instance of summer sleep in the hedgehog, or tanrec, of Madagascar, similar to hibernation; and these extremes are variously connected amongst other animals, in whom a similar, or somewhat similar, state is induced at various temperatures and seasons. Vital actions are more feeble in man when asleep, than when awake; but it is just the reverse with some animals, as in the case of the dog now mentioned, — and this by the way, shows that respiration is only remotely connected with the phenomenon under consideration.

The same experiments upon the nervous system are well known to affect the func-

(1) Elements of Physiology, vol. i. p. 87. (2) Lectures on the Blood, p. 92. tr.

(*a*) See Philip's Exp. Inq. into the Laws of Life, and the analogous experiments of others.
(*b*) Elements of Physiology, vol. i. p. 189.

It appears, likewise, from Brodie's experiments, that when the sensibility of an animal is impaired by narcotics, and the lungs artificially inflated, the power of generating heat is as greatly destroyed as by decapitation; whilst it is again restored in an exact ratio to the return of sensibility. This is shown by a natural process. "From the observations of Dr. Nasse, and Mr. Dundas, on pathological cases, it appears that the diminution of temperature is more in proportion to the loss of sensibility than of the powers of motion." (1)

Analogous to the foregoing experiments, and of the same import, is that by Sir E. Home, who divided the nerves of an antler, when its temperature fell immediately several degrees; but, in a few days, became exalted beyond its natural state. Similar experiments, with like results, have been made by Elliot; and every body knows that the temperature falls when the par vagum is divided. (See Vol. I. pp. 157 — 179, 474 — 480, 568 — 575.)

In all these instances a pernicious influence is propagated upon the forces of life appertaining to any particular part whose nerves are divided. The first result is a decline of temperature; but inflammation may ultimately spring up, (2) and then follows a

tions differently in different animals, and even in the same animal, — as in crushing, irritating, or slicing the brain; whilst there is an analogy amongst the results of the whole which shows that they depend upon common laws, modified in different species. These differences make up a part of the great series of ascending analogies in the general plan of Creation. There is nothing exactly alike betwixt two distinct species of animals, however generically allied, either in respect to form, organization, functions, habits, instinct, &c.; yet they may be all essentially the same. So with their great vital laws; though we think these vary less than organization. A great modification of the latter may be necessary to a slighter one of the former.

Thus, modifying influences arising from a difference in the constitution of different animals may sensibly affect the results of our experiments; and without a careful reference to all the circumstances, important facts may escape observation.

It is said by a distinguished Review, that Dr. Macartney "quotes the experiments of Brodie, as if their fallacy had never been exposed, to show that animal heat does not depend upon respiration." We think, however, that the fallacies are rather on the side of Brodie's opponents; though, as we have said in our text, too much is allowed to the brain by that eminent observer. We think its relation to the production of heat of the same nature as in all other organic processes. But our able reviewer admits that "there can be no doubt that its evolution (animal heat) is greatly influenced by the nervous system."(a) This is exactly our own doctrine, and we contend for nothing more. We doubt not, too, that when physiology shall have been rescued from the trammels of chemistry, this admitted influence of the nervous system will place the generation of animal heat amongst other processes of the vital powers.

(1) British and Foreign Med. Review, vol. iv. p. 425. (2) *See Essay on Inflam.*

(a) British and Foreign Med. Review, July, 1839. p. 197. — *See our Vol. I. p.* 714.

coincident exaltation of heat beyond the natural standard. We shall also see, that when the nerves are differently affected, as in certain diseases of this system, there may be a direct increase of temperature, without inflammation. The generation of heat, therefore, is variously affected, like other vital products, according to the nature of the injury the nerves may sustain. And although there be no reason to conclude that the evolution of animal heat is more dependent upon the nervous system than other secretions, it is perfectly consistent with this view to suppose that it may be more influenced through that system. We think that we shall have shown ample facts to justify this conclusion; and, therefore, to render it even less probable that animal heat is a chemical product, than other secretions. But to us it is perfectly unintelligible, that "animal heat should be influenced by the state of the nervous system; but never, except through the instrumentality of chemical changes."(1) This is a candid admission of physiological facts with an uncompromising adherence to the chemical doctrine.

We may now say, that the foregoing considerations, with many others which we shall yet state, go to confirm the accuracy of Brodie's experiments.

It is also worthy of remark, that the temperature of many new-born animals descends very rapidly to that of the surrounding atmosphere; but, "the rapid progress which they make in acquiring the power of producing heat is wonderful."(2) Now the brain, in new-born animals, is but imperfectly organized, and undergoes great and rapid changes in that respect as the animal advances in life. In the human subject, the change is slowly progressive, and with it progresses, *pari passu*, its influence in evolving heat. In early life, Bichat says, the brain is "in expectation of action." But whether the brain have, or have not, any participation in the foregoing phenomenon, it is clear that the difference in the power of evolving heat can have but little connection with respiration. This function is as perfectly performed at the earliest as at more advanced periods of life; and the only intelligible explanation of the "wonderful progress which young animals make in acquiring the power of producing heat" consists in the rapid development of the organic forces. Such, then,

(1) Dr. Elliotson's Human Physiology, Part 1. p. 246. So, Philip, and others.
(2) Dr. Edwards, Op. Cit. p. 74.

being true of young animals, and the principle being fundamental, it necessarily applies at all other ages. (See *App.* on *Anal.*)

The modifying influence, therefore, which the nervous system exercises over the generation of animal heat being established not only by experiments, but more especially by facts relating to morbid states of that system, to which we shall soon advert, and by all that is philosophical in physiological science; and when we consider, also, how rapidly the nervous influence is determined upon the vascular system, and upon the organic viscera, we have an intelligible explanation of the operation of a very low degree of cold in recalling into action those vessels upon which depends the exaltation of temperature in the torpid hibernating animal. That the intensity of the cold operates, also, as a stimulus in a direct manner upon the *organic* forces, as in other instances of foreign agents, is undoubtedly true.(1) The law, also, being universal, explains the influences of other causes, in health and disease, in modifying animal temperature, and only regards the agency of respiration, like that of digestion, &c., as being instrumental in perfecting the blood, and thus adapting it to the uses of the various organs which are concerned in the elaboration of heat and other products.

SECTION II.

"THE effect of gradual cold," says Dr. Williams, "seems to be to lower the function of the nervous system in such a way that

(1) Our inductions, therefore, must be more or less founded upon the combined results of that organization of which a nervous system forms an element, and that which appears to be destitute of the nervous apparatus, as in the vegetable kingdom. Where the former exists, it undoubtedly performs an important, but subsidiary part; the organic forces being the essential principles, in all organization, upon which the diversified phenomena of life depend. (Vol. I. pp. 157, 474, 569.)

The nerves are especially a medium of sympathy; and although it may seem contrary to philosophy that this remarkable property should appertain to other organized structures, there is yet reason to believe that in the vegetable kingdom, where the property is manifested, it belongs to a system which is more analogous to the organic than the nervous tissue. Analogies must have a limit, where they are concerned in supplying inductions as to the vital properties from the apparent structure of organized matter. The phenomena are better guides than the physical appearances. The muscular fibre, for instance, is clearly not necessary to sensible motion; at least, so far as we may depend upon sight. — See Essay on *Venous Congestion*, Sec. 10.

the lungs and other parts do not feel the want of arterial blood; but in their degraded state to be content with merely venous blood."(1) True; and the various actions of life are greatly depressed in consequence. The secretions are all suspended, in common with animal heat. The proximate cause is the same in relation to each; but, will it be contended that the suspension of the former has been mainly owing to deficient respiration?

Diseases of the brain, and of other organs, supply many important facts which illustrate our inquiry, and which are, at least, opposed to the hypothesis that concerns respiration. Thus, in phrenitis, one arm, or one side of the body, is colder than the other.

"That the maintenance of animal temperature," says Dr. Philip, "is a function of the nervous system, properly so called, appears from a variety of facts generally known; the temperature either of a part or of the whole body being lessened by any cause that impairs the action of particular nerves in the former instance, or of the whole nervous system in the latter."(2)

This, certainly, does not appear to conflict with Sir B. Brodie's inductions, as has been more or less affirmed of Philip's analogous experiments. Besides, if "the maintenance of animal temperature is a function of the *nervous* system," how is it, in any respect, a chemical phenomenon? (See Vol. I. p. 714.)

The foregoing general annunciation, coming from those who believe in the chemical doctrine, might be sufficient in a general treatise; but a critical analysis requires some specifications to give it a proper exactness and authority. And we may now, once for all, make this apology for any apparent minuteness of detail, whether in relation to facts or to comment, that might be out of place in less exact disquisitions.

"That the temperature of a paralyzed part is generally below the normal standard is now universally admitted."(3) That this is owing to impaired vitality is, also, shown by the frequent failure of nutrition in the paralyzed part, as well as other coincident phenomena. In a case related by Mr. Earle, he found the temperature at 70° Fh. in the hand of a paralyzed arm, whilst that of the opposite hand was 92°. He could also effect a temporary restoration of temperature by electricity, and by blisters. "The circulation of the blood did not appear to have suffered, the pulse at the wrist being synchronous, and equally strong with that of

(1) On Diseases of the Chest, Lec. 3.
(2) On Acute and Chronic Diseases, p. 48.
(3) Brit. and Foreign Med. Rev. vol. iv. p. 425.

the other limb."(1) In an injury of the sympathetic nerve, Chaussat saw the temperature fall from 104. 88° to 78. 8°, Fh. in ten hours.(2)

On the other hand, there is a remarkable exaltation of temperature in a part at the invasion of *tic douloureux*. So, when the nerves are mechanically injured. There was a patient at St. George's hospital, whose temperature rose 11° Fh. in consequence of an injury of the spinal column; and this took place when the respirations did not exceed *five or six in a minute.*(3) It is stated by Dr. Macartney and other observers, that when the principal nerve of an extremity is divided, the temperature of the limb is immediately exalted several degrees.(4) The philosophy of this is well expounded by an able advocate of the chemical doctrine. "We should be disposed," he says, "to regard it as due to the temporary excitement of the molecular changes by the irritation produced by the section of the nerve, and propagated to its extremities."(5) Now apply this language to the exaltation of temperature in any inanimate substance, however produced, and we may appreciate the merits of the chemical solution in the former instance.

"In some subjects of insanity," says Dr. Cox, of Fish-Ponds, "who were under strong coercion in the horizontal position, with the head much elevated, whose face was red and the vessels turgid, the difference of heat was very obvious, varying 10, 12, and even 15 degrees.(6)

In apoplexy, the temperature has been known to rise, after death, a number of degrees above the natural standard; and its persistence has been found so uniform in apoplexy, that Dr. Cheyne regards it as a diagnostic symptom.(7) The temperature of a lawyer, dead of apoplexy, was so high at 24 hours after death, that Portal delayed an examination of the body. The same phenomenon is observed after death from other diseases,—especially when the nervous system has been unusually concerned in the morbid process.(8)

"In opening dead bodies at the Hotel Dieu," says Bichat, "I have observed that the time in which they lost their animal heat was very variable; that a

(1) Med. Chir. Trans. vol. vii. p. 173. See, also, Yelloly in Med. Chir. Trans. vol. iii.; and Meckel's Archiv. t. 3. p. 419.

(2) Op. Cit. (3) London Med. Gaz. June 1836. (4) On Inflammation, p. 13.

(5) British and Foreign Med. Rev. July, 1839, p. 198.

(6) Beddoes on Fever and Inflammation, p. 147.

(7) Cases of Apoplexy, p. vii. See, also, Bichat's explanation of the phenomenon in General Anat. vol. ii. p. 51.

(8) Portal sur l'Apoplexie, pp. 4, 239. Morgagni Ep. 5. s. 6, etc.

body continues warm a greater or less time, especially among those who have died suddenly of an acute affection, in the paroxysm of an ataxic fever, for example, or by a fall; for those who die of a chronic disease, lose almost immediately their caloric. The difference in the first is often three, four, or even six hours. This phenomenon arises from the fact, that whenever death is sudden, it interrupts only the great functions; (1) the tonic action of the parts continues for a greater or less time after. Now this action disengages a little caloric from the blood that is in the general system." "When the disengagement of caloric has ceased in the body, that which remains in it becomes in equilibrium with that of the external air, according to the general laws of this equilibrium. Now these laws being uniform, their effect would be the same in every case." (2)

Again, sometimes the temperature in apoplexy is greatly depressed before death takes place, — and this, too, whilst the circulation is such as to admit of bloodletting. Two cases of violent apoplexy, ("violento paroxysmo,") are recorded in the Ephemerides Germanii, (3) in which the blood, as it flowed from the veins, was actually cold. Morgagni (4) mentions an instance of another affection in which the blood flowed "in an icy cold stream" from the arm. Thackrah (5) saw a similar phenomenon. So, also, De Haën. (6) We need scarcely say, also, that when respiration is extremely laboured and slow in apoplexy, the natural temperature is often either undiminished, or considerably exalted. Our familiarity with the fact, however, only increases its importance, and shows, by the frequency of the coincidence, that respiration can be only remotely concerned with the generation of heat.

Here is another variety in apoplectic affections:

"While a gentleman," says Mr. Hunter, "who was seized with an apoplectic fit, lay insensible in bed, covered with blankets, I found that his whole body would, *in an instant*, become extremely cold in every part, continuing so for some time; and as suddenly would become extremely hot. While this was going on alternately, there was no sensible alteration in his pulse for several hours." (7)

Here is another case, from the same observer, not less fatal to the theory of respiration:

"A man fell from his horse, and pitched on his head, and produced all the symptoms of a violent injury. There was concussion, and perhaps extravasation of blood. The pulse was at first 120, but came to 100, and sometimes

(1) This will depend upon the nature of the causes. When death is suddenly produced by lightning, the vital powers are so completely extinguished, at once, that the elaboration of heat, and all other vital processes, are instantly arrested.

(2) Bichat's Gen. Anat. vol. ii. p. 51. (3) An. 1685, p. 271.

(4) Ep. Art. 26. (5) On the Blood, p. 87. (6) Rat. Med. t. 3, p. 36.

(7) Exp. on Animals with respect to the Power of producing Heat.

to 90, and was strong, full, and rather hard. He was *very hot* in the skin; but *breathed remarkably* slow, only half the common frequency." (1)

The following case, by the same author, seems also to have been intended for our special purpose:

"February, 1781, a boy, about three years old, appeared not quite so well as common, being attacked with a kind of shortness of breathing in the night. It had become excessively oppressive about five o'clock on Sunday morning, so difficult that he appeared dying for want of breath. The common rate of breathing in such a boy is about thirty inspirations in a minute. At 10 o'clock, he was drawing his breath with a jerk, — about *two and a half inspirations*, or even less, in a minute. Pulse sixty, *faint*, slow. On tying up the arm, the vein did not appear to rise in the least, so that the *blood did not go its round.* Body *purplish*, especially the lips. He had a *fine warmth* on the skin *all over the body*, although in a room *without a fire*, — not covered with more clothes than common in the month of February, with snow falling at noon." (2)

This, and the preceding case, appear to differ in some physiological details. In the former, the disposition of the capillaries to generate heat seems to have been a good deal determined by the cerebral influence; in the latter, the alteration of the vital forces was probably owing to other causes. Like other cases, therefore, which we have recited, they serve, by their variety, to illustrate the vital nature of the principles which are mainly concerned in the production of animal heat. But, standing alone, they must either subvert the hypothesis which concerns respiration, or we must have a chemical theory for the natural state of the body, and a vital one for its morbid conditions. This would be clearly absurd; at least, if there be any such thing as philosophy, or any consistency in the powers and functions of life. These examples show us, also, how very probable it is, that all our chemical hypotheses in relation to life are the mere offspring of habit, or imitation, or of narrow observation. It is certainly hard to give up the fruit of great toil and research; but it is harder for others to endure it, who prefer to be instructed by the voice of nature, rather than by artificial results. (3)

We shall present other examples to the foregoing effect, as supplied by morbid conditions of the system; since these, more than the experiments which are to come, conduct us to the true philosophy of animal heat.

(1) Hunter's Lectures on Surgery, p. 74. (2) Mr. Hunter, Ibid.

(3) We commend, also, to our minute philosophers Mr. Hunter's experiment upon the carp. It was partly intended to illustrate a vision of our author, by which, as he says, "like other schemers, he thought he should make his fortune." But our author had not only the good sense to abandon it, but the magnanimity to hold it up as a weakness of the human understanding.

Every physician is familiar with the variations of temperature in disease; which, indeed, engage his attention in almost every case. It is often exalted when respiration is slow, and again depressed when breathing is hurried; and it is one of the most common phenomena to find it different, by many degrees, in different parts of the body, and under every variety of respiration and circulation. It will, therefore, be our purpose only to mention a few of the more unusual instances.

Dr. Philip has known the temperature of the skin at 74° Fh. in the cold stage of an intermittent, whilst in the hot stage, it rose to 105°. Craigie found it at 107°, and 109°. Here the respiration and circulation are often most accelerated during the cold stage. This, with the vast difference in temperature, refers the depression of heat to other causes than the mere constriction of the capillaries in the cold stage. Here, too, as in all analogous cases, we have a coincident diminution of all other secretions. Piorry has seen the temperature in six cases of typhoid fever varying from 108° to 117°; and in one of these, the blood was at 113°, Fh. In phthisis, he has known it at 114°, and in a case of pneumonia, the blood was 113°.(1) Prevost found the temperature of the body at 110° in tetanus. (2) Granville says it sometimes rises in the uterine system to 120° Fh. and that it depends on the degree of action in the organ. (3) In hydrophobia, where respiration is probably always accelerated, Currie found that "there was no increase of animal heat in any one of five cases." (4)

How is the natural temperature maintained in consumption, where respiration is sometimes so greatly impaired as not to be compensated by any acceleration of its movements? (5) Or, why is it, in this disease, without any previous reduction of temperature, it habitually rises in the afternoon, — and this, too, in numerous instances, without any increase of respiration? Why do the palms of the hands "burn," when the rest of the surface is cool? Will chemistry explain?

Why is it, that when the general temperature of the body is at some 85° Fh. it may exist at the *scrobiculis cordis* at 106°,

(1) Traité de Diagnostic et de Séméiologie, t. 3.

(2) Dr. Edwards, Op. Cit. p. 490. (3) Philos. Trans. 1825, p. 262 — 4.

(4) Med. Reports, 1, p. 179.

(5) See Grœschen, Pulmonum cum Cute Commerium, 1790; and Brandis, Physiologie, 1808, p. 316, etc.

and upwards?(1) Mr. Malcolmson states, that in the Asiatic Cholera, "the skin is sometimes *colder* during life than after death, and a partial rise of temperature over the trunk is *frequently* a fatal symptom." We have witnessed the same phenomena. Mr. M. also observes, that beriberi supplies analogous instances; and that when the temperature was extremely reduced, "it was not different when the limbs were closely wrapped in woollen, or when the thermometer was held between the soles of the feet or hands, and free evaporation carefully prevented."(2) Is it not obvious, in these instances, that the power of generating heat was lost in consequence of modified vascular action; and if so, then the generation of heat depends upon vascular action, and is, of course, a vital product. This, too, is most emphatically shown, in the instances here and elsewhere stated, by the "partial rise of temperature over the trunk" just antecedently to death. It is analogous to those cases in which profuse perspiration breaks out in syncope, or as patients are in the act of expiring. It grows out of a powerful impression determined upon the *vires vitæ*, by which a sudden change of action is induced in the elaborating vessels.

Why is the temperature often exalted in congestions of the lungs, "where life is endangered by diminished communication with the air;" and why, in such a case will "the abstraction of blood diminish the power of producing heat,"(3) although, by this means we extend the communication of the lungs with the air? Or, again, in congestions of other organs, when the respiration is natural, the circulation in the lungs unobstructed, but the animal heat greatly reduced, why does it happen that the abstraction of blood will at once exalt the temperature, without affecting the respiration, or even increasing the force or frequency of the general circulation?

In the latter cases, the rationale appears to be, as we have endeavoured to explain in our Essay on Bloodletting, that a direct change is exerted by the abstraction of blood upon the instruments of all vital actions, by which the calorific, as well as other functions, are improved or restored. It is here, animating these minute vessels, that we shall find the principles residing, by which we are to account for all the remarkable phenomena of

(1) See our Letters on the Cholera Asphyxia, and other authors upon this disease.
(2) Prac. Essay on the Hist. and Treat. of Beriberi, p. 85. 1835.
(3) Dr. Edwards, Op. Cit. p. 275.

animal heat. As the operation of these forces is modified, whether by natural or artificial causes, so will be the phenomena which depend upon them. This is universally true of all the manifestations of the organic forces, whether they consist of vital phenomena, or of material products. The function of respiration is just as much concerned with one as with the other, and probably no more. It aids, like the chylopoietic viscera, in perfecting the great material from which bile, urine, the gastric juice, &c., are elaborated by the vital forces and their instruments. And just so is respiration concerned in the production of animal heat.

Again, "sympathy," says Bichat, "as we know, has the greatest influence upon heat. According as this or that part is affected, there is disengaged in others more or less of this fluid. How does all this happen? In this way; the affected organ acts sympathetically on the tonic forces of the part; these being raised, more caloric than usual is disengaged. It is precisely the same as in sympathetic secretions or exhalations. Whether the vital forces are raised by a stimulus directly applied, or by the sympathetic influence they receive, the effect that results from it is exactly the same."

And again, the same accurate philosopher: "Each system has its own degree of heat."(1) This fact was not so well known in Bichat's time as now. But it was his induction from general principles. We shall only advert to the example of the dog's nose, which is familiar to all. Hunter,(2) however, rendered the fact sufficiently obvious;—Davy (3) and others have confirmed it. Now, how exactly all this corresponds with what is known of the vital endowments of particular organs. Where they are most strongly pronounced, there the temperature is apt to be highest, there the phenomena of organic life predominate, and there it is that morbific causes make their most frequent and deep impressions, and develope the most exalted temperature.—See Essay on *Venous Congestion*, Secs. 8 and 9.

Finally, we come to what we consider an *experimentum crucis*, supplied by an able philosopher, and by one of the most able defenders of the chemical doctrine of animal heat. He states that

(1) General Anatomy, vol. ii. p. 47.

(2) Animal Economy, ut. cit. And again, "from experiments on mice and upon the dog," says Mr. Hunter, "it plainly appears that every part of an animal is not of the same degree of heat; and hence we may reasonably infer that the heat of the *vital parts* of a man is greater than either the mouth, rectum, or the urethra." (*a*) We have certainly no analogies in the common laws of caloric, or in chemical agencies, that will in the least explain the foregoing, and a thousand other analogous facts.

(3) Philos. Trans. 1814, Part 2, p. 597.

(*a*) Hunter's Works, vol. iv. p. 146.

great differences arise as to oxygen, during the respiration of atmospheric air:

"The real causes are chiefly certain inherent differences in the state of the venous blood, which are indicated, indeed, by other physiological facts, but by none *so unequivocally* as by this variety in the power of altering the oxygen of atmospheric air. The first cause is a difference in the degree of venosity or venalization of the blood in passing through the capillaries." The second and last "cause of diversity in the action of venous blood on atmospheric air is a difference in the proportion of colouring matter contained in the blood."

Now, if the chemical doctrine have any foundation, its advocates should show that there is a greater, or, at least, as great a consumption of oxygen in those states of the system which are attended by an exaltation of temperature, as in the natural condition of the body. On the contrary, however, they show just the reverse of this. Thus, the high authority whom we have just quoted:

"The inferior action of the blood on the oxygen of the air in its passage to the arterial state simply indicates, that it is less removed from a state of arterialization, that is, partakes less than usual of the characters of venous blood. Accordingly, the *least* alteration of oxygen *invariably* occurs in those *febrile* diseases where the *circulation is much excited, and the respiration at the same time free.* These conditions exist most especially in *acute rheumatism;* and it was, therefore, in cases of this disease that the four instances of *slight* action (on the air) formerly mentioned have occurred. On all these four occasions the blood was evidently more florid than usual, and in the instance where the *loss of oxygen was only* 0.57 of a cubic inch, the stream from the vein was so bright, that the gentleman who opened it had at first some suspicion that he had opened the artery." (1)

Here, also, we have from a distinguished chemist, a philosophical resort to the modified condition of the system in disease, for an interpretation of the wonderful peculiarity of living organized matter in manifesting the power of generating heat.

SECTION III.

We set about this subject with the intention of showing by the experiments of Dr. Edwards, that the elaboration of animal heat is a vital function; but we have been unavoidably drawn away from the artificial to the processes of Nature, which is the only true school of instruction. We have thus already seen that the

(1) Dr. Christison, in Edin. Med. and Surg. Journ. 1831, pp. 101, 102.

hypothesis of Dr. Edwards, and all others which immediately concern the functions of respiration, are surrounded by too many exceptions to come within the pale of nature. These exceptions meet us everywhere in the habitual state of the animal, and in the history of disease they become almost as multiplied as the individual cases. Here it is, that we may most successfully contemplate the law and its operations, in the various modifications which it sustains from the influence of remote causes, and those within the body. Amongst the latter, are the derangements to which the lungs are liable, both in their general and organic functions. But far more frequently, and more profoundly, is animal temperature directly exalted, or diminished, by affections of the stomach and the nervous system. We need scarcely say it would be absurd to have one theory to explain the phenomena of heat in health, and another in disease. It would be a violation of all philosophy, as well as a reckless disregard of all facts. (1) According to the common designs of nature, there cannot be one law for the generation of heat in the healthy state of the body, and another which determines the exalted heat of fever. Whilst the various functions proceed in their natural manner, the evolution of heat, like the other products, remains without any radical alteration. But when the latter are disturbed in their natural character, the former is liable to corresponding variations, which can only be explained on the principle that the power of generating heat is as much an attribute of vitality, as any that may be concerned in the process of disease, and that their various modifications are constantly determined by analogous causes. It is a broad, fundamental principle, that "the general phenomena of the disengagement of heat remain always the same in animals with lungs, in those without them, and in plants, all of which have an independent temperature. (2)

Dr. Edwards produces the following fact to illustrate the connection which he supposes to exist betwixt heat and respiration. Frogs and some other batrachian animals will live a long time under water, whilst the temperature ranges between 32° and 50° Fh. The nearer the minimum temperature, the longer will the animals live; and if the temperature be raised above the maxi-

(1) See Strom, Theoria Inflam. Doct. de Calore Animali Superstruct. p. 30, etc. — Dupuytren, in Analyse des Travaux de l'Institute, 1807, p. 16. — Blumenbach, Specimen Physiolog. Compar. inter Animantia Calidi et Frig. Sang. p. 23.

(2) Bichat's Gen. Anat. vol. ii. p. 46. Also, Hunter.

mum, the majority, unless they come frequently to the surface, perish at times which bear a ratio to the exaltation of heat. But whilst they rise frequently to the surface, and thus increase their respirations, life will be preserved. "It is by this means," says Dr. E., "they preserve the equilibrium between the effects of warmth and the influence of the air." This is very true; but it is only asserting a fact, without its philosophy. The philosophy is fatal to the doctrine of heat as dependent on respiration. The water is already too hot for the frogs, and they come to an atmosphere which is still hotter,—and which operates powerfully in raising the temperature of cold-blooded animals. According, also, to the chemical philosophy, the consumption of oxygen, which is now vastly increased, should contribute greatly in exalting the heat of the animal.

We see, therefore, that respiration has a much more important object than the generation of heat, to which it has but a very subsidiary relation; and that in the foregoing instance, as is admitted, the accelerated respiratory movements, in all probability, diminish the temperature of the animals, notwithstanding, also, the temperature of the air exceeds that of the water. According to the doctrine regarding the influence of respiration upon animal heat, the frogs should avoid the heated atmosphere most, when their temperature is highest, if it be in reality their exalted heat, as it is said, which produces the "deleterious effects."

Now we apprehend the philosophy of the foregoing phenomenon, as in all analogous cases, to be this. When the animal is at its minimum temperature, the actions of life are more feebly performed than at its maximum heat. Less carbonic acid, or something analogous, (see p. 16,) is generated in consequence; and the blood is, therefore, longer fitted for the great purposes of life. But being otherwise when the temperature is high, the animal must respire more frequently to get rid of the deleterious matter, and this without any other relation to heat; but, simply for the purpose of maintaining the blood in a proper state for the various functions. The frogs, therefore, come up frequently to the surface to let off the carbonaceous matter. This is, doubtless, the greatest final cause of respiration.

These examples of cold-blooded animals, whose temperature is chiefly regulated by the surrounding medium, appear to us conclusive of the very subordinate relation of heat to respiration.

Frogs are abundantly provided with a pulmonic apparatus, and yet their temperature is but slightly affected by respiration.

An observation of Le Gallois furnishes proof that the foregoing law, in respect to frogs, holds in the case of young mammalia. It is thus stated by Dr. Edwards:

"The cutting of the eighth pair of nerves produces, along with other phenomena, a considerable diminution in the opening of the glottis; so that in puppies recently born, or one or two days old, so little air enters the lungs, that when the experiment is made in ordinary circumstances, the animal perishes as quickly as if it were entirely deprived of air. It lives about half an hour. But if the operation be performed upon puppies of the same age benumbed with cold, they will live a whole day. In the first case, the small quantity of air is insufficient to counteract the effect of heat, but in the other, it is sufficient to prolong life considerably." (1)

Now, according to the doctrine regarding respiration, by extending the relation of the lungs with the air in the former case, the temperature should be maintained, rather than depressed; by which, surely, the "effect of heat should not be counteracted." The true philosophy is the same here as in the case of the frogs. Nor do we so clearly understand what is meant by "counteracting the effect of heat,"—especially by the means which are said to be fundamental in its generation. Indeed, we are told by Dr. Edwards, in another place, that "*the acceleration of respiration beyond the rate of health is a salutary reaction to increase the heat of the body, and counteract the influence of the cooling process.*"(2) We confess we do not understand it.

It was shown by Buffon, that new-born puppies may be plunged for a long time under water of a low temperature, without any sensible inconvenience; a much more satisfactory experiment than Le Gallois'.

But, in the foregoing cases there appear to be other principles involved, than what relates to the decarbonization of the blood; principles that are extensively concerned in the animal economy. There is a certain harmonious relation subsisting betwixt the great vital functions, especially those of circulation and respiration. A sudden and violent disturbance of either is liable to derange the other, and to affect, profoundly, the vital actions of every other part. (See Vol. I. p. 157—160.) All this will be greater in proportion to the extent in which the cause reaches the vital powers of the great organs, as in the division of the pneumo-gastric nerves.

(1) Dr. Edwards, on the Influence of Physical Agents, &c. p. 148. (2) Ibid. p. 260.

The more perfectly the great functions can be made to harmonize, where they are not invaded by absolute disease, the less will be the general violence, when any disturbing cause may operate. In the observation upon the frogs, the higher the temperature of the water, the shorter was the duration of life. The exalted temperature stimulated the heart and blood-vessels to violent action; and so great was the unnatural relation betwixt the two most important functions of life, whilst the animals were immersed, that a great violence was everywhere inflicted upon the vital actions; and, as well for this as the accumulation of carbonaceous matter, the animals soon perished. On the other hand, if the water was reduced to 32°, and the stimulus of heat thus withdrawn, the animals lived a much longer time without respiring. The functions of the whole circulatory system were reduced by the sedative effect of cold to a greater correspondence with the pulmonary functions, than in the former instance, and the shock was wholly less from this cause, upon the general powers and actions of the system. We may also add, that when the temperature is reduced, the susceptibility of the vital powers is depressed, and not only is less carbonaceous matter generated in consequence, but other destructive agents are far less efficient. It is for this reason that when the temperature of young animals is greatly reduced, they bear with impunity, for some time, the respiration of otherwise pernicious gases.

In the same way, also, including the varying demand of the blood for decarbonization, should we explain the phenomena attending hibernating animals, so far, at least, as respiration is concerned.

It is the relations which we have now stated as subsisting betwixt the functions of circulation, respiration, &c., and that betwixt the state of the blood and the organic actions, which enable us to explain the foregoing phenomena and those attendant on the accelerated respiratory movements which are produced by running, febrile excitements, &c., upon a common principle.

Next comes the application of Dr. Edwards's philosophy to the treatment of some affections of adult animals, and the consequences are made, in opposition to some of his own experiments, and to well known facts, to sustain the doctrine of animal heat by respiration.

"A person," he says, "is asphyxiated by an excessive quantity of carbonic

acid, in the air which he breathes; the beating of the pulse is no longer sensible, the respiratory movements are not seen. His temperature, however, is still elevated," "and is too high to allow the feeble respiration to produce upon the system all the effect of which it is susceptible. The temperature must then be reduced, &c. Cold air is, accordingly, successfully applied to the body, and cold water to the face; whilst the application of continued warmth would be one of the most effectual means of extinguishing life."

Now all this may be to a certain extent, correct, although we think in a different sense. But the reduction of temperature which is thus effected is only superficial; it reaches not the great organs of life, where alone it can have any important influence upon the restorative process. The operation of cold, therefore, in these cases, is upon the vital actions of the surface, by which salutary influences are propagated upon the great organic viscera. So far as a superficial reduction of temperature is instrumental, it is mainly so in diminishing cutaneous action, and thus lessening the formation of carbonaceous matter, with which the blood is now loaded, and which is one great cause of the declining actions of life. The lungs are now carrying venous blood in their arterial system; and hence the advantage of a cold atmosphere, since this is denser and contains the largest quantity of oxygen in the smallest bulk. Thence, also, it is manifest, that heat to the surface, or warm air to the lungs, operate injuriously by increasing the fatal amount of carbonaceous matter. (1)

But, after all, if it be now the simple object of the foregoing means to reduce the temperature of the body, because the existing heat is, *per se*, detrimental, by what consistent philosophy can we resort to the very means which are best calculated to establish respiration; which, according to the hypothesis, exalts the temperature in the ratio of the increase? The whole philosophy consists in diminishing, for the moment, those vital actions upon which the generation of carbon depends, and in establishing its elimination from the blood, (p. 16.) Whatever will best accomplish these purposes are among the best remedies for that affection. If the surface of the body be already cold, cold applications are only admissible as agents which may, by their action upon the nervous system, determine sympathetic influences that shall contribute to the return of respiration, and the establish-

(1) This is in no sense humoral pathology.—In our Essay on Venous Congestion, we shall show that there is another important cause which contributes to the phenomena in subjects asphyxiated by carbonic acid gas.

ment of the natural functions. The difficulty has consisted in a narrow view of the subject.

Before leaving this question, we would, also, inquire if animal heat be so dependent on respiration as is assumed, how it happens that the heat of the subject, asphyxiated by carbonic acid, is not reduced when "the pulse is no longer sensible, and the respiratory movements are not seen"? This is the last example which the chemists should have brought forward, — since the temperature is not only "too much elevated" for the doctrine, but it is often actually as high, "when the beating of the pulse is no longer sensible, and the respiratory movements are not seen," as it is in a state of perfect health, — sometimes above the natural standard. Moreover, cold air contains more oxygen in a given bulk than warm, and should, therefore, upon the chemical hypothesis, still farther exalt the heat of the subject. Besides, it is altogether a fallacy to suppose that the general temperature of the body is reduced by the contact of cold air with the lungs, since the living system, in warm-blooded animals, resists the law of communication of heat which prevails in dead matter. This is true even of the hybernating. We speak, however, of the ordinary states of the body; for, cold when extensively applied and long continued, may so prostrate the vital powers, that it will not only diminish the calorific function, but may bring the system more or less under the law of interchange of heat.

Why, in the cholera asphyxia, does the temperature often fall below 90°, — down to 80° Fh. whilst the respiration is perfectly natural or greatly accelerated, and the lungs wholly free from congestion, or any other embarrassment to the circulation, as is also true of the lungs in asphyxia from carbonic acid? (1) The organs of circulation are greatly depressed in their action; but not more so than in those instances of asphyxia from carbonic acid, in which, according to Dr. Edwards, "the pulse is no longer sensible." (2) The true reason for the difference of tem-

(1) See Essay on Venous Congestion, Sec. II.

(2) Dr. Babington thinks that animal heat is mainly dependent for its development upon the presence of red globules of the blood, (a) notwithstanding he regards the blood as a homogeneous fluid. We would rather say that it depends upon a natural proportion of all the constituents of arterial blood, and upon the natural actions of the system, like every other secretion, and like them is subject to modifications from analogous causes.

(a) On Morbid Conditions of the Blood, p. 6.

perature, therefore, in these cases, must be sought in the differently modified actions of the capillary system of blood-vessels; and it appears to be upon this principle alone, that we can explain the infinite irregularities of temperature which are presented in numerous diseases.

Referring to what was said of asphyxia from carbonic acid gas:

"The same practice," says Dr. Edwards, "is pursued in sudden faintings; the means of refrigeration must be employed, — such as exposure to air, ventilation, sprinkling with cold water. The efficacy of this plan of treatment is explained on the principle just laid down. When the asthmatic experiences suffocation, he opens the windows, breathes a frosty air, and finds himself relieved." And yet, farther on, he remarks, "that when an individual experiences a change of constitution which diminishes his production of heat or consumption of air, he cannot endure that degree of cold, which previously would have been salutary to him, without experiencing, sooner or later, an alteration in the rate of his respiratory movements," (1) — meaning that their frequency would be diminished.

The modus operandi of cold, in the foregoing cases, appears to us to be entirely different; and, whilst in neither instance can we think our author's theory applicable, there are some considerations which conflict with the hypothesis of respiration.

We shall first dispose of the case of the asthmatic subject, since this is analogous to that of the asphyxiated, though differing inasmuch as the organs of circulation are in general operation. The contact of cold air operates by diminishing the production of carbon, whilst, from the greater density of a frosty air, the lungs are more abundantly supplied with oxygen, and the blood thus most rapidly fitted for the great purposes of life. (2) Besides, according to the hypothesis which concerns respiration, there should be an immediate exaltation of temperature the moment the asthmatic "opens the windows, and breathes a frosty air." The remedy, according to the hypothesis, is clearly unphilosophical, — since, if the patient require the "means of refrigeration," he should be subjected to a rarified air; though the most theoretical method would be, to suspend his breathing.

The supposed modus operandi, therefore, of increased respiration, in either of the foregoing cases, is not only in conflict with

(1) Dr. Edwards, Op. Cit. pp. 149, 155.

(2) Whytt, reasoning naturally upon this subject, very justly says, that "the increased motion of the organs of respiration, in the fit of an asthma, are the efforts of nature to free the body of something hurtful." (a)

(a) Sympathy of the Nerves, p. 72.

the general hypothesis, but it shows the hypothesis to be founded on assumptions. That it has no relation to the existing temperature of the body may be practically shown by opposite conditions of the system, in which free respiration and a denser air are equally salutary. Thus,

"In burning fevers, with a quick pulse, red skin, and a large and quick respiration, I have given relief," says Dr. Stephenson, "by opening a window, and admitting a stream of cool air to breathe in: the effects of which I have carefully observed to be that, though the body continued *close covered*, in one minute the respiration became slower; and very soon after, the pulse abated of its fulness, and not only the face, but the whole body, of its fiery heat and colour." (1)

Here, then, we have a dense air reducing the temperature of the "whole body," "in one minute," whilst "it continued close covered." It appears to us that the only intelligible *rationale* of the process must be sought in the vital impression of the cold air upon the lungs, and the sympathetic propagation of this impression over the entire powers and functions of the body, by which the elaboration of heat was suddenly diminished. There appears not the least probability, that the action of the air upon the blood had any connection with the phenomenon; or, certainly, in that event, if the chemical hypothesis be admitted, the temperature should have been exalted, since the air respired contained a greater amount of oxygen in an equal volume. The respirations, it is true, were diminished in frequency; but this was compensated by the cause just stated; whilst every one knows that under the circumstances of the case, more air was inhaled at each inspiration, after the frequency of the respiratory movements was lessened. And we may here remark, that this consideration is constantly overlooked; it being assumed that as much air enters the lungs when respiration is rapid, as when it is slow. This will constantly vary, according to various modifying causes.

Moreover, it is well known that the respiration of cold air, in ordinary states of the system, has no tendency to diminish the production of heat in adults, however long it may be continued, — and least of all in "a single minute." Hence, we must look for the principle in laws which are peculiar to living matter, both as it regards the persistence of temperature in the healthy subject, and its extrordinary vacillations in disease. In the latter instance, the cold operates directly upon the morbid vital

(1) Edin. Med. Essays and Obs. vol. i. p. 229. 1746.

powers, — changes their action in "a single minute," and then follows, of course, a change in the products.

In connection with the foregoing subject, it was well remarked, nearly a century ago, that, if animal heat depend on the quantity of air respired, in the natural state of the body,

"We should need less of the pulmonic function, when we are warm, or in a warm place; the heat of our bodies, or of the atmosphere, doing so much of the office of the lungs; and that we should want more of it, when cold, or in cool air. But when we are hot, and want to be cooled, we breathe full and quick; and when we are cold, and want to be warmed, our respiration is slow and small; contrary to what one would expect, if the action of the air on the blood in the lungs were to heat it." (1)

All this is entirely true in respect to man, in a state of health, — however we may constantly meet with the affirmation, that cold air accelerates the respiratory movements in animals. Nor is it true in relation to man, as it was found by Dr. Edwards in regard to certain animals, that the respiratory movements are increased when cold has so far operated as to diminish the absolute temperature of the body. Man soon becomes comatose, and his respiratory movements are diminished. All the vital functions then become violently disturbed. And if the respiratory movements be increased in adult non-hibernating animals, they are shorter and shorter, in proportion to their frequency, and the volume of air respired diminishes in a corresponding ratio, till, in the language of Dr. Edwards, "the powers, being exhausted, these movements, *like all the others*, languish and fail." (2) This acceleration of respiration is only one of the common disturbances, and it appears to have no more direct connection with the maintenance of temperature, than the general disturbance of the circulatory organs.

We have, however, repeated these experiments of refrigeration, and the following has been the result. With new-born puppies, the first contact of cold air accelerated the respiratory movements; which, however, were shorter in proportion to their frequency. But, as soon as the general temperature began to fall, the frequency of the respiratory movements gave way, and soon became less frequent than in health. The same was also true of adult non-hibernating animals. The first impression of cold was that of a stimulant; but as soon as its influence was established it became a sedative. (3) Sensibility and irritability

(1) Edin. Essays, ut supra, p. 230. (2) Dr. Edwards, Op. Cit. p. 157.

(3) It will be seen, as we shall endeavour to show in an Appendix to our *Re-*

became obtuse; respiration became less and less frequent, just in proportion as other functions partially failed. And whilst yet the general temperature was scarcely affected, respiration had become slow and languid. If the animal was disturbed, then the respiratory movements were increased, but soon subsided into their subdued condition. Small birds are not the proper subjects for these experiments. Excessive cold, especially with such as are not constituted for a winter climate, inflicts a violence upon the forces and actions of life, from which no inductions can be made of a sound physiological nature. Take the adults of small quadrupeds, that are constituted for a cold climate, or even dogs, and subject them to a low temperature, and they may supply some information as to the relation of respiration to animal heat. You will then see, that when the respiratory movements are increased, there is no increase of temperature; and as they decline in frequency, if the animal be not smaller than a mouse, the temperature scarcely sustains any diminution. Now reverse the experiment, and place the animal, a dog for instance, in a warm apartment, his respiratory movements will be immediately accelerated, whilst there will be no increase of his general temperature; and the warmer the air, the more complete will be this demonstration. Then give him a sound flogging, and he shall pant for an hour without affecting in any degree the heat of his body. But it may be said that the redundant heat is carried off by perspiration. Then let the blows be inflicted after he has lain for 24 hours in a temperature at zero, there will be the same panting, but no rise of temperature. True, it depends upon a law of nature, by which the operation of cold is resisted, and increased respiration has no effect. But chemical agencies know no such restraint in the inorganic world. If a certain process produce at one time a certain result, it will always follow under the same circumstances. If a mixture of one part of sulphuric acid with six parts of water evolve a heat of 5°, and two parts of the former to six of the latter, a heat of 10°, it will always do it, when the materials are at the same temperature, at all atmospheric temperatures. Just so should it be with animal heat, if the chemical doctrine were founded in nature, so long as the temperature of animals is not reduced;

nous Congestion, that when a profound lesion is inflicted by cold upon the organic properties, it becomes apparently an irritant to those forces, and establishes an action analogous to that of inflammation.

and when exalted, then increased respiration should carry it progressively still higher.

It appears, therefore, that "the acceleration of respiration, induced by cold beyond the rate of health," is no more "a salutary reaction to increase the heat of the body, and counteract the influence of the cooling process," (1) than its acceleration by the stripes is "a salutary reaction to counteract the influence of the" blows.

And then we should not neglect to consider, in connection with the foregoing facts, what has been already stated of the philosophy of the expiring respiratory movements, and the coincident abatement of other functions, in the hibernating animals, and how all those functions are simultaneously restored by a still farther reduction of the external cold, when by its greater intensity, it has lost the nature of a sedative, and has, as during its earliest but momentary influence, become a stimulant. See APPENDIX to *Venous Congestion*, on *Cold*.

Again, there are vertebrated animals in whom the respiratory movements are diminished by cold, as in reptiles, — where it "retards them progressively, according to its intensity, until it arrests them." (2) Here, Dr. Edwards, as on other difficult occasions, is more or less of a vitalist. He refers the diminution of respiration to a peculiarity of constitution. It is in this peculiarity that resides the secret of their diminished heat, and of their "conforming very closely to the external temperature," whatever be the rate of respiration.

The whole operation of cold air, whether it act as a stimulus, or a sedative, is manifestly upon the sensibility of the nervous system, and upon the irritability of the skin and lungs; in virtue of which the great functions of the body are very variously affected, — the temperature of the body being at one time increased, and at another diminished, when respiration is accelerated, and this according to the existing condition or constitutional nature of the vital forces. Again the same vicissitudes in these respects occur, when other causes operate. Nor should it be forgotten, that in young animals, the reduction of temperature begins from the first moment after the cold is applied, and *descends most rapidly when the respiration is most accelerated*, and comes to a stand, when respiration has most abated. But however their constitution may differ, in respect to the

(1) See Dr. Edwards, Op. Cit. p. 160. (2) Dr. Edwards, Ibid. Part 4, c. 9.

power of evolving heat, from that of adult warm-blooded animals, if the decline of temperature be not counteracted by accelerated respiration in the former instance, it is not in the latter, — at least, if there be any consistency in the great laws of nature.

From all which it is evident, that the principle cannot be maintained, that "the acceleration of respiration beyond the rate of health is a salutary reaction to increase the heat of the body, and counteract the influence of the cooling process." (1) If, also, this were a general law in respect to the influence of respiration upon the cooling process, it would be fundamental in regard to animal temperature; and the various modifications of respiration should, therefore, bear corresponding relations to animal heat under all conditions of the body. But this we have already seen to be variously contradicted; and the sequel will not fail to corroborate our inductions.

Again, it is said by our author, that

"The same practice is pursued in sudden faintings, as in cases of asphyxia from carbonic acid; the means of refrigeration must be employed, such as exposure to air, ventilation, sprinkling with cold water. The efficacy of this plan of treatment is explained on the principle just laid down;" that is to say "the temperature is still elevated, and is too high to allow the feeble respiration to produce upon the system all the effect of which it is susceptible. The temperature must be reduced," &c.

Here the whole physiological process is entirely different, to our mind, from the supposed analogous case of asphyxia from carbonic acid. There is a simple suspension, or rather a great diminution, of the organic functions, and least of all is a reduction of the temperature of the body calculated to effect their restoration. If cold air be beneficial, it is by its direct action as a stimulus upon the vital forces, in virtue of which the elaboration of heat is at once augmented, not diminished. Any remedy which should operate directly upon the principle laid down by our author would more or less defeat its intention. (See Vol. I. p. 175 — 179.) There can be no condition of the body, short of absolute death, in which the temperature fails more rapidly than in syncope, and none in which absolute refrigeration can be more pernicious. Indeed, there will be no positive relief, till a reproduction of heat again goes on; and the general hypothesis asserts that it is the direct tendency to cold, when it lessens animal temperature, to diminish respiration. The foregoing is one of the practical errors that has grown out of the chemical theory

(1) Dr. Edwards, Op. Cit. p. 160.

of respiration. It is seen, also, by the results, that what we have said (p. 42,) as to the general inapplicability to organized beings of the physical law of interchange of heat is fallacious in these cases.

But let us analyze the subject a little farther, since these practical examples strike at the foundation of the doctrine, and admonish physiologists to protect their own department against the encroachments of a science which, however vast and important its legitimate objects, has never failed to engraft upon medicine the worst innovations, whenever it has laid its iron grasp upon the science of life.

Now, it is known to every tyro in medicine, that a single drop of cold water falling upon the face, in a paroxysm of syncope, will often produce a convulsive inspiration, and may be sufficient to establish all the functions. The effect, too, will be considerably determined by the force with which it may strike. It will be greater when snapped from the finger, than when a much larger quantity is dropped from the hand. Shall it be said that there is anything like the alleged refrigeration in this drop of cold water? Woodall, a most intelligent and accurate observer, states that the best remedy for syncope is to obstruct respiration entirely, by momentarily confining the nose and mouth. "I have used this course," he says, "from my youth to this day." (1) The philosophy of its operation is probably that of creating a "sensation of uneasiness in the sensorium," as Dr. Philip would call it. (2) "The muscles of respiration," says Mr. Bell, "are put under the guidance of sensibility." (3) This is a remedy

(1) Surgeon's Mate, p. 29, 1617.

(2) See his treatise on the Means of Preserving Health, p. 58, &c. Also, Whytt on Vital Motions, s. 8, and Brodie's Exp. in Philos. Trans. 1812.

(3) "The results," says Mr. Grangier, "prove first, that in parts of the body indisputably deprived of all feeling and power of voluntary motion, contractions may be excited in the so-called voluntary muscles, by impressions made on the skin. Secondly, that this capability of exciting voluntary contraction, is not equally possessed by all parts of the external surface of the body; but that the sole of the foot, which, in walking, comes in contact with the ground, is that precise part in which the action is excited in the most energetic manner." (*a*)

Just so it is in syncope. The face is the part upon which the drops of cold water make their most efficient impression in rousing the respiratory muscles; and the next most efficient impression upon the skin is tickling the soles of the feet. It helps refrigeration prodigiously.

These principles, we all know from observation, are equally true of the viscera of organic life.

(*a*) Observations on the Structure and Functions of the Spinal Cord, p. 95.

apparently the reverse of that of respiring cold air; and yet the principles upon which they operate are not very different. In the same way, the most pungent stimulants applied to the nose, which are calculated to exalt, not to diminish heat, are often far more useful than a current of air, or sprinkling the face with cold water. The drinking of cold water is also useful; but alcohol or ammonia may succeed when the former fails. Each operates upon a common principle, — that of exalting the forces of life either directly, or through the medium of a sudden and strong impression transmitted to the *sensorium commune*.

In our essay upon bloodletting, we have pointed out other means of restoration from a paroxysm of syncope, none of which have any connection with the imputed principle of refrigeration; but their tendency is directly the reverse. Carry the doctrine of refrigeration into absolute practice, and the entire surface of the body should be bathed with cold water in the most obstinate cases. Try it, however, and you will find it fatal; not, however, by a direct abstraction of heat from the body at large, but by the pernicious impression of so general an application of cold upon the functions of the skin, and by sympathy, upon the heart and other viscera. On the contrary, in these cases, we apply heat, and other uniform stimulants to the general surface, and thus endeavour to rouse the vital forces and capillary action of the skin. This doctrine of refrigeration, therefore, must be sent back to the chemical school for reconsideration. It is not suited to the practical business of life.

The whole of our philosophy is corroborated by Dr. Edwards, when he comes to explain the manner in which an intense degree of cold rouses the hibernating animal from his torpid state; for in this instance he regards it as acting upon the principle of a stimulus.(1) So, also, "the immersion," says Dr. Edwards, "of a great part of the body in warm water, is frequently an efficacious means of exciting the movements of the chest, and reanimating a child just born without signs of life."(2) But what physiological difference is there betwixt this case and that of syncope? All these cases, indeed, are analogous; and whether heat or cold be the agents, they exert their effects as vital stimuli. Refrigeration has just as little to do with one case as with another, — as appears by our author's own showing in the instance last cited.

(1) Op. cit. p. 159. (2) Ibid. p. 283.

If we now revert to the experiments of Dr. Edwards on the combined action of air and temperature, we shall farther see that his conclusions do not appear to be sustained. The cold-blooded animals, from the very nature of the question, are in no respect proper subjects for the general deductions which are made; (see Vol. I. p. 698, &c.) though their phenomena are clearly opposed to the whole chemical theory of animal heat; and, in regard to new-born puppies, they approximate so nearly the hibernating animals, that, in rendering them torpid, they sustain a loss of 40° to 50°, or more degrees of Fh.; a reduction of temperature sufficient in itself to influence powerfully all the great vital functions, and to place the animals in anything but a natural condition. Vital stimuli, and other natural agents, cease to manifest their natural developments whenever the organic powers and functions are violently impressed. This standard of comparison is, therefore, inappropriate, however advantageous to our views. Our philosophy, so far as experiments are concerned, must be deduced from the phenomena of adult, warm-blooded, non-hibernating animals. And in respect to the practical inductions, as intended to be exemplified by the foregoing cases of asphyxia, &c., the cold is not applied to the general surface of the body, or at least, anything like absolute refrigeration would be pernicious; whilst in the experiments which are carried up to the foregoing important objects where human life is immediately concerned in the true philosophy, cold is not only universally applied, but profound refrigeration is produced.

If animal temperature depend directly on the relative quantities of atmospheric air to which the pulmonic circulation may be exposed, and it be true, according to Dr. Edwards, that, on the application of cold, "the acceleration of respiration beyond the rate of health is a salutary reaction to increase the heat of the body and counteract the influence of the cooling process;" or, if respiration "stand related to the production of heat as cause and effect," — then the whole reasoning as to the supposed effect of increased respiration in diminishing temperature in asthma is suppositious; or, if true, is subversive of the whole theory; whilst the statement which we have made as to the reduction of an exalted temperature by increased respiratory movements, or by the inhalation of a denser air, whether the exalted temperature have depended on natural or morbific causes, or natural or morbid states of the system, is utterly at variance

with the chemical theory. We shall soon show, also, that the same statement is corroborated by the experiments of the chemical philosophers.

If the principle just laid down by Dr. Edwards have any existence, it is fundamental, and should apply, in a general sense, to at least all warm-blooded animals. But we have seen that absolute exceptions exist, and many others are admitted. Amongst the warm-blooded vertebrata, exclusive of the hibernating, are many conditions, "in which the temperature progressively falls, notwithstanding the acceleration of respiration." (1) All young animals, indeed, have their respiration greatly accelerated on the application of cold, whilst their temperature generally descends very rapidly, although the cooler air contains a greater quantity of oxygen in a given bulk, than the warmer; and it is remarkable that the fall is greatest at the beginning of refrigeration, when the respiration is most accelerated. According to Dupretz, also, more oxygen is consumed by young animals, than by adults. (2) Dr. Edwards exposed "young birds, whose temperature was about 100° Fh. to an atmosphere of 64°, when they cooled down to within one or two degrees of the external air." Others of the same heat were exposed to the high temperature of 71° Fh. when they cooled rapidly down to within one degree of the atmospheric temperature." (3)

Here it certainly cannot be said that accelerated respiration had any apparent tendency "to counteract the influence of the cooling process;" and, indeed, in this very conclusion our author agrees.

"Whatever," he says, "be the *modifications* of the respiratory movements of young unfledged birds, their cooling is always progressive until the limit at which the cold benumbs them; and it is not at *this period* that we can discern the influence of respiration upon temperature; but *some days later*, when they develope more heat, we frequently recognise by unequivocal indications, that the acceleration of respiration beyond the rate of health is a salutary reaction to increase the heat of the body, and counteract the influence of the cooling process." (4)

Now the whole of this matter appears to us susceptible of explanation upon plain physiological principles, and to place the chemical doctrine in its proper attitude. If there were the least foundation for the hypothesis, the younger birds should certainly manifest some evidence of its truth; and that "if increased respiration and the development of animal heat stand related

(1) Dr. Edwards, Op. cit. p. 161.
(2) Annales de Chimie, t. 26, p. 360.
(3) Dr. Edwards, Op. cit. p. 71
(4) Ibid. p. 160.

as cause and effect" in any one instance, there should be some manifestation of this connection in all others. The law, being fundamental, is utterly baseless, if it be liable to palpable exceptions. (See Vol. I. p. 626, *note.*) But we have the full admission, that in very young birds, the increased respiratory movements have not the least tendency to counteract the cooling process. Chemistry, therefore, is out of the question ; and we must look for the true philosophy in other principles. These we shall find in certain laws which are as foreign from those of chemistry, as the contradictory facts are from each other. The organization of these young birds is not fully developed. The whole cerebral system, and every other part, are only yet in partial existence, and all reason, as well as facts, assures us that there must be a corresponding analogy in respect to the forces of life. Therefore is it, that the moment the actions are only slightly subdued by the depressing influence of cold, the functions of the secerning vessels are more or less arrested, and along with other animal products there is a failure in the generation of heat. But even in these cases, as in that of the hibernating animals, there is a constitutional provision for the safety of life, by which the pernicious influence of cold is resisted, if the low temperature be not excessive. And when we come to regard the condition of the same animals "some days later," and see them resisting the influence of cold under the foregoing circumstances, we have a coincident proof, in the converse phenomenon, and in the greater maturity of organization and the vital forces, that the greater development of animal heat is purely a vital process. But, supposing a state of absolute ignorance of physiological science, the contrast which appears in the immediately preceding cases, under the same circumstances of respiration and external temperature, shows that the evolution of heat depends, essentially, on some other cause than respiration.

The foregoing results attended, also, similar experiments upon puppies ; and the same is more or less true of the young of all animals.

Will any one believe that the adult human subject, when his general temperature is actually reduced, can elevate, in the least, the existing degree, by any voluntary acceleration of respiration ? According to the hypothesis, it should be a fundamental remedy. On the contrary, however, stimulants of any description, that shall increase the action of the capillary vessels, will start the

evolution of heat. Nor is the inefficiency of respiration owing to a want of circulation in the capillaries of the internal parts; for when we come to the subject of venous congestion, we shall show that it is the effect of reducing the general temperature of the adult human subject, to determine the blood upon the internal capillaries. Besides, the experiments upon the birds, just recited, show that the phenomenon is not owing to an absence of blood in the internal capillaries; since, otherwise, it should have been equally true of both cases.

If respiration be performed in a deep and hurried manner, in a tranquil state of the body, there should be, according to the rule, an evolution of heat, and *vice versa*. By the foregoing act, also, not only a greater amount of air is supplied, but a greater volume of blood is determined upon the lungs.(1) But every body knows that there is no attendant increase of animal heat.

On the other hand, however, as we have shown, "when we are hot and want to be cooled, we breathe full and quick; and when we are cold, and want to be warmed, our respiration is slow and small;" and, as we have seen, it is admitted by the chemists, that respiration is increased by the stimulus of heat; but then, the science has it, that "this increase of the respiratory movements is necessary to *counteract, at least for a time*, the effects of the external heat."(2) Is it said that respiration is accelerated by running, &c., and the temperature augmented? Here, the organic functions are increased; and Becquerel and Breschet found that the temperature is affected 1°—2° Cent. by each contraction of a muscle.(3) All the secretions are greatly augmented in a corresponding manner. The sweat flows profusely, and the same cause is one of the most efficient means of increasing the bile.

And then, in respect to the hibernating animals, we have not only opposite results to those just stated, but perfectly conflicting ones; such, indeed, as are utterly incompatible with chemical philosophy. At an atmospheric temperature of 20° Fh. their heat goes rapidly down to near the freezing point, notwithstanding the accelerated respiratory movements; and there it stands.

(1) "Dr. Holland said, that he had made repeated experiments, and had invariably found, that a series of deep inspirations did always bring to the lungs a larger quantity of blood than previously existed." (*a*)

(2) Dr. Edwards, Op. cit. p. 152. (3) An. des Sci. Nat. Mai, Oct. 1836.

(*a*) Silliman's Jour. of Science and Arts, vol. xxxiv. p. 28. British Association for the Advancement of Science.

But depress the temperature of the air still lower, down to zero, and, although the respiration be scarce sensible, an evolution of heat begins at once, and the temperature of the animal rises rapidly up to 97°, and that, too, without any remarkable acceleration of respiration, there being only a slow return to its natural state.(1) Now it is manifest, from the circumstance that accelerated respiration did not counteract the decline of heat when the inferior degree of cold was applied, it had no connection, in a chemical sense, with the great and rapid restoration of heat, when the intensity of the cold was increased. If you appeal to constitution, this is exactly our argument,—and that constitution consists in the adaptation of the vital forces to the evolution of heat according to the impression of vital stimuli. The peculiarities of the hibernating animals also supply an impressive illustration of the specific nature of the powers of life, as contradistinguished from those of physics, and of the variety which appertains to the vital forces.

For these reasons, we dissent from the induction which is derived from these experiments, and which is expressed, as follows, by Dr. Edwards: "In these experiments," he says, "the external temperature had nothing to do with the restoration of heat, except by exciting respiration and circulation; thereby showing that increased respiratory movements and the restoration of heat stand related as cause and effect."(2) We might equally make the same affirmation of any other returning function. One would be just as logical as the other.

There is also another remarkable peculiarity respecting the temperature of hibernating animals. They maintain their heat, unlike the young of other animals, when the thermometer is pretty low; and do not part with it till *sleep* takes place. Till this event comes, they commonly bear all degrees of temperature without passing into the hibernating state. This singular fact is a key to the whole philosophy. Their *sleep* is induced by a change which is established in the organic forces, and it is this change which arrests the generation of heat. It is coincident with the alteration of the vital actions. The *sleep*, therefore, has not the connection which has been supposed by Dr. Hall, and others, with the diminished elaboration of heat. Like the latter, it is only

(1) Dr. Edwards, Op. cit. pp. 158, 159. (2) Op. cit. p. 158.

one of the consecutive results which follow the change in the organic powers,—or, if you prefer, the vital actions.(1)

We have just seen, that the young of certain non-hibernating animals, whose natural temperature is 100° Fh. when exposed to an atmosphere of 71°, or only 5° below summer heat, cooled rapidly to an equilibrium with the surrounding medium, notwithstanding the respiratory movements were simultaneously increased. This, being more or less true of all new-born animals, it is fair to establish an hypothesis upon these experiments as upon those which are made upon adults. We might maintain an equally fallacious induction, that the accelerated respiratory movements and the decline of temperature "stand related as cause and effect." Or, at least, we should have the conflicting theories, that in early life it is the effect of increased respiration to diminish animal heat, whilst the principle does not obtain with adults. The contradiction shows the absurdity of either induction. Experiments which aim at the discovery of any great law must be considered connectedly; especially when that law is to be grounded upon their absolute results. The moment we depart from their indications, and explain difficulties and contradictions by other principles, we lose sight of true philosophy, and are in the wide field of conjecture, where we can only hope, like the alchymists, to stumble upon the truth. This truth may be as foreign from the immediate object of our experiments, as the vital are from the chemical laws.

What, then, is the cause of these remarkable differences as to variations of temperature under the influence of the same causes, amongst hibernating animals, and the young and the old of the non-hibernating? Respiration professes to explain it one way, in one instance, whilst its apparent effect is perfectly opposite in another. We apprehend, therefore, that respiration has only a remote connection with the phenomena. We think, rather, that the whole is to be explained by the difference in the condition of the vital forces in the different animals, or at their different ages. According to these differences, cold will make different impressions upon their organic actions and variously modify all

(1) If diminished temperature take place in the natural state of an animal, the principle is the same; the actions of life are performed with less energy, than in a state of wakefulness. Dr. Edwards contends for the fact; (*a*) but is it common, even with the young? Observe the infant, or the man,—you will find no general diminution of temperature. But respiration is more slowly performed. Will chemistry explain?

(*a*) Op. cit. Part 4, Chap. 17.

the results. Accelerated respiration is the first in the series. The philosophy of this we have endeavoured to explain. In all, the various actions are more or less reduced, according to the constitution of the animals, and the intensity and continued operation of the agent,—often modified, however, by very accidental causes. Amongst the numerous results, the generation of heat is, in consequence, more or less diminished; and diminished respiration, circulation, &c. follow as other results.

In certain small animals, a high atmospheric temperature overcomes the constitutional provision which establishes an exact measure of heat; and, although in these cases, respiration is greatly more increased than by the application of cold, it should be just the reverse if the chemical philosophy were founded in nature, that on the application of cold, "the acceleration of respiration beyond the rate of health is a salutary reaction to increase the temperature of the body and counteract the influence of the cooling process." Delaroche and Berger exposed to the temperature of 122° and 200° Fh., a cat, a rabbit, a pigeon, a yellow-hammer, and a large frog. In about half an hour, they became agitated, and their respiration was progressively accelerated for about three-quarters of an hour, when it became panting; and, at last, they died.

Upon this experiment Dr. Edwards remarks, that "notwithstanding the diversity of species, and of classes, and of the degrees of heat to which they were exposed, they all acquired nearly the same increase of temperature, the limits of the variation being from 6.25° to 12.92° Fh." (1) These are examples in which heat modifies the vital forces in such a way as to allow a partial communication of external heat agreeably to its physical laws; and as soon as it determines the complete extinction of the vital forces, an equilibrium of heat becomes established. Nor does this conflict, in the least, with what we have said at p. 42, as to the resistance of external heat or cold, since in the cases there supposed, the organic changes now contemplated are not established, and the application of cold was comparatively momentary. In our present case, the continued and extensive application of cold will, like that of heat, induce the same vital changes, and ultimately bring the system partially under the inorganic laws of caloric, before the full extinction of life.

The acceleration of breathing by heat, Dr. Edwards states,

(1) Op. cit. Part 4, ch. 14, s. 1 and 2.

is "a *general phenomenon*." (1) If, then, "its acceleration beyond the rate of health be a salutary reaction to increase the heat of the body and counteract the influence of the cooling process," when cold is applied to the surface; what is its object when it arises with as much uniformity, and to a greater extent, from the action of heat? Dr. Edwards shall explain: "*This increase of the respiratory movements*," he says, "*is necessary to counteract, at least for a time, the effects of the external temperature*." (2) In one case, the temperature is exalted by the increased frequency of the respiratory movements; in the other, it is depressed, or rather counteracted, by the same cause; or, as Dr. Edwards finally has it,—probably by way of escaping under the cover of a solecism,—"more air comes in contact with the lungs in a given time, and reanimates what the heat depresses." (3) To such as may comprehend this explanation, we have only to say, that, agreeably to the doctrine, and, as we understand it, to the foregoing statement, the "reanimation" consists in an increase of heat by the respiratory movements, whilst the external heat depresses the animal temperature. But it is agreed on all hands, that the direct tendency of a high external temperature is to raise the heat of the body, and the only wonder is that such is not actually always the result. Since, therefore, when heat is applied, we see not either what is "depressed," or how, by the chemical rule, the "effect of the external heat is counteracted" when "more air comes in contact with the lungs in a given time," especially, also, as the air respired has an exalted temperature. It is clear, therefore, that the chemical doctrine must fall by the observations on which it professes to rest.

Again, what is to be "counteracted" by the greatly increased respiratory movements which follow running, or which attend the immersion of the limbs in hot water, or any cause that excites the organs of circulation? By the chemical hypothesis the accelerated respiration should tend to increase the heat of the body, whilst it flatly contradicts the vital doctrine of the "*vis conservatrix*," whose "counteracting" aid it enlists in these extremities. (4) Is not increased respiration, in all these cases, owing to a principle which preserves a harmonious relation amongst the great organs of life; so that, if the action of the

(1) Op. cit. p. 152. (2) Ibid, p. 152. (3) Ibid. p. 152.
(4) Dr. Edwards, Op. cit. Part 4, ch. 9 and 10.

circulatory organs be preternaturally excited, the lungs, from their mechanical, as well as vital relation to the general circulation, shall adapt themselves to the change; and this, as the facts show, without the slightest reference to temperature? Take an opposite case, that of fear. Here the circulation and respiration are often greatly accelerated; but this is an instance in which the temperature of the adult subject actually falls. (See Vol. I., pp. 410—414, 422.) Or another, in which it was found by Martine, that long fasting reduces the temperature of the body several degrees, although respiration continue unaffected. The diminution of the body corresponds with the diminution of the blood. The philosophy seems to be obvious. The organic actions are languid; and heat, in common with the other secretions, is diminished in consequence. The blood, however, is oxydized in the usual manner, and the same quantity of carbonic acid is evolved as in the well fed subject. (P. 16.)

Whatever irregularities, in health or disease, may arise in respect to temperature, they are susceptible of an intelligible explanation, in exact conformity with all the other results of life, by modifications of the vital forces and their actions as carried on by the capillary vessels, and as appears to us, by no other philosophy.

The foregoing contradictions, and varieties, appear, therefore, not to be reconcilable by the fundamental principle which has been assumed; and which is brought into a more embarrassing attitude by those, who, like Dr. Edwards, mix up with the chemical, in partial subserviency, the doctrines of the vitalist when chemistry stumbles. (See Vol. I., pp. 48, 49, 75, 85, and Essay on *Digestion*.) Every law of nature, whether in the animate or inanimate world, produces consistent phenomena. There may be some apparent, but no real exceptions. (Vol. I., p. 626, *note*.) Whenever this is denied, it is always for the purpose of building up a doctrine with which nature is at war, and which has no stability. If "increased respiratory movements and the production of animal heat stand related as cause and effect," in a chemical sense, in any one instance, it is a fundamental phenomenon, dependent on a universal law, and should not be contradicted by any palpable exceptions.

And here we may mention the curious fact, that the respiration of pure oxygen gas depresses the temperature of animals,(1) —but not more so than it lessens the products of other animal

(1) Allen and Pepys respired it without inconvenience.

functions. This should not be so, but exactly otherwise, (1) if the chemical doctrine had any foundation. But in the forces of life we find a perfectly intelligible solution; the interpretation being even supplied by the various coincident results which attend the depression of heat.

It is stated by Dr. Edwards, that every new portion of air which enters the lungs not only abstracts an amount of caloric sufficient "to raise the whole mass nearly to the temperature of the body;" but, "in virtue of this acquired elevation, whatever may have been its previous hygrometic state, it converts into vapour the liquid with which it may be in contact." And it should be recollected, also, that "air in a frost contains scarcely any watery vapour; so that, when raised to an equal temperature with the air in summer, the quantity of liquid dissipated by evaporation is much greater," than when already charged with moisture. (2) There will then have been abstracted from the lungs the sum of the free caloric acquired by the air, and the latent caloric employed in converting the liquid into a state of vapour. According to the observations of Lavoisier, and Seguin, the loss by perspiration from the skin and lungs is in the ratio of two to one. (3) The vapour expired being, also, at a much higher temperature than cutaneous perspiration, it is apparent that the lungs must be a powerful abductor of heat. But neither pulmonary evaporation, nor the respiration of cold air, are capable of varying, in any essential degree, the natural temperature of adult non-hibernating animals. It is even difficult to reduce the temperature of the smallest adult warm-blooded animals. In the month of February Dr. Edwards confined five adult sparrows in glass vessels at an atmospheric temperature of 32° Fh. and covered the lid with ice. In the course of an hour, they lost, on an average, 7° Fh. — some having lost *none*. Their temperature then remained stationary to the end of the experiment,

(1) This was once admitted; and upon the strength of the hypothesis, Dr. Beddoes "slept equally well under fewer blankets than usual, after taking oxygen gas at going to bed"! and Dr. Thornton "restored warmth to the feet of a patient" by this gas.

(2) Dr. Edwards, Op. cit. pp. 179, 263.

(3) Trait. de Chim. p. 228. See, also, to the same effect, Swammerdam, de Respiratione, s. 1, c. 1, § 9, and c. 3, § 4; — Boyle's Works, vol. i. p. 103; — Harvey, de Motu Cordis, Ex. 2, p. 194, and Ex. 3, p. 232; — Fabricius, de Respiratione, l. 1, c. 6; — Bartholin, Anat. p. 430; — Morozzo, in Journ. Phys. t. xxv. p. 120; — Le Gallois, in An. de Chim. et Phys. t. iv. pp. 5, 113, and Sur la Vie, pp. 20, 241; — Spallanzani, de Respir. &c.

which lasted three hours. In another like series of experiments, in the month of August, the mean loss was 2.9° at the end of the first hour. (1)

The extraordinary fact that man and animals maintain, in the arctic regions, an equal temperature at all seasons, is abundantly established:

"The thermometer," says Capt. Parry, "for seventeen hours, ranged between 54° to 55° below the zero of Fh. during which not the slightest inconvenience was suffered from exposure to the open air." "No unusual sensations were experienced during the winter, though in going from the cabins into the open air, and *vice versa*, we were constantly in the habit, for some months, of undergoing a change from 80° to 100°, and in several instances of 120° of temperature, in less than a minute; and what is still more extraordinary, not a single inflammatory complaint occurred during this particular period." (2)

Mackenzie says, that some of the northern savages follow the chase in the coldest weather with only a slight covering. (3) Lewis and Clarke state, that two Indians slept upon the snow during the night in a light dress, when the thermometer was 40° below the zero of Fh. The man was uninjured; the boy had his feet frozen. (4) Now it is evident that no civilized man could sustain such an exposure. The phenomenon is owing to the *power of habit* in its relation to the forces of life, and is utterly insusceptible of explanation on any other principle.

In the arctic regions, are the rein-deer, musk-ox, bears, foxes, hares, birds, &c. living also upon half-frozen food; yet maintaining under those circumstances the same temperature as when transported to southern climates. It is also important to remark, that the respiratory movements become accelerated in the warmer climate.

In fifteen out of sixteen foxes, the temperature was 100° to 106¾°, in the other 98°; the thermometer ranging from *minus* 3° to — 32° Fh. (5) Capt. Lyon found that the tetro albus maintained its temperature at 50° below the zero of Fh. (6) The

(1) Op. cit. p. 82. (2) Journal of a Voyage, &c. p. 121.

(3) Mackenzie's Travels in North America, p. 94.

(4) Lewis and Clarke's Travels in North America.

(5) Parry's Journal of a Voyage, &c. p. 157.

(6) Lyon, Sur Températures de quelques Animaux du Nord, &c. in An. de Chim. et Phys. 1825, p. 223. See, also, Gmelin, Flora Siberica, Præf. — Barlow's Chronolog. Hist. of Voyages into the Arctic Regions, c. 2; — Dr. Aikin, On the Attempts to Winter in high Northern Latitudes, in Mem. Manchester Philos. Socy. vol. i. p. 89, second ed. — Philos. Trans. Abd. vol. iii. p. 470. (Ice froze on lakes 12 feet deep.) — Martine, de Similibus Animal. et Animat. calore, 1740; — Pallas, Novæ Species Quadruped. et Glirium Ord. 1774, and Travels in Russia; — Hunter, in

same resistance of cold is seen in the whale, who maintains a temperature of 102° in the frozen regions. It was equally so with the smallest birds; whilst it is obvious that the cooling influence of the atmosphere on these small bodies should be incomparably greater than on the larger animals that inhabit the same region, but whose temperature is alike fixed at a lower standard.

And then, on the other hand, are the well known experiments of individuals subjecting themselves to an excessively high temperature, without sustaining any sensible variation of their heat. This was fully demonstrated by Blagden, Banks, Fordyce, Solander, G. Home, Dundas, Dr. North, Phipps, Seaforth, and Dobson, who exposed themselves to a temperature of 260° Fh.(1)

As to high natural temperatures of the air, the effect would, *a fortiori*, be the same. At a very extreme degree of atmospheric heat, Gov. Ellis "could never raise the mercury above 97° with the heat of his body."(2) Similar observations were early made by Lining, and Adanson; and they have since been so generally repeated, that it is well ascertained that neither climate nor season make a greater difference than 1° to 2° Fh.; and it is important to remark, as showing the entire independence of this phenomenon of respiration, that the change does not take place till such as remove from one climate to another shall have been for some time subjected to the new condition of vital stimuli. It is the result of acclimation, and trivial as it may seem, it is full of the most instructive illustration to a reflecting mind.(See Vol. I. p. 691.) Some individuals, like Dr. Franklin, have a constitutional temperature of 96°, which remains without variation under all circumstances of external heat.

Again, the power of resisting heat and cold appertains to certain animal substances that are devoid of organic actions. This

Philosoph. Trans. 1775, p. 446, and 1778, p. 7; — Davy, in Edin. Philos. Journ. Jan. 1826; — Braun, Nov. Comment. Act. Petropol. t. 13, p. 419; — Skjelderup's Dis. sistens vim frigoris incitantem, 1803; — Bauer and Becker, de Effect. Calor. et Frigoris exteri.

(1) Philos. Trans. 1775, pp. 111, 484, 463. See, also, Dantz, Exp. Calorem Animal. Spectant. 1754 — Duhamel and Tillet, in Mém. de l'Acad. des Sciences, 1764; (at 290°.) — Berger and Delaroche, Exp. sur les Effets qu' une forte Chaleur prod. sur l'Œconomie, 1806; — Crawford, in Philos. Trans. 1781, p. 479; — Van Mons in Journ. de Physique, t. 68, p. 121, 1809 — Dantz, Exp. Calorem Animal. Spectantia, 1754.

(2) Philos Trans. 1758. p. 755.

was proved by Mr. Hunter, in respect to eggs, "in a degree equal to many of the more imperfect animals." The fresh egg possesses, also, the power of resisting putrefaction, — which farther shows the existence of a common principle upon which those phenomena depend. (1) This, indeed, is an instance which appears to denote an especial dependence of animal heat upon the *vires vitæ;* whilst its greater evolution is seen in the movements of organization, like the other phenomena of life, to be the result of those powers in active operation. The former condition is more incomprehensible than the latter, since we have no evidence of organic action in the unincubated egg. Still it is no more remarkable than the laws upon which the phenomenon depends. It is a beautiful proof, also, of the distinct nature of life when regarded in its connection with organic matter; that its laws are specific, and that life is an active, not a passive state. (See Essay on the Vital Powers, Sec. I. and on Digestion.)

We see, then, in the foregoing demonstration of the power of all warm-blooded non-hibernating vertebrata to maintain a uniform temperature under the greatest vicissitudes of atmospheric heat that are compatible with life, a proof of a most astonishing law of the living body, in perfect conflict with the laws of caloric as they exist in the inorganic world. We know it as exactly as we comprehend the nature and operation of the most precise law in physics. It is, in itself, demonstrative of the government of living organized matter by specific forces, — establishes a positive distinction betwixt those forces and the organized structure. If we are not right in this induction, let the ground of objection be shown. We mean not the usual denial. The objection must be founded upon a broad and philosophical survey of all the phenomena of heat that relate to living matter as they may be modified by natural causes, or by morbid states of the system; and the ground must cover the general physiological condition of organized beings. It would be an unmeaning jargon to say, that the generation of animal heat is a chemical phenomenon, but dependent on certain other variable powers; more especially since the very existence of these powers is denied even by those who retreat to their assistance, as if for the purpose of magnifying an absurdity.

It is evident, that the foregoing extraordinary principle is mainly connected with the vital forces of the solids, — that it is

(1) Animal Economy, ut. cit. Exp. 39, 40, 41; also, his Lectures on the Principles of Surgery, p. 22, Phila. — So, Mayo, in Physiology, p. 64, and others.

an indispensable element of life in all warm-blooded animals, and that the direct function of generating heat cannot be separated from it. The power of resisting putrefaction is scarcely more a fundamental law of the animal economy, than that of maintaining a uniform temperature, as it respects warm-blooded animals; and there would seem to be as much reason for referring the former to chemical influences, as the latter, and *a fortiori*, of resolving the whole living being into chemical agencies.

Again we revert to the cold-blooded animals, as supplying, by the contrast, an equal proof that respiration, and oxygen gas, are only remotely connected with animal temperature, and not in the least in a chemical sense. Their temperature is principally regulated by the surrounding medium, since "it differs no more than one or two per cent. from the external air throughout the various seasons of the year."(1) Frogs, &c., have capacious lungs, and their remarkable subjection, therefore, to external heat is completely subversive of the chemical hypothesis. Respiration, too, is as indispensable to these animals as to man; and they equally perish when deprived of atmospheric air. In their ordinary state as much oxygen is absorbed, and as much carbonic acid expelled, as by warm blooded animals. It is true, they are rather more tenacious of life; but, this is owing to peculiarities of those forces by which life is constituted, whilst the variety itself adds confirmation to our whole doctrine. There is nothing in physics that will explain it. These animals will live for a time in hydrogen and carbonic acid gases; and in connection with this fact, it is important to observe that their temperature, according to Spallanzani, Edwards, and others, is no more affected by them than by atmospheric air.

The fact, therefore, that the vast tribe of cold-blooded animals respire equally with the warm-blood, and that carbonic acid is also exhaled, though in variable proportions, whilst they generate heat but very feebly, appears to denote a great independence of animal heat of the direct agency of the respiratory function in the warm-blooded animals. Still the fact, that cold-blooded animals possess in a low degree the power of evolving heat, even adds to the force of our conclusions. Mr. J. Davy, in his South-

(1) Edwards, Op. cit. p. 197. — This is an extraordinary fact in the animal economy, and strikingly exemplifies all that we have said in relation to the modifications of the forces of life in different genera of organized beings. See p. 24, and vol. i. pp. 650, 696—698, &c.

ern voyage, found "the temperature of all the fish he tried above that of the water, by two or three degrees, Fh."(1) Here, too, we should contrast the temperature of the whale 102°, and that of the porpoise 100°, &c., with the frog, turtle, &c. If you resort to constitution, you resort to the vital forces, and utterly abandon your own ground. You say, in one case, it is because they are cold-blooded, and in the others, because they are warm-blooded, and so on. Such, indeed, is the fact. But is it not because the organization and vital forces are not adapted to the same generation of heat, in one case, as prevails in the other; and this, too, where organization may be in a high degree simple, as in the bee? Nor can any explanation be founded upon any known difference in the function of respiration, the constitution of the blood, or the changes effected in that fluid. (See pp. 24 — 26, *note*.) Cold-blooded animals respire, also, as we have said, by the skin as well as by the lungs, gills, &c., which, by its great extent in these instances, farther shows the design of this function for other more important purposes. Some animals, like the cobitis fossilis, swallow air, and thus respire by their intestines. Even the embryos of frogs, toads, sharks, rays, and the sword-fish, are provided with external bronchi. In mammalia, the respiratory function of the ovum is constituted by the circulation of the parent. There are, also, speculations whether the placenta and liquor amnios may not serve the function of respiration to the ovum, whilst it is affirmed that the temperature of the ovum depends wholly upon the parent.

But whilst frogs, and other cold-blooded animals, are peculiarly subject to the law which regulates the distribution of heat in the inorganic world, they possess, (we must repeat,) as shown particularly by Crawford, a feeble power of resisting its influence; since "a living frog acquires heat more slowly than a dead one."(2) And yet, with this palpable fact, combined with the subjection of living frogs to the physical law of caloric, and their large respiration, Crawford went on to construct his ingenious and elegant hypothesis of the dependence of animal heat upon chemical agencies. It is, however, one of the rare instances in which false inductions will always find a passport to well merited applause in their great ingenuity. Here was an apparent exactness conformable to the precise laws of chemistry, a

(1) Lon. Quarterly Journ. of Science and Arts, No. 3, Art. 26.

(2) Crawford, in Philos. Trans. 1781, p. 485.

beautiful simplicity, a harmony of elementary parts, and no mixing up of the forces of chemistry with those of vitality, to expound a simple phenomenon of nature. It will remain forever a most honorable monument to the human mind, whilst it will be equally a lesson to warn us of the dangers by which reason is surrounded.

Again, let us contrast with the cold-blooded animals, that respire with lungs, some of the minute members of the insect tribe, which have a very inferior respiration. Thus, Dr. Martine found that the thermometer rose to 97° in a hive of bees.(1) A greater result was obtained by Reaumur.(2) Huber says that the heat of a large hive is upwards of 90° Fh.(3) Mr. Hunter found the thermometer raised to 93° and 98° in a hive, in spring,—to 104° in summer,—to 82° when the air was at 40°, and to 73° in winter.(4) Newport's observations have been somewhat different; but he has ascertained that the law established by Dr. Edwards, or rather illustrated by him, that young animals have a more feeble power of generating heat than the mature, is equally applicable to insects.(5) He also found that the power of generating heat is exalted during their breeding season,(6)—though it might have been more difficult to ascertain that the amount of heat evolved is in proportion to the quantity of air respired. This phenomenon, we shall see, corresponds with our observations upon vegetables. This observer states another fact, still more important to our views. He found that the temperature of an insect, which was similar to that of the surrounding air, suddenly rose 20° Fh. when irritated, or became active from any cause. So precise are the laws of nature, that upon this simple fact alone it would be safe to found a general induction as to the dependence of animal heat upon the forces of life. Nor is this principle in the least invalidated by the various modifications which occur, till we meet with some analogy in the cold-blooded animals, as it respects the law of slow communication of heat, in the inorganic world. For, however frogs, &c. may be liable to the operation of that law, the principle is within themselves that is necessary to all the emergencies in relation to the generation

(1) Essays Med. and Philosoph. 1740, p. 331.
(2) Hist. Nat. des Insectes, t. v. p. 360. (3) Mém. sur les Abeilles, t. i. p. 305.
(4) Lectures on the Principles of Surgery, p. 77.
(5) On the Temp. of Insects, &c. 1837.
(6) Proceedings of the Royal Society, No. 28, June 15, 1837.

of heat. And, since their organic processes are as well carried on at low as at higher temperatures, and the latter is no special embarrassment, we have not only a proof that heat is comparatively unimportant to the cold-blooded race, and of a corresponding modification of the calorific function, but an elegant demonstration that Unity of Design is never at fault; since, these animals being greatly abstracted from the uses of caloric which obtain in the warm-blooded race, nature has left them, in a measure, to the laws of caloric as they operate in the inorganic world. (See Vol. I. p. 67.)

Dr. Buckland has been often commended for his philosophy in deducing the existence of light, at the era of the trilobite, (that *first* of created animals according to the geologists,)(1) from the discovery of its *eyes*. This was well,—and perhaps a stretch of philosophy for one who may be supposed, without impugning his genius or knowledge, to be, like the chemists, but imperfectly acquainted with the science of physiology. It would have been, however, still better, had not Dr. Buckland afterwards exploded the light, and substituted "darkness," and an "atmosphere of dense vapours." And better still would it have been, had he not placed these ancient animals, and the vegetables which compose the great coal formations, "exactly organized like others of our own day,"(2) in an atmospheric temperature, that would, within the tropics, solidify an egg in three minutes.

This is the plain statement of the important subject before us. Should a physiologist, who might have first discovered the fragment of a solitary palm leaf, organized like a leaf of the existing day, down in a *granite* rock within the polar regions, and another in a similar rock within the tropics, have inferred the existence of a temperature of 215° Fh. within the tropics, to explain the appearance of the leaf in the northern regions; or, should he have denied the existence of the same light and the same atmosphere as now prevail, to rear up some hypothesis to explain other ill-digested facts, he would be held guilty by his compeers of a flagrant violation of three of the most fundamental laws of nature; and, as a physiologist, would be considered in the highest degree visionary.

Whatever may appear to conflict with the laws that relate to organized matter must be laid aside for a farther development of facts. Better derange "the music of the spheres," than disturb

(1) See Bakewell's Geology, p. 24, 1839. (2) Buckland's Geology, ch. 2, &c.

the fundamental laws of living organized matter. No philosophy can stand it for a day, that comes in conflict with the principle which we have fairly, though strongly, expressed. It has been often attempted; but the assailants fell by the recoil of their own weapons.

The subject which we have thus far investigated possesses peculiar claims upon the physiologist, and presses him to the rescue. It is one of no trifling import, since, more than any other, it has been employed to demolish the most stupendous and sublime system of nature. If the forces of chemistry were the foundation of animal heat, it would be a fair analogical induction that every other vital process is equally so, and the science of life would be condemned to the laboratory, and medicine would be a burlesque upon the human mind.

In examining the merits of this subject, we have found it most useful, as is our custom, to take with us the experiments, the observations, and conclusions of one or more philosophers who differ from ourselves. If the right, therefore, be at any time upon our side, we get rid of a formidable barrier which is constituted by the facts and arguments of powerful opponents; especially when we may convert their facts to our own views of the truth. In this essay we have taken, particularly, Dr. Edwards as our companion; since we have thought it important to remove the difficulties which his genius has created. Nor is it a special object with Dr. Edwards, in his work, to establish the chemical hypothesis of animal heat. He aimed at practical results from his experiments, and often reasons as a vitalist. We have therefore mainly regarded those experiments as they affect the chemical doctrine,—but little in their practical relations. The experiments, too, are of the greater value, since they have no favourite hypothesis in view, however they may have for their secondary object the chemical theory whenever difficulties may not require the substitution of the vital. (See our Vol. I. p. 52.)

SECTION IV.

We have seen that Dr. Edwards, unlike most of the chemical philosophers who have treated of animal heat, calls to his aid the "constitution" of animals to explain certain anomalies which

defy the chemical hypothesis. We hear much about the "power of the system to generate heat," without being let into the secret in what that constitution, and that power, consist. To allow that the forces of life have a large and uniform share in the generation of animal heat, would make a repulsive medley, in its connection with the chemical hypothesis. Now that "constitution," and that "power," are something more than ideal; something different from the organized structure; for, in the latter case, many variable phenomena, in adults, proceed from unvarying conditions of structure. Let us inquire, then, a little farther into the nature of "constitution," and the "power of generating heat according to the constitution."

We have not specially connected with the consideration of this subject those influences which are said to arise, under particular circumstances, from the relative proportions of air that may be actually consumed. These cases, however, come under the general rule. Where less air is consumed, if it be owing to the youth of an animal, it depends on constitution, and it is the *nature* of the *constitution*, not the proportion of air, which makes the difference in the ability to maintain a uniform temperature when cold is applied. But, if young animals consume less air than adults, it is sufficient, in a temperate air, to maintain their naturally high temperature; and when cold is applied, the acceleration of respiration far beyond that of adults under similar circumstances, should at least protect them against a decline of temperature to that nearly of the surrounding medium, were there the slightest foundation for the chemical hypothesis.

Just so is it with all the varying conditions of animal heat. In health, the varieties are owing to peculiarities in the natural condition of the vital properties; in disease, they arise, like all the other changes, from morbid alterations of those properties; and, if the blood sustain any want of its proper influences from defect of respiration, this will contribute towards the modifications of temperature, in the same way that it affects the other results of life, and, we apprehend, in no other.

Although Dr. Edwards derives some illustrations regarding the connections of the phenomena of animal heat with respiration, from certain morbid conditions of the body, as in asphyxia from carbonic acid, syncope, the cold stage of intermittents, &c., yet it is manifest that he looks upon disease as supplying facts which it is prudent not to investigate. "The question now is,"

he says, "what is the influence of the respiratory movements on the temperature of the body, when they are raised beyond the rate of health? We cannot answer this inquiry by observations on the sick. *The circumstances are then too complicated to admit of our deriving conclusions from them.*" (1)

In this conclusion we do not at all agree. It is an unwarrantable abandonment of nature for the contrivances of art. They are conditions, which, above all others, give us a clue to the true philosophy. The vital forces are altered by disease, and with them there is a change in all the phenomena and results, of which the modifications of animal heat are one. Hence, it appears to us, that a very obvious "conclusion" may be deduced.

In the experiments of reanimating the torpid hibernating animals in an atmospheric temperature of 3.7° Fh. below the freezing point, the temperature of a bat rose from 39° to 80.6° Fh.; that of a hedge-hog, from 37.4° to 89.6° Fh.; that of a dormouse from 37.4° to 97° Fh. Upon these experiments Dr. Edwards remarks, that "the external temperature had nothing to do with the restoration of the animal heat." This was plain enough; but not so clear our author's induction of the fundamental law that they "thereby show that the increased respiratory movements and the restoration of heat stand related as cause and effect."

But external heat, as we have seen, also accelerates the respiratory movements of vertebrated animals, — the hibernating, as well as others.

"It is a general phenomenon," says Dr. Edwards. "We then extend our relations with the air, and increase its vivifying influence. The respiratory movements become more rapid or more extensive, and thus more air comes in contact with the lungs in a given time, and reanimates what the heat depresses. This increase of respiratory movements is necessary to counteract, at least for a time, the effects of external temperature."

We have already examined the conflicting nature of the foregoing facts, and have shown upon what common principle the temperature is exalted in the former, and either depressed, as in fever, or maintained, when natural, at nearly a uniform standard, in adults. Again:

"Here, then, are several cases," (alluding to a series of experiments,) "in which the *acceleration* of respiration above the type of health may have a sensible effect upon animal heat. In the *first*, the temperature of the body *falls* under the influence of the cooling cause; but, by the *reaction* in question,

(1) Op. cit. Part 4, c. 10.

it *rises* a little, without however, being restored, and may afterwards *fall lower*, presenting fluctuations. In the *second*, it *diminishes*, and afterwards *returns to the point of departure*. Lastly, in the *third*, it does *not fall*, and can not only support itself, but even *rise* above what it was at first."

"There are individuals among hibernating animals which do not become torpid in the season of hibernation; a fact frequently observed in the *domestic* state. It is sufficient in this case, that *their power of producing* heat should be increased; a power, in all animals, susceptible of varying between *very distant limits*. This change can be even produced *at pleasure*, in some, by *suitable food*, and a graduated temperature."

"When the faculty of evolving heat is not the same, the *vitality* will be different. First, the relation to the external temperature will be changed. The need of warmth and the power of supporting cold cannot be the same where the *internal source* of heat has not the same activity."

"A potent chill," in a particular case of exposure to cold water, "acted on the *faculty* of *generating heat*, producing a sensible diminution in it, greatly exceeding in duration the length of time in which the cold was applied." "The *calorific function* had not acquired all its *lost power*."

"Warm-blooded animals may be divided into two classes, in regard to the influence of the seasons; viz. those whose constitution is perfectly in harmony with the climate, and those whose constitutions are not adapted to it. The first undergo changes corresponding to the season, which allow them the free use of their powers, and that enjoyment of life which constitutes health. According as the temperature falls, their *internal source* of heat increases, until it attains its maximum in winter, and afterwards declines with the elevation and duration of the external temperature. Here, then, is a *new element* which should enter into the explanation of the uniformity of animal temperature." "But the system only acquires this power of accommodating itself to the external temperature with the slow progress of the seasons. In summer, a degree of cold, which we bear in winter, would take the body, as it were, by surprise and unprepared. The *power* of producing heat being thus reduced to its minimum, the loss would be insufficiently repaired. In this respect, *our states in summer and winter differ in the same manner*, though not in the same degree as young animals differ from adults. In the former, the increase of the power of producing heat takes place through the *progress of organization* while under the influence of a mild temperature; in the latter, by the influence of cold in degree and duration suited to their constitution."

"These changes, however, do not take place in all animals. There are some whose constitution is not adapted to so great a range of external temperature. The cold which they can sustain without inconvenience is much less, because they have not the *same resources* for *repairing* the loss of heat. When reduced below this limit, a fall of temperature produces an effect the reverse of what has been above described. Instead of increasing the production of heat, it diminishes it." (1)

The work from which we have thus quoted abounds with similar practical facts, all of which concur in referring the gene-

(1) Dr. Edwards, Op. cit. Part 4. ch. 2, 3, 5, 9, 10, 17.

ration of heat to the vital forces, whilst they unequivocally demonstrate the existence of such forces. There is no possibility of explaining them by any other facts or analogies, and, therefore, by none of the laws which appertain to the inorganic world. They are perfectly *sui generis;* completely isolated from every phenomenon in the world of dead matter.

As to the vicissitudes that are occurring in the relative power of evolving heat in summer and winter, they have manifestly not even an indirect connection with respiration, but depend entirely upon different modifications of the vital forces. Respiration is even more accelerated in summer than in winter, whilst it is said the power of generating heat is diminished at the former season. (1)

In respect to the natural differences in constitution that are denoted by apparently contradictory facts in relation to animal heat, they are as clearly constituted by natural modifications of the same forces, which are as much, or more influenced by other causes than by respiration; whose power of evolving heat in young animals is greatly and rapidly depressed by the operation of cold, notwithstanding the respiration is accelerated during the first stages of the decline of temperature; but which, again, as the same animals advance in life, acquire the power of completely resisting the same cause without the former acceleration

(1) We think that this can only be admitted in a qualified sense. There is actually more heat generated in summer than in winter under equal and natural circumstances of atmospheric temperature, and especially when the temperature is low in the latter season. The difference arises from greater vascular excitement during the heat of summer; and the uniformity of animal heat is then maintained in part by increased evaporation from the skin. De la Roche, and others, by placing animals in a heated atmosphere charged with moisture, raised their temperature several degrees. On the other hand, the vital forces are so modified by the stimulus of heat, that when cold is suddenly applied, there is a lesser evolution of heat than under equal circumstances in winter. The difference, therefore, is constituted by a difference in the susceptibilities of the vital forces to the action of different causes, at different seasons, from which result variable phenomena in respect to animal heat. It does not, therefore, appear to us proper to say that the power of generating heat is greater in winter than in summer; especially since the generation of heat is actually greater in the latter season, nor will atmospheric heat develope more animal heat at the former period. *Power* is wholly relative in these cases, and is only a contingent result of the operation of different stimuli upon the forces of life, and varies at different seasons according to the nature of those stimuli. It is increased by the heat of summer whilst heat is in operation; but more greatly depressed at that season by the sudden application of cold, than in winter.

These phenomena are insusceptible of explanation but through the medium of the vital forces.

of the respiratory movements; "the animals thus passing from the state of cold-blooded to that of the warm-blooded," whilst in the hibernating mammalia, diminution of heat still goes on although respiration have come to a stand; or, when the cold becomes intense, is carried to its highest pitch by the very cause which had produced its great decline; which maintain an almost unaltered state of heat when the respiratory movements are greatly accelerated by external heat, and resisting equally the heat of the surrounding medium; which actually abate the exalted heat of fever; which are so influenced by season, that their power of producing heat is said to be less when its production is greatest; which power "may be varied, in some, by suitable food and a graduated temperature;" which "is generally diminished in natural sleep, though modifications occur which change the relation;" which is so modified in the cholera asphyxia, that the temperature may greatly fail whilst respiration is accelerated, and the lungs free from congestion; or, is undiminished in asphyxia from carbonic acid, "when the respiratory movements are no longer seen;" (1) or, may attain, as in apoplexy, preternatural vigour after respiration and circulation have entirely ceased.

"Constitution," then, and the "power of generating heat," manifestly relate to the vital forces, and to nothing else. The united operation of these powers, through their instruments of action, results in the elaboration of bile, gastric juice, heat, &c., from the blood. That particular determination by which they eliminate heat, in all parts of the body, may be called a law, though it is but the joint result of the vital powers, concurring in a certain manner to a specific effect. The result is variously affected by climate, season, the quality and quantity of food, stimulants and sedatives, cold or warm air applied externally or to the lungs, by morbific agents, and other causes; or, as the vital forces happen to sustain peculiarities in relation to individuals, age, &c., so will the generation of heat be modified when respiration is exactly the same; and along with those modifications of heat are variations, more or less coincident, of other products. The causes are obvious from the effects. The former are few and simple; the latter are diversified without end.

Most of the reasoning which abounds in authors who believe animal heat to depend specifically upon respiration, or the result

(1) Portal says that the heat has been known to remain very high in these cases, as in apoplexy, for many hours after death. — *Sur l'Apop.*

of a chemical process, consists in reconciling difficulties by referring them to the vital powers, and sometimes to the entire exclusion of the chemical hypothesis. True, they do not say *vital powers.* They would otherwise be non-conformists. They speak of "constitution,"—"the power of evolving heat,"—yet turn into ridicule the only true philosophy, and the only possible thing which they themselves can mean. If they hazard the "term *vitality*," it "is employed for the want of a better," but "without any connection with the mystification which sometimes attends its use;" whilst others, like Dr. Elliotson, can see nothing in "animal heat," "but a peculiar *state* only;" and here, as in the case of "vitality," Dr. E. "adopts the common language in speaking of animal heat," to make the subject intelligible. (Vol. I. p. 49.)

It is from the blood, like all other animal products, that heat is derived. And since decarbonization, and, perhaps, an absorption of oxygen, is indispensable to the healthy performance of all other functions, it is doubtless important to the generation of heat; though manifestly less so in the latter instance, since we see the evolution of heat sometimes going on when respiration is nearly, or quite extinct; whilst, in the cold-blooded animals it exerts but little, if any, influence upon temperature. Decarbonization of the blood, and probably the absorption of oxygen, are among the numerous processes by which its vivification is perfected, and by which it is prepared for an elaboration of the various animal products, and in animals of a certain constitution for the evolution of heat. When respiration ceases, all the most important functions immediately fail; but it is remarkable that the evolution of heat appears to be the *very last.*

We conclude, therefore, that the elaboration of animal heat, and all other secretions, are on a par in regard to principle. It is true, a certain proportion of latent heat may be extricated by the conversion of blood into the solid parts. But this would be counterbalanced by a corresponding change of the solids, particle for particle, into fluids. This appears to us to be fatal to a late doctrine which imputes animal heat to this cause; as well, also to the condensation of gases.(1) Besides, what becomes of the principle of condensation where the temperature rises after apparent death? (P. 31.) Where is oxygen gas?

(1) It is said by an able writer, that "there is much evidence to prove that animal heat is principally maintained by the chemical changes constantly taking place in the tissues." (*a*)

(*a*) British and Foreign Med. Rev., Vol. 5, p. 105, 1838.

SECTION V.

We shall now state some experiments which we have made for the purpose of advancing this inquiry.

It is well known that hibernating animals may be roused from their state of torpor by simple mechanical irritation. We had projected an experiment of rousing them by means of electricity, whilst they were surrounded by carbonic and hydrogen gases at a temperature below the subdued heat of the animals. But having failed of obtaining such an animal, we substituted new-born puppies. A rapid succession of strong sparks were passed through the body from head to tail, and although there was a sensible elevation of the thermometer, we do not rely upon the result. The experiment should be tried upon a hibernating animal, which should be in a state of *perfect* torpor. If a marmot, in a torpid state at a temperature of 30° or 40° Fh. be deprived of atmospheric air, or even enclosed in a receiver with carbonic acid, at a much higher temperature, (53°) he will have sustained no injury, "nor," according to Edwards, "have shown any sign of uneasiness at the end of four hours." (1) Spallanzani makes the same statement; but complete torpor was maintained. He then established a slight respiration, and again put the animal into a jar of carbonic acid, when it perished, as we infer, in a short time. (2)

Mr. Hunter's experiments on the temperature of trees have appeared to us decisive of the philosophy of the production of heat by organic matter. Some doubt, however, has been thrown upon their accuracy; it having been assumed that the interior of an object so large as a tree would be more or less protected against the cold by the exterior. (3) We, therefore, undertook a series of experiments of the foregoing nature. We selected periods when vegetable life was becoming progressively active, and before the development of leaves.

On the 9th of April, 1839, we repaired to a forest in New-

(1) Dr. Edwards, Op. cit. p. 137. (2) Sulla Respir. Mem. ter. t. v. s. 11, 12.
(3) See London Med. and Phys. Journ. No. 271, p. 265.

Jersey, provided with very delicate thermometers, of Fahrenheit's scale, constructed for our object. The bulbs were no larger than the stem, the range of the mercury extensive, and the degrees marked upon the glass. The stems filled exactly the bore of a small spiral augur; and when the glass was introduced, the air was excluded by applying a silk handkerchief around the hole. The perforations were all made on the northern side of the trees. Fifteen minutes, at least, were allowed for the subsidence of heat that arose from the friction of the perforator; and the thermometer was generally reapplied at different intervals afterwards. The perforations were made about four feet above the ground, and the diameters of the trees were ascertained at this part. When the diameter was five inches, or more, the perforations were made to the depth of 2½ inches. When the diameter was less than five inches, the thermometer was introduced as far as the centre of the tree. All the observations are given that were made.

Range of the thermometer in the shade, during the observations, which lasted six hours, from 38° to 52°. Near freezing at sunrise.

A dead, upright, dry tree was selected as a standard of comparison. Its diameter was 12 inches. The temperature of this tree, at the close of our observations, was 45° at the centre, and in all other parts.

Juglans squamosa,	diameter	10	inches,	48°	Buds slightly enlarging.
Do. do.	"	6	"	49°	do.
Fagus sylvatica,	"	10	"	49°	Buds swelling.
Quercus tinctoria,	"	7	"	49°	No budding.
Castanea Americana,	"	12	"	50°	do.
Betula nigra,	"	4	"	51°	Flowering.
Salix Babylonica,	"	18	"	53°	Buds unfolded.
Do. do.	"	18	"	53°	do.
Pinus Canadensis,	"	18	"	54°	
Platanus occidentalis,	"	18	"	50°	No budding.
Do. do.	"	6	"	54°	do.
Do. do.	"	4	"	55°	do.
Juniperus Virginiana,	"	4	"	55°	
Robina pseudacacia,	"	3	"	62°	do.
Populus lævigata,	"	4	"	62°	In bloom.
Do. do.	"	4	"	64°	do.
Do. do.	"	3	"	63°	do.

Populus lævigata,	diameter 3 inches,	65°	In bloom.	
Do. do.	" 2 "	67°	do.	
Do. do.	" 1¾ "	68°	do.	

Believing that if our doctrine of the generation of animal heat were correct, we should find an elevation of vegetable heat as the warmth of the season increased, and the energy of vegetable life became more exalted, on the 19th of the same April we made another visit to a forest upon the island of New-York.

The same method was observed as on the preceding occasion, excepting in the perforation, which was always two inches when the diameter exceeded four inches; otherwise it penetrated to the centre. Weather cold, and rainy, since last observation.

Range of the thermometer in the shade, during the observations, which lasted five hours, from 40° to 65°.

Temperature of two dead, dry, upright birch trees, one eight inches in diameter, the other six inches, at end of observation 60° in all their parts. Temperature of the earth six inches below surface 47° in shade, at close of observation.

Betula nigra,	diameter 15 inches,	54°	Buds swelling.	
Platanus occidentalis,	" 6 "	59°	do.	
Quercus virens,	" 8 "	62°	do.	
Do. do.	" 2½ "	73°	Buds much more advanced.	
Do. tinctoria,	" 18 "	65°	Buds swelling.	
Do. do.	" 6 "	66°	do.	
Juniperus Virginiana,	" 5 "	64°		
Do. do.	" 2 "	79°		
Acer rubrum,	" 12 "	65°	In bloom.	
Castanea Americana,	" 4 "	66°	Buds swelling.	
Cornus Florida,	" 2 "	68°	Flower-buds advancing; no leaves.	
Fagus sylvatica,	" 12 "	68°	Buds opening.	
Juglans alba,	" 4 "	75°	Buds swelling.	
Do. do.	" 1 "	83°	Buds larger.	
Do. do.	" ¾ "	82°	Buds opening.	

It is abundantly manifest from the foregoing observations that vegetables possess a vital power of generating heat, according to the activity of their organic forces; and we carry the analogy to the animal kingdom. The temperature was not influenced by that of the earth, as seen by the preceding statement. The heat of the latter, however, was not ascertained at the first ob-

servation. It appears, also, that "the power of generating heat" is greatest in proportion to the youth of trees. This remarkable fact is not only especially indicative of the vital agencies in the generation of vegetable heat, but is worthy of notice on account of its opposition to what obtains in the animal kingdom in respect to age. It corresponds, however, with observations upon herbaceous plants, where nutrition is more rapid. The difference depends upon the relative difference in organization at the corresponding periods of life.

We have made no observations upon the temperature of herbaceous plants, these having been already ample, and superseding, indeed, the necessity of the foregoing. Senebier saw the thermometer rise from 79° to 143° Fh. when placed in the midst of a dozen spathes of the arum cordifolium at the time of opening the sheaths; and Huber, from 21° to 42° cent., the atmosphere having been, in either case, at the first mentioned degrees. (1)

(1) Op. cit. — See, also, Mayo's Physiol. p. 79; and Ellis on Respiration, p. 204.

PHILOSOPHY OF DIGESTION.

"Quicunque de natura humana amplius diserentes audire consucvlt, quam ejus ad medicinam pertinet, huic non est commodum hunc meum sermonem audiré." — HIPPOCRATES *de Nat. Hom.*

"The opinions most generally received in physiology, those consecrated by the assent of all celebrated authors, often rest upon very uncertain foundations." "Examine all the physiological and all the pathological phenomena, and you will see that there is no one which cannot be ultimately referred to some one of the properties (vital) of which we have spoken." — BICHAT'S *Anat.* &c. (1)

"Digestion is very different from chemical solution, which is only a union of bodies by elective attraction, not a real change of the substances themselves, but of their properties. But, digestion is an assimilating process. It is a species of *generation*, two substances making a third; but the curious circumstance is its converting both vegetable and animal matter into the same kind of substance or compound, which no chemical process can effect."

"Those who took it up chemically being ignorant of the principles of the animal economy, have erroneously referred the operations of the animal machine to the laws of chemistry." — HUNTER'S *Observations on Digestion.*

"The changes which take place in the substances capable of giving nourishment, and therefore of being converted into the essential parts of the chyle, are totally different from those changes which take place any where but in the stomach, duodenum, and jejunum, when alive; therefore no experiment made any where, excepting in these intestines of the living animal, *can in the smallest degree influence the doctrine of digestion.*"

"Food, placed in all the chemical circumstances that can be conceived similar to those in which it is placed in a living stomach, will never be converted into chyle, but will undergo other changes totally different." — FORDYCE *on Digestion.*

"Spallanzani sort de l'estomac des animaux et du sien propre, pour former un estomac sur sa table." — *Considerations sur la Methode de l'Abbe* SPALLANZANI, *in Œuvres Spall.* (2)

"The application of principles requires more than simply the knowledge of the principle itself; and therefore those who cannot reason from analogy, or draw general conclusions from a few convincing facts, and who require to have every relative conclusion or inference proved by an experiment, however unnecessary or fatiguing to the reader, must be pleased with Spallanzani."

"We can have no very high idea of experiments made by gentlemen, (chemists,) and *priests*, who for want of anatomical knowledge, have not been able to pursue their reasoning even beyond the simple experiment itself."

"There are in nature's operations always two extremes; and the mind of man eagerly adopts that which accords with some principle *he is fond of, and with which he is best acquainted:* but the intermediate connections and gradations, being less striking, do not forcibly affect the superficial inquirer." — HUNTER'S *Observations on Digestion.*

"If from great Nature's, or our own abyss
Of thought, we would but snatch a certainty,
Perhaps mankind might find the path they miss —
But then 't would spoil much good philosophy.
One system eats another up, and this
Much as old Saturn ate his progeny." — BYRON.

(1) Vol. i. pp. 17, 445. — We use the American ed. of 1822; and the Parisian of 1813.

(2) T. ii. p. 334.

"The genius of Hunter long ago explained the objections to other theories of digestion. These have been turned into ridicule to smooth the way for hypotheses that have no better foundation."—CARSWELL *in Hebdom. de Med.* 1829.

"In the depths of Physiology subsists an invisible world."—Dr. HAKE *on the Organic Corpuscle.*

Like the forges of Vulcan, every part is animated, and nothing moves or is struck into existence but by the agency of the vital forces.—"*NATURÆ non hominis.*"

HAVING hitherto seen that the various processes of life, and all their results, whether healthy or morbid, with the single exception of digestion, depend exclusively upon powers which have no analogies in the inorganic world, but with which they hold an absolute hostility, we might safely conclude that the same powers are equally concerned in the initiatory step which prepares the material for all their subsequent operations. Yet, is this especially the ground upon which chemistry has reared its batteries, and from which it sends forth its artillery into the various dominions of the organic world. Yet, is it here, that vitality is exemplified in its most impressive and astonishing aspects, and where unequivocal demonstrations abound that fluids, as well as solids, are endowed with the principles of vital operations. Finally, it is here, especially, that nature has illustrated her distinction between the animate and the inanimate world, and established her chain of connection.

The chemical doctrine is thus laid down by one of the most able and illustrious in the field of science. It has also the merit of being set forth to illustrate "the Power and Wisdom of God."

"1st. The stomach," says Dr. Prout, "has the power of dissolving alimentary substances, or, at least, of bringing them to a semi-fluid state. This operation seems to be altogether chemical.

"2d. The stomach has, within certain limits, the power of changing into one another, the simple alimentary principles;" and "this part of the operation of the stomach, appears, like the reducing process, to be chemical; but not *so easy of accomplishment.* It may be termed the *converting* operation of the stomach.

"3d. The stomach must have, within certain limits, the power of *organizing* and *vitalizing* the different alimentary substances." "It is impossible to imagine, that this organizing agency of the stomach can be chemical. *The agency is vital, and its nature is completely unknown.*" (1)

We would first inquire, although not very relative to our sub-

(1) Chemistry, Meteorology, and the Function of Digestion, considered with reference to Natural Theology, b. iii. c. 3.

ject, how the foregoing account of the process of digestion enlarges our views of the "Power and Wisdom of God," any more than the ordinary process of putrefaction? Nor is this at all an extravagant comparison; for it has been seriously affirmed by the chemists, that the putrefaction of a muscle results in a substance resembling chyme.(1) It is certainly true, also, that the same Power was necessary to establish those agents upon which putrefaction depends, as the more exalted powers of life; and the same Omnipotence is as positively manifested in an insect as in man. But the finite comprehension of the human mind is just as much enlightened as to the Power of Omnipotence by the contemplation of an ascending series of principles upon which the various phenomena of nature depend, as by tracing the analogous illustrations of Power from the simplest elements of matter up to the most perfect of sensible existences. It appears to us, indeed, that it is rather the laws, than the mechanism of nature, which most excite our adoration of its Author. There is more to admire in the operation of the principles which unite into atmospheric air and nitric acid the elements of which they are composed, than in the elements or the combinations themselves,—more in "the music of the spheres" than in the vast sublimity of the individual objects,—more in the powers which carry on the functions of organized matter, than in the organization itself. What were nature without its animation? But this is nothing more than an emanation, in connection with matter, from the forces which we are considering.

We believe the distinguished author whom we have quoted expresses the prevailing doctrine; which makes the reluctant admission, that the chemical product is in some mysterious way imbued with the properties of life. We say reluctantly, (Vol. I. p. 48,) because of the doubt which is raised by our able author, as well as by most others, in simultaneously adverting to the chemical process, and especially from the declaration that "the nature of the vital agency is completely unknown." Now the nature of this agency is no more concealed than that of the chemical forces by which digestion is supposed to be wholly performed. We know nothing more of the latter than what we gather from their effects; and since the results of the vital powers are incomparably more diversified, we should be even better informed of their nature, and more certainly assured, as we have otherwise

(1) Dr. Davy, ut cit.

endeavoured to show, of their existence as something distinct from the matter which they animate. See Essay on the *Vital Powers.*

Since, therefore, it is conceded by philosophers who defend, in extenso, the chemical hypothesis of life, that there may be *something* appertaining to the stomach totally distinct from the chemical powers, and which is capable of imbuing the chyme with *vitality* and *organization*, it is quite a philosophical conclusion, that this *vital something* has an important agency in *preparing* the material for the admitted exercise upon it of its vivifying or organizing power. Nor can we see any valid objection to the supposition that this vitalizing power, which so far transcends the chemical forces, may not be fully adequate to any transmutations the food may undergo; and this induction is the more corroborated by the consideration that matter already in an organic state must be better fitted for the process of vivification, than it can possibly be after its elements are broken up and recombined by forces with which those of life are in absolute opposition. Besides, the vitality of the gastric fluid, or the vital influence of the stomach itself, (') being fully admitted, and even capable of organizing the food anew, this, in itself, should protect the alimentary matter against any chemical agencies which have been supposed to operate. That this counteracting power, indeed, prevails to the full extent which we allege, appears to be rendered certain by the ordinary absence of any variety of chemical changes where numerous substances are mixed together in the stomach, and these often possessing strong affinities for each other, and their operation promoted by a high temperature. It cannot surely be that any feeble acidity of the gastric juice should not only surpass those affinities, and counteract the admitted

(1) Although the gastric juice be the principal agent in the process of digestion, we know of no good objection why the stomach itself may not cöoperate in a direct manner. It is an untenable ground that we do not understand the modus operandi. (*a*) The same, as we have said, may be affirmed of every effect. It is not here, as in chemistry, where the forces which operate out of the living body are the subjects of more precise demonstration; nor do the latter supply any analogies from which we may reason to the results of organized matter. In the latter case, however, we have an apparently strong analogy in the maintenance of vitality in the reduced alimentary matter, or in blood, by its simple contact with the parietes of a large blood-vessel, or even with the cellular tissue. But that chemical decomposition shall be thus delayed, the substance protected must possess vitality, since a corresponding effect is not exerted upon dead animal muscle. This, too, we hold to be another proof of the vitality of the blood.

(*a*) See Bostock's Physiology, vol. ii., c 10, § 4, p. 409.

vital influences, but resolve the heterogeneous mass into a homogeneous substance. Chemistry must here be consistent with itself, and not renounce, for the sake of hypothesis, those precise laws by which in its legitimate pursuit it lays open, with astonishing exactness, what had appeared the arcana of nature. Here, too, is presented an instance in which it must be necessarily assumed that the properties of life and the forces of chemistry must act together in concert in converting dead into living matter — one destroying and the other vitalizing; whilst the assumption is contradicted by all that is known of the relations of these forces to each other. (Vol. I. p. 32.)

Nor do we see with what reason it can be maintained that more is necessary to the various functions and results of living matter than the vital properties; since it is admitted by the strongest advocates of the chemical hypothesis, that it will not explain a single phenomenon of life; or, in the language of Dr. Prout,

"There is no reason why the chemical changes of organization should result from the mechanical arrangements by which they are accomplished, neither is there the *slightest* reason, why the mechanical arrangements in the formation of organized beings should lead to the chemical changes of which they are the instruments." (1)

It will be also readily conceded to us, that we make no unusual demand upon philosophy, in thus attempting to simplify causes, where one is perfectly adequate; and especially where it is admitted that all the others are wholly inadequate. (2)

Let us now attempt an analysis of our subject; beginning, after the philosophical manner of Dr. Prout, with a simple substance submitted to the gastric action. Here we think it will appear by the showing of the chemist himself, that physical agencies have had no connection with the change, and if not here, in no other instance. This conclusion will follow more and more irresistibly as we multiply the varieties of food, and, therefore, the chances of discordant results.

The forces of chemistry, in the case supposed, would result in the separation and recombination of the elements of albumen, and the product, therefore, should be very different from that sub-

(1) Ut cit. We adhere to Dr. Prout, inasmuch as he is the highest admitted authority in organic chemistry, and one of its most candid and lucid expounders. (Vol. I. p. 36 — 42.)

(2) "There is an observation," says Fordyce, "I beg leave to make with regard to a mode of reasoning which has been too often adopted in physiology and medicine; to wit, that it has frequently been thought sufficient to prove that a thing was not impossible in order to ascertain its actually being true." — On Digestion, p. 104.

stance. But what says the chemist? "Through all the *apparent* changes, the albumen has undergone no *real* change. What was introduced into the stomach as albumen is still albumen in the chyme; at least chemists have pronounced it to be so." (1) (See Vol. I. pp. 529, 575—577, *notes*.) The same affirmation is made by Mr. Bird, who considers the albumen and the gastric product chemically identical, and yet different from each other, as do also Tiedemann, and Gmelin, and others. Here, too, should be connected the statement as to the chemical identity of albumen and fibrin; (2) which, however, is only a very limited part of a long series of close affinities which are admitted to prevail betwixt the foregoing substances, and a variety of other organic products, and which is wholly incompatible with the absolute distinction that prevails among the combinations of chemistry where the elements are alike, but the proportions a little varied.

But, if some vital change have been exerted upon the physical character of albumen, it should appear under a new aspect; and we have it admitted by Dr. Prout, that "it has assumed an appearance altogether different from albumen." The chemist, however, may find the same elements, in their proper proportions, which compose albumen, whilst they shall have undergone new combinations amongst each other, and in ratios which differ from chemical compounds, in virtue of the vital influences of the gastric juice. This, we apprehend, is the great peculiarity appertaining to the vital actions, whilst chemistry, allowing its accuracy, has not looked beyond the several elements of which an organic substance may be composed. (3) Again, it is said by Dr. Prout, that another

(1) Dr. Prout, ut. cit.—It is proper, however, to say, that the suppositious qualification is made, that the "albumen has merely become chemically combined with a portion of water." But is this compatible with the doctrine, that the "reducing" agent of food is chlorine, muriatic, or some other acid? Is it consistent with the supposition that other substances are completely decompounded by the same agent? Finally, however, we have the candid admission, that chemistry is not only utterly unable to supply the rationale of the process, but the whole ground is abandoned and consigned to the "vital agency, whose nature is completely unknown;" for thus, our author: "In no instance do we appear able to invert the process, or to complete an organic compound, by again separating the water; though *such change*, within certain limits, seems to be to the ORGANIC AGENT JUST AS EASY AS THE REDUCING PROCESS."—(*See our vol.* i. pp. 56—60, 66.)

(2) See Mr. Bird in Guy's Hospital Reports, No. 6, 1838, p. 48. Also, M. Denis, Nouv. Expèr. sur le Sang. in Archiv. Gén. de Méd. Fev. 1838.

(3) Dr. Prout supposes that the albumen of the blood is converted into gelatine, by giving off a portion of its carbon, and that this is the original source of the car-

"Of the chief materials from which chyle is formed, namely, the oleaginous principle, may be considered to be already fitted for the purposes of the animal economy, without undergoing any *essential* change in composition."

True, and therefore no chemical change; but no one doubts that the resulting chyme "is altogether different in appearance." Besides, we again ask why, if other less assimilated substances be subjected to great changes in virtue of the supposed chemical solvent of the gastric juice, the oleaginous principle should be so little liable to its action? Is it from anything like intelligence on the part of the agent? If, however, on the other hand, our vital forces are the agents in digestion, the changes should be slight in the foregoing substances, since their elements are already combined according to the laws of those forces. This appears to us an important argument, and to be entitled to consideration. Chemical agents know no distinction between substances which may have been more or less assimilated to the animal organization; but they alike decompose the elements of each, though animal substances more readily than vegetable, or inorganic, and with greater resulting differences. In our "Vital Powers," we have stated many analogies where eliminations from the blood are nearly identical in their elements, but where it is manifest they have undergone different combinations which chemistry cannot explain. (Vol. I. p. 56, &c.)

We find, therefore, in the incipient change of dead into living matter a full display of those powers which operate in the most elaborate organization, and an equal exclusion of the forces which appertain to dead matter. And here may we stop, for a moment, to contemplate this wonderful exhibition of the "Power and Wisdom of God," at the very threshold of life; this sanctuary, where neither inorganic matter, nor the forces which it obeys, can gain admittance; where the line of separation begins abruptly betwixt dead and living matter, but whose analogies are preserved in the conversion of one into the other, through new modes of combining the same elements; and doubtless our admiration would increase, should we mount along the entire functions of assimilation, and find, at each step of the ascending series, that the whole agency has been committed to forces that

bonic acid. (Our p. 16.) But it happens that as much carbonaceous matter is evolved during the process of starvation, as after feasting. Besides, scarcely any two analyses agree as to the composition of albumen and gelatin, — especially in their relations to each other. And so we ever find it; — one chemical fact being in collision with another. (P. 14.)

have no existence in the inorganic world; that the whole is the harmonious result of a principle which may form an intermediate link betwixt spirit and matter; (Vital Powers, Sec. 3;) and that there is no power within our control by which we can determine the nature of the changes. Casting a glance at the vegetable world, we find the connection continued by other analogous links with elementary matter itself; but here, as in the higher department of nature, the line of separation is equally defined, however low in the scale of analogy may be the properties of life which have their beginning in vegetable organization. We shall say nothing more here of the various facts appertaining to the two organized kingdoms, (see Vol. I. pp. 68, 594,) which illustrate this subject, excepting to remark that the researches of physiologists have shown that some vegetables are evidently provided with an apparatus corresponding to the animal stomach, which last is said to prevail universally. However we may scrutinize the process of digestion in either of the great kingdoms, we find that nature has provided intricate means for the purpose, and established analogies in their organization and phenomena that are unknown in the organic world. And, if we carry the analysis through the whole labyrinth of animal and vegetable creation, we meet, at every step, with so much Unity of Design, such gradual modifications of structure and functions, and all conspiring to a specific end, we must conclude that the whole are governed by forces peculiar to themselves.

But, notwithstanding the astonishing Unity of Design, and the gradation of analogies which connect together the most dissimilar of the systems which are designed for digestion, there are peculiarities attending each in every species of animals, which place at a remote distance the laws of chemistry.

"An alimentary canal is observed in every class of animals, and almost in every species, and its form and structure vary according to the situation of animals in the scale, or according to the kind of food on which they are destined to subsist, and the extent of elaboration it requires to undergo. The peculiarities presented by the digestive organs are, therefore, intimately connected with the diversities of form manifested by the organs of animal life, and with all the living habits and instincts of animals." (1) — (Vol. I. p. 36, *note.*)

We see, also, an extension of this principle in relation to certain periods of life. In ruminating animals, for instance, the fourth stomach is alone employed during lactation. And yet, with all this variety, there is but one common result, one homo-

(1) Grant's Outlines of Comparative Anatomy, p. 306.

geneous chyle. The same principles, the same vital impulses, which ultimately develope and call into action the three first of the stomachs of the ruminantia, appear to be not less concerned in the consummation of their final cause.

One of the most important arguments in favour of vital digestion consists in the remarkable endowments of the stomach, as manifested by its vital signs, and the sympathies which prevail between this organ and all other parts. These have been well expounded by Hunter, Abernethy, and many others, even by chemical physiologists. The final cause of this peculiar constitution of the stomach, this lavish supply of the forces of life, this subservience of other organs to its dominion, must be sought in its adaptation to the generation of a fluid that may bestow the first and most difficult act of vitalization upon dead matter. There would, also, have been something harsh and abrupt in nature, to have admitted into the recesses of her living organization mere dead matter. It is opposed to all analogy, and is, therefore, opposed to all reason. But, that a fluid should perform this astonishing office, this first and great step in the ascending series, it must possess in a high degree the forces of life. Mysterious as they may be represented, we must come at last to the admission of their existence. It is fair, then, to conclude that an organ destined for such a high function should possess the necessary means; and the best evidences in favour of this *a priori* inference are to be seen in its diversified manifestations of life. (See *Venous Congestion*, Sec. 8.) To deny the dependence of vital results upon the specific powers which we have hitherto considered, necessarily involves an exclusion of all the evidence by which we infer the existence of gravitation on the intangible properties of matter ; or as well may we doubt the reality of spirit, or the Maker of the eye, because it cannot see Him that made it.

We might go on with a variety of proof, derived from the chemical advocates of digestion, to show that the doctrine is founded upon indefensible premises. Thus, it is supposed that chlorine is uniformly present in the gastric juice, that it is the principal agent in the "reducing" process, and that "it mainly contributes towards effecting the union of the food with the water." But, besides the objections already stated, we shall see that this is an hypothesis erected upon another, since the probable exist-

ence of chlorine is assumed, whilst it is fully admitted that the foregoing union is only surmised. Again, however,

"We may close this section," says Dr. Prout, "by observing that there is strong reason to believe, that the *solvent power*, (chlorine) which we have described, or some power having a great resemblance to it, exists not only in the stomach, but *in every part of an animal body.*" "Before solid parts can be removed, they must be dissolved, digested in fact; and such solution, in many instances, is probably effected, as it is in digestion, by combining these solid parts with water." — *Ut. cit.*

Here the conjectural nature of the chemical theory is variously exhibited; whilst the foregoing rationale may be contrasted under a similar aspect with that of galvanism. Neither chlorine nor anything "having a great resemblance to it," nor the galvanic fluid have been detected in the blood or in the instruments of organic action, (Vol. I. p. 63,) whilst the objections we have already stated as to the want of analogies between the conversion of different species of food, which should obtain were chlorine the "solvent power," are equally applicable in the present instance. And farther on, (c. 4,) Dr. Prout argues with great force and effect, that the matter taken up by the absorbents is not wholly excrementitious any more than chyme; but that a proportion of it is again appropriated, and undergoes "a progressive organization" in the absorbent system, acquiring "a highly animalized character."

We have seen that the foregoing parallel is not sustained by any analogy between the supposed chemical agents of the gastric juice and the means which are concerned in the decomposition of the body, where the nature of any menstrua must be as various as that of the tissues, the organization, and the modifications of the vital properties, upon which the generation of the reducing substance depends. This is conformable to all analogies in organized matter; and since the chemical agents should, therefore, be very various, (as appears to be implied by Dr. Prout when he speaks of a substance "having a great resemblance to chlorine,") chemistry should not have wholly failed of detecting one or more of the number. Nor will it answer to assume, as is also done, that galvanism is a "reducing" agent in the process of vital decomposition, since this conjecture is opposed by the assumed analogy between the solvent power of the stomach, (or chlorine,) and that of the absorbents.

Still the analogy in the foregoing processes, as pointed out by

Dr. Prout, certainly exists; and the resemblance is so great, and so "strongly confirmed," as to induce him to apply the same term, digestion, to the function as performed by the absorbents and as it goes on in the stomach. We must, therefore, look for a strong analogy in the efficient causes, and this we may find, and only find, in the vital properties, whether acting through the medium of the solids, or the secretions to which they have been imparted.

The gastric juice being designed, as is admitted by chemists, for a vitalizing office, reason suggests that it must possess the necessary powers of vitality; and these we can easily imagine to exist in the gastric juice in greater development than in the blood itself, since it is the product of an organ endowed in the highest degree with the vital properties, and destined to make the first and most important impression upon dead matter. And what proves the vitality of the gastric juice, were it necessary to show the fact, is the endowment of the chyme with the properties of life; or, should the latter fact be questioned, we may say that its admission into the lacteals, and this almost exclusively and without stint, appears to us conclusive of its vital nature, since all dead matter is known to be more or less obnoxious to the same vessels, whilst it is everywhere incompatible with the living organization. By the same reasoning we infer the vitality of the blood, as we do of both from their resistance of chemical agencies. (See Vol. I. p. 642, *note*.)

There certainly can be no doubt, when the gastric juice is abstracted from the stomach, and divested of its vitalizing powers, it may exert direct chemical effects upon dead matter, and even upon a dead stomach; just as we shall soon see an artificial substance operate, which is supposed to be the gastric juice. This, however, is anything but digestion, as is sufficiently evinced by the general admission that the *vitalizing* influence is necessary to the process.

We confess that we were a little staggered in our views of digestion by the Medico-Chirurgical:

"The transcendental vitalists," says this Review, "are loth to admit the operation of chemical agents at all, and would seem to consider it derogatory to suppose that any changes, save the subtle ones effected by the powers of life, are worked upon the aliment." "The *vital principle*, whatever it may be, incessantly makes use of chemical and mechanical agents for its purposes; and it is no more degrading to it to employ an acid liquid, and a triturating

process in order to digest the aliment, than it was to have recourse to bony levers, cartilaginous pulleys, and tendinous ropes," &c. (1)

Will our distinguished Review maintain that "the vital principle incessantly makes use of chemical agents for its purposes"? Is not this "a begging of the question" at issue? Has it been fairly shown in a solitary instance? The analogical induction, therefore, as it relates to the gastric juice necessarily fails. We humbly think, also, that the imputed analogy does not obtain in other respects; since the bony levers, muscles, tendons, heart, blood-vessels, &c. are mere instruments acted upon by the "vital principle," and have no part in the vital results except as they are the passive instruments of the forces of life. Not so, however, with the imputed agency of the gastric juice. Here other and distinct forces are supposed to operate, and to take an equal part with the forces of life in one of the animal functions. Nor is this all. These chemical forces, or an equivalent agent, are supposed to appertain to one of the products of living organized matter; and through that product, and by that product, to operate independently of the vital forces, or, under their control. This view, we believe it will be admitted, is opposed by the principle upon which the argument proceeds, since there is no analogical fact to warrant the induction; and with equal truth may we affirm, that there is nothing to aid our conception of the cöoperation of the chemical and vital agents, (Vol. I. p. 32,) whilst all that is known of their relations to each other proclaims their absolute independence. This fact we have more than once had occasion to express; and that there may be no ambiguity as to our meaning, we would say that we speak of these forces as they are concerned in the decomposition and recombination either of the elements of matter or the constituent parts of a compound substance. As to the influence of physical agents upon the forces of life, we have considered that subject in our "Vital Powers," as, indeed, we have the former.(2)

But, again, it is the admitted final cause of the gastric juice to bestow life upon dead matter, whilst it is incontro-

(1) Med. Chir. Rev. Lond. vol. xxix. p. 107.

(2) We have endeavoured to show, in our "Vital Powers," many broad and irreconcilable distinctions between the forces of life and of chemistry. We there said that the chemical and other physical forces operate upon matter, and *are not acted upon*. The other operate exclusively upon living matter, and are also influenced by the former, and *are acted upon* by foreign causes. You may say that the distinction is not absolute, as manifested in the disposing effect of electricity, or some other third substance upon the affinities of two others. Here it would seem that the chemical forces may be influenced in the way which we have denied. But it is

vertible that inorganic matter is insusceptible of any such influence from gastric action. Every fact proclaims that nature has provided the vegetable kingdom for the purpose, especially, of determining organic combinations out of inorganic substances for the sustenance of the higher department of life. It is manifest, therefore, that it would be an absurdity on the part of nature to have ordained that chemical agencies should operate even at the very threshold of life, at the very fountain for which she had provided elaborate means to subvert the combinations of chemistry, and to bring them into new arrangements that should approximate those changes which they were destined to undergo from gastric action; and far less probable is it, that the principle should be lost as she ascends in organization; since every chemical result within the stomach would tend to reduce the aliment to inorganic matter, to counteract the final cause of nature, and, de facto, to render the means of sustenance more and more indigestible, and progressively liable to chemical changes of an inherent nature. Nevertheless, it is evident that the gastric juice is capable, according to its constitution in different animals, of dissolving, though not of organizing, inorganic substances.

However we may regard this subject of analogies, it is every where opposed to the conclusion that the gastric juice is a chem-

wholly different from those impressions which are made upon the properties of life. In the former case, the principle is the same as when two substances are brought together, and unite by chemical attraction. The combination of these only developes the operation of forces which are ever ready to act as soon as opportunities exist. You change the polarity, or vary the plus and minus of electricity in different substances, and in that way promote the operation of affinities. But in none of these instances, are the forces acted upon, or in any way influenced in their natural tendencies, or affected in their natural character.

Again, by the aid of electricity, you resolve chemical combinations into their elements. But here the principle is the same; for by the intervention of a third substance of any peculiar properties, the elements are so impressed that their attractive forces cease, and decomposition follows.

Nor is there any analogy betwixt the operation of electricity upon living and dead matter. In the former case, it operates upon the properties of life, — in the latter, upon the matter itself. That its action, in the first instance, is upon the vital forces alone, is abundantly manifest from its total failure to produce any of its phenomena after those forces cease; but it will now produce the phenomena which distinguish its operation upon dead matter. We speak, of course, of what may be properly denominated vital phenomena. But you say, it is a masterly agent in effecting combinations amongst the secretions, in governing the actions of organs, &c. Perhaps we have already said enough in reply, especially if it be not to the purpose, in another place. See Vital Powers and *Appendix*.

ical agent. This fluid is admitted to be organized and endowed with vitality. Like all other parts of the organized body, it is the product of analogous forces and instruments. But, in no other part has it been shown, in the least, that there subsists an agent of chemical operation.

That the contractions of the stomach facilitate the process of digestion, we cannot doubt; but the former are of a vital nature, and the result, as it respects the aliment can be no other, nor is any other apparently supposed, than that of either disintegrating the particles, or of otherwise exposing them more freely to the vital agent. This auxiliary, therefore, is the same as the knife or the organs of mastication, and has no real connection with the digestive process.

But, if the "vital principle" be capable of "making use of chemical agents," we see not why it may not be equal to the whole work itself. (1) An adequate power is certainly admitted by the foregoing supposition. One may be comprehended, whilst the other is mysteriously obscure. One is more agreeable to the rules of philosophy, and abolishes that inextricable confusion which attends the chemical hypothesis. Besides, what is meant by "making use of chemical agents?" How are they brought into use, and how are they always maintained in one exact operation in each particular process, of which there are multitudes going on in the same individual? When do they begin to operate, and what part do they perform, and what is the allotment of the properties of life? Is there any known concert of action between the two species of forces? On the contrary, is it not every where demonstrated that the forces of life are in direct opposition to those of chemistry? (Vol. I. pp. 31, 32, 62, 99, 101.)

Whatever be the construction, by uniting the two forces, we convert what is probably a most simple problem, like all other processes of nature, into the greatest paradox that has been yet devised by the ingenuity of man. It is in vain to say that some of the organic compounds appear to be such as result from chemical affinities; since chemistry is here incapable of showing the identity of the combinations, and since every thing else appertaining to life appears to indicate the absence of the chemical forces. No one can affirm that the actions of life may not generate combinations that may be analogous to chemical results, yet radically different, according to our former explanation. Upon the same ground, it might be assumed, as it is in part, that

(1) So says Dr. Prout; see p. 84, *note.*

all the motions of organized matter depend upon physical powers, because we witness motion from gravitation, elasticity, attraction, &c. The endowment of the forces of life with capabilities more or less analogous to those of chemistry would seem to be a necessary correspondence, in conformity with Unity of Design, since the elements of organized matter are derived from inorganic, whilst dead organic matter is alike the subject of chemical destruction as of vital changes. (P. 16.)

We now come to another argument of universal application to living matter, upon which we also especially rely. It appears to us fatal to every physical hypothesis of life. If digestion be admitted to result in part from chemical agencies, it can only be upon the ground that alimentary matter is divested of life, and therefore the subject of chemical decomposition. But this rule will not apply to any constituent part of the living body; and since all parts are derived from the blood, and this fluid is shown and admitted to be endowed with vitality, neither this nor any other vital part can be liable to the imputed chemical changes. In making this statement, especially of the blood, it should be well considered that the changes which are necessary to constitute many of the secreted products are of a radical nature, formed by elementary decomposition and recombinations; such as chemical agents are supposed to effect among the elements of compound substances. Nor will this be denied us, since it is the very essence of the catalytic and other chemical hypotheses of life. Admitting, therefore, the presence of any amount of galvanism, or of any other chemical agent that can be possibly surmised, within the living organization, it would be utterly incapable of decompounding any living particle either solid or fluid; not even the most watery part, since this is equally endowed with the resisting properties of life. (1) We are sustained in this position by every known phenomena of organized matter; and let

(1) In all the analyses that are undertaken of the animal fluids, &c. it appears to be considered that the several component parts, which appear in the hands of the chemist distinct combinations, are equally so in their natural state, and exist as simple intermixtures. But we apprehend that it is far otherwise, (allowing the apparent component parts to have a certain definite existence,) and that an intimate union is established amongst the whole at the moment of their formation; at least, that such is true of all the solids and fluids that may be endowed with vitality. This would be a substantial objection to the chemical hypothesis, and a reason why there has been no mutation of any one of the organized products. We doubt not, however, that the difficulty is more profound, and that the constituent which is displayed by the chemist has often no existence within the pale of organization. (See Vol. I. pp. 529, 676, *notes.*)

the experiment be tried upon every part with any reasonable degree of galvanism, or of chlorine, or other agent which imagination may substitute, and the imputed decomposition cannot be effected. How much more impossible, therefore, the supposed catalytic effects of the vascular parietes? (See Vol. I. p. 55.) Since, then, upon this single principle, we see not how the conclusion can be avoided, that the decomposition of the living solids and fluids must depend on other forces than those of chemistry, all philosophy conducts us to a like conclusion that the recombinations are effected by the same agents. And in this construction are we also supported by the absence of those definite proportions of elementary principles which chemical agencies always establish, by the palpable mutations in organic products which chemistry identifies, and by the impracticability of restoring any organic matter when artificially decompounded. And that such appears to be truly the fact, is seen in the separation of the elements which are thus combined as soon as the vital powers cease their operation, and their universal disposition to unite into inorganic compounds, and in the definite proportions of chemistry.

From the foregoing facts, it follows upon the ground of analogy, which is strong upon the subjects under consideration, that the gastric juice must be, as admitted, endowed with vitality; that it can exert no agencies which are utterly foreign to other living matter; and since, as we have seen it stated by chemists that the process of decomposition throughout the body is similar to that of digestion; it follows, therefore, from the foregoing premises, that the gastric juice exerts all the changes to which alimentary matter is subject, in virtue of its vital powers; whilst the stomach itself contributes to maintain the incipient organization, and probably by its influence upon the gastric juice after its secretion, to promote the process of vivification. And from all that has been now said, it must be concluded that the gastric juice is a substance sui generis; and since, also, its formation requires the agency of living blood-vessels, and being a vital fluid derived from another endowed with vitality, it should be admitted to be insusceptible of chemical manufacture. But such is not the conclusion, even of eminent physiologists who should look with jealousy upon any invasions upon the laws of nature, especially such as it is their peculiar province to illustrate. We are presented by these philosophers with artificial compounds, of an incongruous nature, and are told that each is capable of

the same precise results as the gastric juice, whilst the organizing effect of that fluid is fully admitted. Spallanzani, who received so severely the rebuke of Hunter, was philosophical compared with the recent grotesque innovations. To the chemists, however, we should look, at least, for unanimity as to the specific nature of the reducing agent, since every variety of food is resolved into a uniform homogeneous substance. But here, too, there is an equal conflict with fundamental laws. Free muriatic acid having been found, or supposed to exist in the stomach, it has been concluded that this must be the great agent in digestion; whilst Dr. Prout, and others, affirm that "free muriatic acid more or less retards the process of reduction." (1) Dr. R. D. Thompson, however, by digesting muscular fibre in dilute muriatic acid, produced a substance "exactly resembling chyme." (2) This matter had been presented in rather a different shape by Eberle, (3) who found that the mucous membranes digested in muriatic or acetic acids, (4) formed a substance that would digest other alimentary matter. Schwann, Müller, (5) and others, have prosecuted the inquiry, and there is a general agreement that they have generated the true gastric juice; the galvanism of others to the contrary notwithstanding. (6) More recently Todd,

(1) Dr. Prout Op. cit. (2) Sixth Report of the Brit. Association, &c.

(3) Physiol. der Verdauung, 1834.

(4) Ed. Baynard states that "he remembers a man who died with a cheese in his belly, by drinking new milk upon sour beer, which, so frightened people from the use of milk, that all forsook it but the wiser calves."

(5) Müller's Archiv. 1836, p. 66.

(6) The galvanists are equally confident, and exclusive, carrying their results far beyond the process of digestion. Dr. Philip, it is well known, in the very midst of his empire of the "vital laws," maintains the dependence of digestion and secretions upon galvanism; and the practical Edwards descants, as we have already seen, upon the "artificial formation of chyme, milk, and the principal conditions of the secretions," by the same agent, and by that alone. (Vol. I. p. 52.) But these are only incipient steps towards a substitution of physical agencies for Creative Power. It had been already shown in the work shops of Priestley, Ingenhouse, Needham, O. F. Müller, Tiedemann, Wrisberg, Treviranus, Monti, and, more recently, by B. de Vincent, Fray, Mærklin, Nitzch, Cross, and others, that animals could be formed out of inorganic matter by maceration in water, or by galvanism and other analogous processes; though it be that animals require for their sustenance organic matter. We may also add, that these animals copulate and lay eggs; and that Spallanzani found, that when animal infusions were boiled, and the air excluded, no animalcula were produced; and Wrisberg stopped the process of generation by covering the surface with olive oil, and thus preventing the ingress of ova from the air. Yet he believes the dead matter arranges itself into a complex living apparatus. Schulze states that each globule of blood, or of milk, or cerebral matter, is converted, by solution,

and Schweitzer have elaborated the gastric juice after the manner of Schwann, and have added some experimental details, which go with the rest in showing the chimerical nature of the enterprise. (1) But has it ever occurred to any of these gentlemen, to ascertain whether they could obtain from their chyme, even by the aid of nature's bile, a product resembling the intestinal fœces, or have they demonstrated any analogy between the natural and the artificial chyme, unless through the vague results of fire and acids? Again, the digestive "mixture retains its solvent properties for months," (2) whilst the gastric juice loses its solvent power soon after its abstraction from the stomach. And here we may ask, whether it loses, also, its chlorine, or its muriatic acid? And what may seem equally to establish a total opposition between the digestive mixture and the gastric juice, is the no small circumstance that the chemist may torture and extinguish "the digestive principle" of the former in a variety of ways, and then transmute it back in all its vigour. Thus, according to Schwann, as sanctioned by Müller, the "digestive principle," when neutralized by an alkali, "may be precipitated from its neutral solution by acetate of lead, and can be obtained again in an *active state* from the precipitate by means of hydro-sulphuric acid." (3) This precipitate, we are told, when thus restored, and thus compounded of principles radically different from the original mixture, is essentially the same as the gastric juice, and that the results of such artificial preparations must be taken as the test of the *physiology* of natural digestion; that, abandoning nature,

into many hundred animals, as full of life and motion as the best of us. See APPENDIX on *Spontaneous Generation.*

But, we have wandered from our subject of galvanism. It is not remarkable that the speculations which we have noticed should have led to the creation of animals by this agent. The materials are taken, in imitation of the Almighty, out of the earth, and by aid of the potent fluid, our philosophers transmute silex into flesh and blood; (*a*) whilst Dutrochet has created muscular fibres from albumen by the same "universal agent." (*b*) The misinterpreted Bolingbroke remarks, that "the men who attempt to do this leave to God nothing more than they assume to themselves, excepting a greater degree of power." (*c*) But our object was to show the harmony amongst chemical philosophers as to the nature of the agent which conducts the analogous processes of life. The several kinds assigned are as nearly allied to each other, as the "nervous influence" to the galvanic fluid.

(1) British Annals of Med. Nos. 16, 23.

(2) Müller's Elements of Physiol. vol. i. p. 545. (3) Ibid. p. 546.

(*a*) See Mr. Crosse's account of the process, and a representation of the animals in American Journ. of Science and Arts, vol. 32, p. 374, July, 1837.

(*b*) Ann. des Scien. Nat. 1831. (*c*) Works, vol. v. p. 436.

we must look to the resources of the laboratory for any satisfactory account of her vital processes. Nor do we at all exaggerate; for it is distinctly avowed that we knew nothing of digestion till the invention of the artificial mixtures; that Schwann, for instance, "having discovered that the infusion of (dry) mucous membrane with dilute acid, even after it is filtered, still retains its digestive power; the digestive principle, *therefore*, is clearly in solution, and the *theory of digestion by contact falls to the ground.*"(1) Here, a most important physiological induction is wholly founded upon a process which has not the most remote connection with organized matter; and as to the chemical paradox which involves the agencies of muriatic acid, alkalies, acetate of lead, hydro-sulphuric acid, and mucous membrane, it is certainly a burlesque upon the science of chemistry. As to the imputed "digestive principle," it may prove destructive to a goose, though we have great doubt whether it will digest a raw onion. The process which we have stated might be indefinitely carried on by other alkalies and other reagents, and still the "digestive principle" would come up on the development of an acid; and should the "*mixture*," as it is well denominated, be occasionally subjected to the action of fire, we shall still find the mysterious "principle" unextinguished. We would prefer the old doctrines of attrition, or putrefaction, or even that of fire as maintained by Heraclitus and Hippocrates.

Dyspeptics have only to swallow chlorine, muriatic acid,(2) or the "digestive mixture," (which has been most appropriately named by Schwann, and Müller, *pepsin*,) and "throw physic to the dogs." Galvanism is "a rushlight to it;" and though the stomach be no longer a galvanic trough, it is still among the wares of the laboratory.(3) Galvanism, indeed, after its signal triumph, is already in the wane.

> "But let it go:—it will one day be found
> With other relics of 'a former world.'"

It should be borne in recollection that every animal, however

(1) Müller's Physiology, p. 545.

(2) It should be recollected, however, that according to Dr. Prout, it is the peculiar misfortune of dyspeptics to have a redundancy of this kind of the "digestive principle."

(3) Not necessarily so, however. Magendie, for instance, "proved by a multitude of experiments, that the stomach is of no use in vomiting, since when it was cut away, and a pig's bladder substituted, there was no difference in the effects!" —*British and Foreign Med. Rev. Oct.* 1839, *p.* 537.

small, is said to possess a stomach. So far as the microscope may be trusted, we learn from Ehrenberg that the smallest monad, which is only $\frac{1}{2000}$ of a line in diameter, has this apparatus. Shall we believe, therefore, that such an organ is so universally provided, and so variously constructed, for the mere purpose of generating an acid, or any other chemical agent? (1) And here our conceptions are aided by analogies, since the same universality, and the same varieties, are found to prevail in respect to an urinary apparatus, whenever urine is secreted. So of the testes, mammæ, &c. (See Vol. I. pp. 37, 591, *notes*, 588 — 593, 676, &c.)

Besides the philosophers of whom we have spoken, there are many others, as Tiedemann, Gmelin, Bostock, Carswell, Magendie, Montègre, and, indeed, chemists and physiologists generally, who consider acidity an essential principle of the gastric juice, and that digestion depends upon the chemical action of the acid; or, according to Professor Dunglison, "the theory of chemical solution may now be regarded as completely established." (2) But has it been shown that the same acid is not generated after the division of the nervi vagi, when digestion ceases? We are told that the secreted fluid is apparently unchanged. It appears, also, that distinguished physiologists, who have devoted their attention to the science of life, Hunter, Haller, Willis, Spallanzani, Fordyce, Dumas, Schultz, &c. regard the reputed acid as the chemical result of vegetable decomposition. (3) Spallanzani, whose experiments were almost endless, Scopoli, Chevreuil, and others, rarely succeeded in finding it at all, and in some animals never. Spallanzani, indeed, affirms that the gastric juice is neither acid nor alkaline in its natural state. Are these observa-

(1) It having been proved to the satisfaction of many chemical physiologists, that the stomach has no apparatus for the generation of an acid, Purkinje and Poppenheim (*Müller's Archiv.* 1838,) have called in the aid of galvanism, — that ever ready servant of chemistry. The experiments upon which the conclusion is founded, were conducted with all due consistency in tumblers and other utensils, when it was found that nasal *mucus*, *saliva*, &c. yielded to a powerful galvanic battery a small quantity of something like muriatic acid. (Vol. I. p. 676.) The analogy is then carried up to organic life, and the brain and the nerves are assumed as identical with the galvanic battery and conductors.

(2) Physiology, vol. i. p. 500.

(3) Thus, the accurate Fordyce, from his experiments, "was led to conclude that the formation of acid in the stomach during digestion, is *always* produced by the digestion not going on perfectly, the food running into fermentation." (*a*)

(*a*) On Digestion, p. 151. See, also, Schultz de Aliment. Concoctione, 1834, and the others on Digestion.

tions to be set aside to accommodate an hypothesis of life which identifies dead, nay, inorganic, with living matter? Pinel, and a friend of his, had no acid in their stomachs, according to Thènard and Chevreuil. Grosse, says Dunglison, found the juice to vary according to the nature of the animal and the food; and "Dumas proved by experiments on dogs, that it was *acid* or *alkaline* according as the animal had fed on vegetable or animal diet." (1) In Haller's day, and at an antecedent time, the gastric juice had no acid principle. "Although there may be some rare signs of an acid," says this illustrious observer, "it does not, therefore, become us to suppose that *food is animalized by a chemical process;* much less to compare this process with the action of an acid." And, anticipating the modern experiments with the "digestive mixture," he declares of analogous proceedings at his own era: "frustra etiam quisquam, imitatus liquores acres chemicos, *liquorem corrodentem* invenerit, qui carnem in pultem resolvat." (2) And we believe that Hunter's prophecy holds good to this day; that

"If ever any matter is formed in any of the juices secreted in any part of a vegetable or animal body similar to what arises from fermentation, we may depend on it, it arose from that process; but we may also depend on it, that there is a defect of the living principle in these cases." (3)

These are not the mere speculations of genius, but the conclusions of genius after a long, and wide, and experimental survey of nature.

Acids, according to Helm, (who had the best opportunities) lose their acidity soon after they get into the stomach; and it is only by supposing, at least, that their ordinary effects are counteracted, that we can at all explain the compatibility of nitric acid and calomel in the human stomach. (4)

Thus might we go on with an almost endless exposition of the contradictions of chemistry, as to the nature or the presence of any acid in the stomach; and we should find them to abound among the chemical theorists of life. Leuret and Lassaigne maintain that the gastric acid, in all the vertebrated animals, is

(1) Physiology, vol. i. p. 502. See, also, Dumas, in Elemen. Physiol. t. i. p. 278 — 280. (2) Elemen. Physiol. t. vi. l. 19, s. 5. § 2.

(3) Lectures on the Principles of Surgery, p. 17.

(4) We have seen these two agents, through ignorance of their action upon each other, exhibited alternately in large doses, within two or three hours of each other, without any apparent detriment. In one instance the interval was only thirty minutes.

lactic acid.(1) Tiedemann and Gmelin found that the gastric juice of horses and dogs is acid only after food is introduced into the stomach; and this is confirmed by Leuret and Lassaigne. The former, however, find it to be sometimes *muriatic*, and, again, *acetic* acid, whilst the latter discover nothing but lactic acid. Dr. Beaumont comes forward with a perforated stomach; but any conclusions from his experiments, (whose accuracy we do not distrust) as to the agency of an acid in digestion, are contradicted by a parallel case, where Helm(2) avers that he could find no acid in his patient, who had also a fistulous opening in the stomach. Treviranus, reasoning as a chemist, supposed there must be fluoric acid in the gastric juice of fowls;(3) but all the tact of Tiedemann and Gmelin failed to detect it. Were it, however, the agent of siliceous solution, it should exist in a large quantity, as should muriatic, or any other acid that may be imagined to reduce the great quantities of our ingesta. There is no comparison between a quarter of mutton which is sometimes devoured even by man, and the inch square which is employed in experiments with a strong solution, allowing the greatest imaginable production of the gastric juice. It becomes, therefore, more and more apparent, that acids, in a chemical sense at least, have no agency in digestion. We learn, too, from the experiments of Dr. Beaumont, that a dilute mixture of muriatic or acetic acids produced wholly different results in their action upon roast and boiled beef from those of natural digestion, and that it requires gastric juice, *just from the stomach*,(4) to produce a substance that shall have even the *visible appearance* of chyme.(5) Dr. B. found it necessary to make additions of the *fresh* juice to carry on his process out of the body; whilst the necessity of the *recent* juice is proof alone, that in his experiments the reducing process was not of a chemical nature.—(Vol. I. p. 591.)

Again, if acids have an existence in the quantities that are known to be indispensable to their imputed office in digestion, there should always be an abundant extrication of carbonic acid gas, when alkaline carbonates are freely admitted to the stomach; and the process of digestion should be suspended by their repeated

(1) Recherches Physiol. sur la Digestion, 1835.

(2) Zwei Krakengeschichten, 1803, 8. (3) Biologie, t. 4. p. 362.

(4) See Exp. 25, 26, 27, second series; and Exp. 28, 33, 48, third series.

(5) Exp. 96, 105, 106, 115, third series; and Exp. 46, fourth series. Müller, also, utterly denies that acids, *per se*, exert any solvent effect. *Physiol.* p. 542.

administration; both of which inductions, however, are contrary to fact, whilst the *artificial* gastric juice "when neutralized with carbonate of potass becomes inert." (1) If the ingesta, on the other hand, run into fermentation, acids are then generated; and it is here only that chemistry contributes to the process, which it does by destroying the acid which chemistry assumes as the agent in the process. Again in these cases, where acid exists, and alkalies fail, sulphuric, muriatic, and nitric acids, indiscriminately, by stimulating the stomach, restore the true gastric juice, and improve digestion; and that their operation is truly physiological appears also from the attendant vital phenomena. So true is all this that we see lately promulgated by Dr. Thomson an "*acid* indigestion," and an "*alkaline* indigestion." How is it, that the nitrate of silver, in doses of the fourth or an eighth of a grain, produces its remarkable effects upon the system? Why so hazardous in doses of two grains, if muriatic acid be present even in a small proportion, and when less than a dozen grains of the muriate of soda will render it inert? It is obvious that the hypothesis may be tested by other direct experiments of the same nature, and that chemistry itself may be brought to its subversion.

But, what appears to place this subject in its proper rank in science is the declaration of Müller, that of all the acids he had employed, the *oxalic*, mixed with mucus, came nearest to the gastric juice. (2) Or, we might add other interpretations of digestion by erudite chemists to the same effect. Thus, Dr. Davy:

"It is impossible to witness the change which takes place in muscular fibre, in consequence of *putrefaction* giving rise to a fluid very like chyme in appearance, without asking, may it not be concerned in digestion itself, according to the earliest theoretical notions on this subject?" But the reader will take notice, that our distinguished author states, farther on, that "*twenty* different semi-fluids might be mentioned, to which, as far as the eye can judge, this putrid matter bears as close a resemblance as to chyme." (3)

It is stated by the learned editor of Mr. Hunter's Lectures, that "some experiments have lately been performed in America which show that undressed animal food undergoes a species of imperfect *digestion when enclosed in fresh wounds*." (4) Are not these voluntary statements conclusive that the whole chemical hypothesis is a mere speculation?

Taking an average of the foregoing observations, it appears to

(1) Müller's Physiology, vol. i. p. 545. (2) Physiol. vol. i. p. 543.
(3) Edin. Med. and Surg. Journ. vol. xxxiv. pp. 264, 265. (4) P. 54.

be probable that it is with the gastric juice as with the saliva; which was found by Mr. Laycock, from 567 observations, to be "generally alkaline in the morning and acid in the evening."(1)

It is the opinion of Mr. Taylor, that "the secretion" which produces perforations of the stomach after death "cannot be the healthy gastric juice;" since it would otherwise be "much more common in healthy persons," or, "would not be met with in diseased subjects."(2) This is doubtless true, in some of the "rare" instances which occur; when we have no hesitation in believing that the contents of the stomach are of an acrid nature, and exert a chemical action upon the organ. But the unquestionable experiments of Dr. Carswell, and others, show that perforations are produced in other instances by the healthy gastric juice, which clearly maintains its vitality in some instances for a short time after death.

These considerations lead us to state an objection to the chemical theory of digestion which appears to us conclusive. Taking for the basis of our induction the experiments upon which the chemical theory is founded, it should necessarily follow that the stomach would always undergo solution after death, especially

(1) Lon. Med. Gaz. Oct. 7, 1837.

We may also say, that Schultz pronounces the saliva of adults to be acid, especially when retained in the mouth, but that it is always alkaline in children, whether held in the mouth or not; (*a*) and that Mitscherlich found it alkaline during a meal, but acid at all other times. (*b*) Van Selten found the saliva acid before breakfast 17 times, and alkaline 24 times; but after breakfast, it was acid 25 times, and alkaline 15. But, from another suite of experiments, he concluded that when it was acid before breakfast, it became alkaline during the meal, but after breakfast was over, it became acid. (*c*) To this we may add the disagreement between Tiedemann and Gmelin, Treviranus, Ure, Kuehn, Kastner, Van Setten, and others, as to the existence of *sulpho-cyanogen* in the saliva. Just so is it with all other secretions; all showing that the acid or neutral state of the gastric juice is accidental. The coincidence in the chyme and chyle of man and animals involves some analogy, at least, in all the secretions concerned in the process of digestion, if it be of a chemical nature. But chemistry is either everywhere discordant, or opposed to its own conclusions. For thus, again; Magendie finds the pancreatic juice of dogs alkaline, (*d*) whilst Schultz says its acid. (*e*) The latter found the secretion in the cæcum of rabbits alkaline before a meal, and acid during digestion. Tiedemann and Gmelin found it always acid in dogs, but in the horse, always alkaline. Schultz, supposing the cæcal secretion to be always acid, assumes that the *stomach digests during the day, and the cæcum during the night.* Müller objects to this reposing provision, and call it a "hypothesis." (*f*) We have stated these circumstances partly with a view to the subject of galvanic polarity, farther on.

(2) Guy's Hospital Reports, Aug. 1839.

(*a*) De Aliment. Concoctione, 1834. (*b*) De Saliva ejusque Vi et Utilitate, 1837.
(*c*) Rust's Magazin. 1832. (*d*) Physiol. t. 2, p. 367. (*e*) Ut cit. (*f*) Physiol. vol. i. p. 556.

in subjects who die with the capability of digesting food; such as the victims of war, of the gallows, &c. But it is well known, as stated by Mr. Taylor, that "perforations of the stomach, from solution, are very rare in the human subject."

Prof. Dunglison, as we have stated, regards the "chemical theory of solution as completely established;" yet our able professor, coming to the facts, admits that,

"The discovery of the acetic and muriatic acids, in the secretion, aids us a little in solving the mystery, but not much;" whilst the "diversity of the results obtained by chemical analysts, &c. have led some physiologists to doubt the existence of any such gastric juice or solvent as that described by Spallanzani;" and, in regard to the anti-putrescent and coagulating power of the gastric juice, they are "stumbling blocks to the chemical physiologist." (1) — (See our Vol. I. pp. 30 — 32, 38, 41, 49, 56, 58, 62, 64 — 67, 75, 95, 101, 437, 529, 675 — 677.)

Dr. Bostock, too, who advocates, in extenso, the chemical theory of digestion, and of life, says,

"It is not a little remarkable, that when the gastric juice has been examined with relation to its chemical properties, *nothing* has been detected in it which appears adequate to the effects we observe to be produced. As far as we can judge, it *resembles saliva*," &c.; and as to digestion, he observes, that "the more *obscure is the subject, and the less real information* we possess concerning it, the more numerous have been the attempts to frame hypotheses to account for it;" "we are in a great measure ignorant of the agents by which it is effected." (2)

Our author also agrees that the anti-putrescent and coagulating power of the gastric juice "are extraordinary and inexplicable;" and concludes that the chemists are mistaken, and that coagulation is "an effect *entirely sui generis*, which we are not able, at present, to refer to any general principle." (3) As to the anti-putrescent property, he says that,

"We can only say concerning it, that it is a chemical operation, the nature of which, and the successive steps by which it is produced, we find it difficult to explain; at the same time, that we have very little in the way of analogy which can assist us in referring it to any more general principle, or to *any of the established laws* of chemical affinity." — (Our Vol. I. pp. 35, 514.)

Berzelius, also, not less an advocate of the chemical hypothesis of life, (See Vol. I. p. 36,) after exhausting the foregoing inquiry, was forced to the admission, that we know nothing of the

(1) Physiol. pp. 503, 511, 507.

(2) Elements of Physiology, vol. ii. c. 10, s. 3 and 4, pp. 382, 403, 409.

(3) Dr. Bostock, Op. cit. vol. ii. c. 10, s. 3; and vol. i. c. 6, s. 5.

Montègre, (*Op. cit.*) denies that gastric coagulation depends on the action of an acid; which is also conclusively shown by Dr. Young. — See Fordyce on Digestion, p. 58.

solvent principle of the gastric juice. But, it is perhaps said, that these authors are obsolete, thrown aside amongst the rubbish of antiquity. Well, then, here is Müller, fresh from the laboratory, and fresh with honours; of whom, however, it may be said hereafter, as it is now of Hippocrates, "he was well enough in his dark age; but who thinks of Müller's authority in these revolutionary days of science?" But let that pass; here is Müller, and we all of us now swear by him. This philosopher, after saying that "the theory of digestion by contact falls to the ground," because "Schwann discovered that an *infusion of mucous membrane with dilute muriatic acid*, even after it is filtered, still retains its digestive power;" and arguing from these results of the laboratory, that "the activity of the digestive principle depends on the presence of a free acid" in the gastric juice, concludes that, "*it must, therefore, be allowed that no investigations thus far have informed us of the nature of the active solvent principle of the gastric juice.*" (1) And so in various other enlightened quarters. Thus, —

"Most alimentary substances undergo in the stomach a very decided change in their chemical composition. How this change is produced is still a matter of dispute. No one, however, has as yet even attempted to prove *that it results from the action of the gastric fluid.*" (2)

And such we shall find to be the ultimate admissions of all but the asteroids, who have toiled at this contest with nature. "Felix qui potuit rerum cognoscere causas."

By such proof it is contended that "the physiologist should not have recourse to the vital explanation, until every other has failed him;" since "the explanation is, in truth, another method of expressing our ignorance, when we affirm that any function is executed in an organic or vital manner." (See our Vol. I. pp. 17, 21, 24 — 26, 28, 30, 42 — 45, 50.) But, then, — "the secretion of the fluid which is the great agent of chymication, is doubtless *vital*." "The organizing agency of the stomach is vital, and its nature completely unknown." (3) (P. 103.) We may also say, that we have endeavoured to show that we have more real information as to the properties and laws of life, and far greater ground to reason about them, than the chemist or the mechanical philosopher have respecting the forces of inorganic matter. (Vol.

(1) El. of Physiol. vol. i. pp. 545, 548, 543.

(2) American Journ. of Med. Sciences, No. 37, p. 131, 1836.

(3) Dunglison's Physiology, vol. i. p. 509. 1836. Agreeing with Dr. Prout. (P. 80.)

I. pp. 30, 34, 43, 50.) And considering this undeniable fact, and what we have, and shall have seen of the admissions of chemists, that they cannot interpret a result of organized matter by the laws of chemistry, it is not a little unaccountable that eminent and learned physiologists, like Dr. Bostock, for example, should summarily dispose of the doctrine of the vital properties as expounded by Hunter and Bichat, upon the ground that "we have no independent evidence of their existence, nor any conception of the mode of their operation." "They have never been proved to exist, and are, at the same time, incomprehensible."(1) (Vol. I. p. 35.) This common doctrine is, at least, as much opposed

(1) Bostock's Physiology, vol. ii. p. 309. The Dr. is here speaking of the secretions; but this observation, which occurs also in other places, is intended to be general in its application.

Dr. B. is not even disposed to allow the pneumogastric nerve an agency in the process of digestion, because "we can form no idea of the mode in which the mere chemical action of two bodies can be effected by the nervous influence." (*a*) This is exactly our own argument, (Vol. I. p. 714; Vol. II. p. 26, &c.) whilst we urge a variety of facts to sustain it. The vitalist is intent on the observation of nature. He sees in the phenomena of digestion something that implies the agency of chemical forces. He goes to the practical chemist, as the proper source of information, to learn whether there be any analogies in his science which will account for the wonderful results. He is told, as we have variously shown, that there is nothing in the gastric juice, and nothing in the known operation of the chemical forces, "nothing whatever in the complex organization," which throws any light upon the subject, (p. 83.) Not choosing, however, to take the mere ipse dixit of the chemist, that digestion, and other vital functions, are, notwithstanding, chemical processes, he examines the living organism to see if there be not something here as remarkably different from any thing in the world of mere matter, as its results are different from the phenomena of simple matter. He investigates the whole range of vital effects, varies them in a thousand ways by experiment, observes their infinite and unique modifications in disease, and finds there is no one that can be explained by chemical agencies. He sees that all the phenomena of living beings are influenced by causes that have never been known to modify the results of chemical forces; and he sees, too, that the properties of life constantly resist the disorganizing effects of chemistry; whilst their instant extinction is succeeded by the ravages of the latter. With this almost infinitely varied knowledge, which is exclusively foreign to the inorganic world, he approaches the subject of digestion; and whilst he justly applies to it the whole force of his analogies, he finds also here a thousand coincident facts. — See APPENDIX on *Analogy*.

Many physiologists are disposed to think with Dr. Bostock, that "we gained no real insight into this subject by employing a *new phraseology*." Certainly not; but we have gained by it an exclusion of the chemical phraseology, which was about as well suited to designate the actions of life as it would be applicable to the mechanic arts. "Terms of this nature," however, "have no real value except as generalized expressions of facts." (*b*) Even Müller remarks, that "he, who chooses to call the

(*a*) Physiol. vol. ii. c. 10. s. 4. p. 441. (*b*) Mayo's Physiology, p. 2.

to all inductions in chemistry, as in vitality, whilst it inculcates a disregard of every phenomenon of life, and the essential foundation of the healing art. Even Dr. Fletcher, whose "truculent blade" against the "vital principle" is commended by the distinguished British and Foreign Medical Review, (Our Vol. I. p. 714.) admits that, —

"Of the immediate nature of physical causes in general we know absolutely nothing;" but "we know that organized matter, when acted on by certain appropriate powers, manifests the phenomena of life; that, the more perfect is the organism, the more remarkable are these phenomena, and that any change in the former produces a corresponding change in the latter." (1)

Again, if digestion be a chemical process, considering the great varieties of food that are often mingled in the stomach,

heavenly bodies 'organisms,' may do so. In my youth, I thought it quite right to do so. The consideration, however, that the differences of organic beings and ordinary organisms are greater than their resemblances, determined me to drop a designation so agreeable to some inquirers into nature." (*a*) (See Vol. I. pp. 31, 629.)

(1) Rudiments of Physiology, Part ii, p. 30.

It is due, perhaps, to Dr. Fletcher, that he be farther heard: "If we reflect," he says, "on vital actions, *anomolous as they appear*, what do we find in them, in fact, but certain movements of either particles or masses of matter, not certainly identical with, but still very analogous to those which, in unorganized matter, we call chemical and mechanical; and which we are contented to ascribe, not to any substantial principle of action, but to certain properties and powers resident in these matters, the reciprocal action of which gives rise to what are called attraction and repulsion? And why need we hesitate to admit that similar, though *not the same*, properties and powers may, in organized beings, be competent, whilst they are in mutual cöoperation, to effect those actions in which life consists," &c.? Aye, "MUTUAL CÖOPERATION."!

It brings no light to the subject to ask why "we need hesitate to admit that properties and powers similar to the physical may be competent to effect those actions in which life consists?" This is the very question before us; and we "hesitate," not only because we can see nothing to justify the "admission," but every thing opposed to it, — even the admissions of physical philosophers themselves.

In the foregoing extract, there is also a begging of a specific question, since even no chemist contends for any analogies between the absolute processes of life and those of inorganic matter; and, indeed, the latter only begin in organized beings when the former end. Our author is, also, manifestly impressed with the distinction, since he constantly speaks of the *properties of life* in contradistinction to those of mere matter, and allows in his most guarded paragraph, that they "are not the same;" which means, we suppose, something as different as sensation is from the fall of a stone. (See our Vol. I. pp. 48, 49.) Nor should it be neglected, that philosophers of this school, (even Dr. Prout who believes the "corporeal machine" to be a mere chemical laboratory, see Vol. I. p. 36,) defend the vitality of the blood and gastric juice as something utterly distinct from any thing in physics.

That the vital powers are not the same as the physical, that they are real powers, and no fiction, is an important concession, (Vol. I. p. 714.) which enables us to proceed at once to inquire, through the medium of their phenomena, into their intrin-

(*a*) Physiol. vol. i. 2nd. Pref.

agreeably to all that is known of such combinations when immersed in any mixture that may resemble the gastric juice, there should be nothing uniform in the results at any two meals; but the greatest confusion should prevail. (Vol. I. pp. 56, 57, 62.) It will not answer to compare experiments made with "*pepsin*" upon a single substance, and that one, too, always of an animal nature. Let us have well secured in a bladder a due proportion of turtle soup, fish and melted butter, beef, mutton, veal, venison, pig, poultry, lobster and its accompaniments, snipe and woodcock larded with pork, cony and capon, terrapin à la mode Françoise, cabbage and vinegar, potatoes, squash, peas, pickles, pies, puddings, cheese, custards, sweetmeats, ice-cream, jellies, apples, pears, peaches, prunes, grapes, nuts, figs, brandy, porter, madeira, champaign, burgundy, and hic hæc *hoc*, which, according to Millingen, was the daily repast of our forefathers, and the "ne plus ultra of human genius," with as much of the *digester* as you please. (1) Put the whole into a tub of water at 98° Fh., and

> "Amidst this tumult of fish, flesh, and fowl,
> Wine, nuts, vegetables, cheese and gravy,
> All in masquerade, ————————" (2)

if the bladder do not explode whilst the epicure, who has made the same repast, is quietly taking his siesta, and without any ex-

sic character, without embarrassing the investigation by again indicating the fallacy of the analogies by which they have been supposed to be related to the forces of physics. It is manifest, also, from their admitted dissimilarity, that we can in no respect reason from the supposed nature of one to that of the other, but that each must be tested by their respective phenomena. This is too plain a rule in philosophy to justify farther comment. Nor shall we argue the question as to any substantial principle that may be supposed to appertain to the "properties and powers of matter;" but granting there is none, this conclusion will strengthen the absolute distinction between the organic and inorganic forces, since the final and total disappearance of the actions of life, whilst organization remains, irresistibly implies, that a substantial principle has been extinguished; (Vol. I. pp. 26, 87, 92, 98,) and this is farther confirmed by the immediate invasion of the forces of physics, which had been, till then, utterly resisted. The demonstration, therefore, is very various. 1st. in the perfectly unique phenomena of life, resulting from vital stimuli. 2d. the direct opposition of the powers of life to those of physics. 3d. the instant disappearance of vital results when death is produced by means that can have no intelligible effect in deranging the structure of parts. 4th. the immediate succession of inorganic actions. (Vol. I. pp. 68—70, 583, 588—596.)

True, the phenomena of life generally result from "certain movements of either particles or masses of matter;" but it is these very inherent movements which imply the existence of a principle for which we contend and which nothing in physics can

(1) Curiosities of Med. Experience, pp. 79, 83. (2) Byron's Repast in Don Juan.

plosion, we will go over to the enemy. Nay, we will rest the decision of this question upon an experiment with "pepsin" or chlorine, or galvanism, or "*oxalic*," (p. 101,) or other acid, upon a decoction of simple arrow-root. (1) Whilst, however, the foregoing hotch-potch undergoes an endless variety of changes in the hands of the chemist, he confesses that in the stomach of the epicure, "whatever the nature of the food may be, the general composition and character of the chyle remains always the same;"(2) and the same affirmation may be made of the chyme.

Nay more, "the assimilating organs," says Dr. Prout, "appear even to decompose principles which are still considered as elementary; nay, to form azote or carbon; so that it is *impossible to define what on an emergency*, these organs are capable of doing."

And here it is important to remark, that whilst the gastric juice is supposed to convert, by its chemical agencies, all substances from the albumen of an egg and the farina of arrow-root up to the most glutinous compound into the same homogeneous substance, it is found, on entering the duodenum, to have acquired nearly the composition and most of the characteristic properties of blood. Has it, also, ever been considered what a vast amount of acid would be necessary, on chemical principles, to accomplish the digestion of such a meal as we have just contemplated; or how impossible it would be for the trifling quantity which may be contained in the gastric juice to effect the chemical solution of a leg of mutton, and a fair proportion of other substances, which are completely disposed of by some bipeds in less than six hours? Have any experiments ever been conducted upon this scale of nature?

And again, no culinary process can adapt to man much of the food of graminivorous animals; and whilst "pepsin," and muriatic acid, are as abortive here as in our suppositious cases, no chemical distinctions prevail between the gastric juice of man

explain. (vol. i. pp. 44, 45.) But, are there not vital phenomena appertaining to the egg and seed which are independent of physical movements? (Vol. i. pp. 17, 21.) May we not derive some light upon this subject from the manifestations of mind? (Vol. i. pp. 84, 91, 106.) Has not the rejection of the vital principle, by extending the supposed physical analogies been a prolific cause of materialism, in its moral sense? — We justify every repetition of our "Vital Powers" on the ground that this is a distinct essay.

(1) It is well stated by Fordyce, that it is not compatible with chemical action to suppose that a single menstruum can form with a single principle, like farina, the several substances which constitute chyme. — On Digestion, p. 139.

(2) Dr. Prout, Op. cit. b. 3, c. 3, s. 2.

and those of animals. But the vitalist expounds the problem by referring to the variety of organization in the apparatus for digestion, and, in conformity with all analogy, as great a variety in the insensible constitution of the gastric juice.(1) The chyle, too, is nearly, if not exactly, chemically identical in all the genera; and since chemistry professes not to explain this coincidence, we recur to the differences in the structure of the digestive system as the foundation of peculiarities in the vital principles of the gastric juice of different animals, by which a common result is obtained in its action upon grass and flesh.(2) The food of herbivorous animals is essentially composed of saccharine matter, whilst the chyle is the same in composition as of animals that live on flesh, or the Esquimaux who fattens on blubber; and what is most remarkable of the former, it consists mainly, like the chyle of the latter, of oleaginous and albuminous principles.

As to experiments with the gastric juice out of the stomach, have they ever been properly conducted? Have they not been mostly limited to a solitary kind of animal or vegetable matter;(3) and is it probable that even the most recent juice would reduce to proper chyme a simple vegetable principle as farina, much less the ordinary mixtures of food? On the contrary, it was shown, in the simple processes, by Spallanzani,(4) Pelletier, Fordyce, and others, particularly by Pelletier, that the substances, when subjected to the gastric juice in a dead stomach, or out of the organ, underwent an acid or putrid fermentation; the juice being in this condition a far inferior solvent, as well as preservative, to the

(1) See remarks on Pus, Mucus, Serum, &c. in Essay on *Inflammation;* and Vol. I. p. 627—628.

(2) Dr. Bostock thinks that the vitalists, "in order to prove the point which they wish to establish, ought to show not only that the chyle, but that the *fæces*, also, are similar."—*Physiol. vol. ii. c.* 10, *p.* 410. We think not; but that their ground is amply justified by many of the foregoing facts, at least till they are consistently explained by the chemist. The vital forces, however, are only concerned in eliminating the nutritious matter, and converting it into one homogeneous substance. Having thus accomplished this great final purpose, they suffer the fœces to pass on as wholly unworthy their farther notice; and these will be very likely, therefore, to come out of the ordeal with such differences as one would suppose should appertain to the residuum of ingesta that are distinguished by an endless variety of component parts, and that are cast aside by the vital forces. But let the chemist try his utmost skill with that of our favourite forces, and we doubt not that the latter will wholly surpass him, not only in the elaboration of chyle, but in the formation of excrement.

(3) See Müller's Physiology, p. 537—540.

(4) Experiences, Dis. 2, §85; Dis. 4, §149, 186; Dis. 5, §216, &c.

artificial preparation. Fordyce affirms that, "whether we employ gastric juice, or bile, or saliva, &c., in no case has chyle or *anything like it* ever been formed." (1) Müller, also, gathering the results of experience, states that the action of saliva and bile upon organic compounds out of the stomach is wholly different from what takes place within the body. (2) (Vol. I. p. 591.) Here is a strong analogy; but this author resolves the problem as to the gastric juice by a simple result.

"Has the gastric juice," he says, "the property of *dissolving* the food submitted to its action, whether in the stomach, or out of the body? The answer to this question depends wholly on our being able or not, to *dissolve* food artificially by means of gastric juice mixed with it out of the body." (3)

This is certainly neither the question, nor the answer. The question is, will the *same products* arise out of the body, as take place within; not whether the gastric juice be capable of effecting a simple *solution*. Any other inquiry leaves the subject in its original obscurity, or, rather, leads us into error by false analogies. The experiments, too, must be tried upon various mixtures of food; they must cope with nature in her ordinary processes. Doubtless, the gastric juice retains for a certain time, after removal from the stomach, its vital properties,(4) in a diminished degree, and may partially organize some simple compound like beef or bread, or the stomach itself. But, if muriatic acid, or chlorine, or other chemical solvent, happen to be present, and experiments with these menstrua be well founded, a solution somewhat resembling chyme will equally follow. It is manifest, therefore, from the entire failure of organic chemistry in all complex and precise investigations, it may be impossible to arrive even at the actual condition, and the nature of the agency of the gastric juice when removed from the stomach. (Vol. I. p. 675—677.) But, allowing, as we believe to be true, the presence of diminished vitality, this becomes progressively exhausted, and before the process of vital solution shall have been

(1) On Digestion, p. 144.

(2) Physiol. p. 289.—Here is enough in two pages of our author to explode the whole doctrine of physics in relation to all the processes of life.

(3) Müller, ibid. p. 537.

(4) What shows the probability of this is the fact, that another fluid, the semen, which, like the gastric juice, has foreign relations, was shown by Spallanzani to retain its vital properties after removal from the vesiculæ. He "succeeded as well by artificial means in procuring impregnation, as if the male himself had performed the proper function." This is sustained by Mr. Hunter. But there was no artificial semen attempted; nor are we aware that it has been by modern chemists.

completed, chemical affinities will set in, and vitiate the whole result. There may then be found some albumen, some fibrin, &c., so far as analysis may be trusted; but even this will not prove that the whole process was not purely chemical, since the beef may not have undergone an entire chemical decomposition. We are the more confirmed in this last conclusion by the statement of Müller, that "organic substances once formed, even when subjected *to the action of inorganic compounds out of the body, frequently undergo merely a change of organic combination.*" (1) This, we apprehend, will explain all the supposed analogies between the results of "pepsin," and "oxalic acid" in their action upon "beef" and the changes which beef undergoes in the stomach.

Inductions as to the chemical nature of digestion have been sometimes partially founded upon the green appearance of the bile, under circumstances of *disease*. But here acids are known to result from the fermentation of toast-water alone. Besides, the bile is already green in the gall-bladder, (2) and it is to the liver alone that we must probably attribute the change.

If the properties of life be alone concerned in the process of digestion, it should probably happen, that whenever those properties of the stomach are deranged, the gastric juice would be more or less incapable of exerting its vital effects, and that the matter of food would then be digested as it is by "pepsin," or by "oxalic acid." And we truly find, that just in proportion as disease invades the stomach does food remain unchanged or runs into varied fermentation. Indeed, we have nearly a demonstration of our ground in the sudden and complete suspension of the process of digestion, in a state of health, by the abrupt invasion of

(1) Müller's Physiol. vol. i. p. 289.

(2) This, also, sets aside the conclusions as to the direct action of calomel upon the bile in imparting the green colour. It may indirectly contribute to this effect; but in a vital sense, by stimulating and otherwise modifying the vital action of the liver, and an oxyd of mercury will do the same; whilst the continued use of either will sooner or later be followed by yellow dejections. The green colour, too, rarely or never appears from the action of calomel under circumstances of health. Since, however, acids and calomel impart a green colour to the bile out of the body, and fail of this effect in the former case, it is another demonstration that chemical affinities are resisted by the vital fluids of the body, as they appear even to be amongst many *inorganic* substances when subjected to the gastric juice. This, however, appears to be abundantly shown in the harmony with which a vast variety of alimentary and non-alimentary substances abide together in the living stomach; some, too, being acid, whilst others are alkaline, &c.

mental emotions; and again by running or other causes that embarrass vital action. We are content that the result should be attributed to a partial suspension of the secreting process.

From what we have now shown as to the existing facts in relation to digestion, we feel entitled to ask, whether there be any better foundation for the chemical hypothesis than for its confrère which, at a former period, united a large sect of philosophers? That the reader may properly understand our allusion, we may recall to his mind, that Borelli (1) estimated the force of the muscles concerned in the mechanical digestion of food at 261,186 pounds; Wainewright, (2) at 260,000; Fracassini, (3) at 117,000; and Pitcairn, (4) at 12,900 pounds. And, as to another ally, *putrefaction*, which carries such striking analogies with the phenomena of life, we have seen (p. 101,) that it is seriously proposed by existing chemists that it shall be again restored to explain what may not be satisfactorily resolved by the artificial processes.

What we have hitherto said as to the dependence of digestion upon the properties of life is farther supported by the exclusive operation, as we have variously endeavoured to show, of these properties in every other step of the process of assimilation; and again, if we have shown the latter, it forms an important ground of analogy as to the former. Reason, proceeding upon facts, rejects the compatibility of the forces of life and those of physics; and so completely are the latter excluded, that every motion, animal or organic, depends on the former. The supposed analogy which combines the two we have variously shown to be without foundation, as should be inferred alone from the uniformity of the vital product of every part in its normal state, (yet diversified according to the part,) and from the correspondence of the modifications in disease with certain vital phenomena; since these considerations are opposed to the rules of analogy as applied to the capricious results of chemistry operating in a uniform and highly compound fluid.

In respect to galvanism, M. Donné (5) has lately stated that the saliva, which is generally said to be alkaline in its natural state, becomes acid under certain circumstances of disease, as does also the pancreatic fluid. M. Dumas, in his report to the

(1) De Motu Animalium, p. 80. (2) Mechanical Account of Non-naturals, p. 5.
(3) De Digestione, c. 14, p. 58. (4) Elementa Med. p. 32.
(5) Archiv. Gén. May and June, 1835.

Academy of Sciences upon M. Donné's essay, has the following observations as to the chemical theory of the secretions:

"If the secreting organs are the poles of a pile, separating the secreted fluids from the blood, it is difficult to explain how the poles of the pile could be reversed *as the consequence of diseased action;* yet it is not impossible to conceive that it may be so. We know, in fact, that it is sufficient to change the exciting fluid of the pile in order to reverse the poles. If, therefore, the phenomenon of secretion is of an electric character, we ought, by changing the condition of the organ which performs the function of a pile, to change the nature of the fluids secreted."

We have quoted the foregoing paragraph, since it appears to be one of the unanswerable objections to the chemical hypothesis of life, though not so intended by its able author. Who does not see the entire want of analogy between the "exciting fluid" in one instance, and the galvanic apparatus, which is made the same, in the other? Besides, what changes "the condition of the organ" unless it be a change of the vital forces? And since it must come to this, why look farther for "a change in the nature of the fluids secreted"? But this important agency of the forces of life in "changing the condition of the organ," or reversing its poles by mental affections alone, is wholly overlooked by the chemist, although he cannot move a step without this preliminary interposition.

Whenever nature undertakes the construction of a galvanic apparatus, she does it in a way to save all dispute among physiologists. (Vol. I. p. 114 — 117.) But, in all the instances where this provision has been made, the galvanic product has been shown to differ from that which is elicited from inorganic substances, and to depend upon the forces of life; whilst it has no reference whatever to the economy of organization, but like horns and stings, is designed as a mere matter of defence.

Since, therefore, it appears to be manifest, that no vital product can be formed without the agency of specific forces which are in every respect contradistinguished from those of chemistry, imagination must yield to the necessities of the case; and such being our unavoidable position, it is time to abandon not only the manufacture of animals, but of the gastric juice; for, if both be equally the product of organized matter, and the latter so invested with the forces of life, that, according to Dr. Prout's admission, "the stomach must have, within certain limits, the power of organizing and vitalizing the different alimentary sub-

stances," it is as absurd to attempt the imitation of one, as it is the creation of the other. "Gentlemen," says Bacon, "nature is a labyrinth in which the very haste you move with will make you lose your way;" whilst it is conceded by Dr. Prout, that nature will not permit the chemist to officiate as her journeyman, even in the most trifling degree; (1) and by all others, that "vital chemistry is too subtle a power for human science to detect, or for human art to imitate;" (2) whilst it is avowed by an able chemical physiologist, that "the chemistry that is taught in the schools is not the chemistry of life; and it would puzzle many of its professors to say in what point it can be more extensively applied to physiology, than hydraulics or any other science." (See Vol. I. p. 75.) Our able Silliman, in solitary relief, standing wholly aloof from the "schools," thus disposes of the subject of digestion: "It belongs," he says, "to *animal physiology*, and we therefore dismiss it with these few citations of facts." (3) Finally, it is avowed by Dr. Prout, that,

"The most determined skeptic cannot assert that there is *any* necessary relation, or indeed, *any relation whatever* between the *mechanical arrangements* and the *chemical properties to which they administer*. There is no reason why the chemical changes of organization should result from the mechanical arrangements by which they are accomplished; neither is there the *slightest reason* why the *mechanical arrangements*, in the formation of organized beings should lead to the chemical changes of which they are the instruments." (4)

And here we are reminded of the philosophy of Dr. Beddoes, who inquires,

"Does not muscular contraction really depend upon the combination of oxygen with hydrogen and azote, in consequence of a sort of explosion produced by the nervous electricity? According to this hypothesis, animal motion would be produced by a very beautiful pneumatic machinery, and our nervous and muscular systems may be considered a sort of steam engine." (5)

Hutcheson, probably seeing the want of analogy between the chemical process and a steam engine, actually attempted in a

(1) Gulstonian Lectures, 1831. — Dr. Philip says that Dr. Prout complains that "cooks, unfortunately, are seldom chemists; a circumstance in respect to the physiology of digestion less to be regretted, than that chemists are seldom physiologists." (*a*) And yet Dr. Philip thinks that all the secretions are the result of a chemical process; "that voltaic electricity may be substituted for the nervous influence, not only in the more simple, but in the most complicated functions of that power;" and "is capable of forming the secreted fluids from the blood, and supporting all those functions on which the structure of every part depends." (*b*)

(2) Dr. Roget. (3) Elements of Chemistry, vol. i. p. 615.

(4) Bridgewater Treatise, ut cit. b. 3, c. 4.

(5) Additional Observations on Girtanner's "Laws of Irritability."

(*a*) Lon. Med. Gaz. vol. xiii. p. 914. (*b*) Inquiry into the Nature of Sleep and Death, p. 25.

treatise on philosophy to show that an animal is a steam engine.(1) See Girtanner's opinion, Vol. I. p. 399.

Returning to Dr. Prout, it is also the opinion of this distinguished chemical physiologist, that,

"The *means* by which the peculiarity of composition and structure are produced, which is so remarkable in all organic substances, like the *results* themselves, are *quite peculiar*, and bear little or *no resemblance* to any artificial process of chemistry." (2)

Russel says, "the properties of contagion are only to be collected from their visible effects." Chisholm rejoins, that "the revolution (in chemistry) which has since taken place, and the astonishing discoveries it has led to, account for Russel's opinion." (3) Chisholm wrote in 1794. What has the subsequent "revolution in chemistry" done but to encumber the science of organic life, not excluding even contagion, with dogmatic mysteries and degrading absurdities? We constantly speak on the authority of chemists themselves.

"Every subsequent attempt," says Dr. Bostock, "to discover the elements of organized substances, differs more or less from those that preceded it." (4) So, too, Berzelius, (5) Daniel, (6) Stevens, (7) &c. (P. 14, *note.*)

Pus, and the caseum of cheese, are said to be alike.(8) Chemistry professes also to show that the albumen of eggs, lymph, mucus, and the product of certain cancers are nearly or exactly the same,(9) besides other remarkable coincidences of the foregoing nature. We may also add that the mean analysis by Berzelius, Gay-Lusac, Thenard, Prout, and others, identify starch, lignin, gum-arabic, sugar, and vinegar. We must suppose, therefore, that the elements and constituents are combined in peculiar ways in the several instances by the agency of the vital properties, of which chemistry can take no cognizance, and in which, therefore, it has had no participation. (Vol. I. pp. 35, 589, 676, *note*, &c.)

(1) On the "Œconomy of the Human Frame upon the New Philosophy," in his "System of Moral Philosophy."

(2) Ut supra, c. 1.

(3) On the Pestilential Fever of the West Indies, vol. i. p. 254.

(4) Bostock's Physiol. vol. i. c. 6, s. 7. (5) In Ann. Philos. vol. 4, p. 402.

(6) Children's Thenard, p. 358. (7) On the Blood, &c. p. 20.

(8) Andral's Patholog. Anat. vol. i. p. 295.

(9) Dr. Elliotson says that "even in the utmost refinement of his (man's) luxury, and in his choicest delicacies, the same great principle is attended to; and his sugar and flour, his eggs and butter, in all their various forms and combinations, are nothing more than disguised imitations of the great alimentary prototype, milk, as presented to him by nature." — *Human Physiology.*

Professor Caldwell, observes, that "after the blood is dead, and has undergone real dissolution, and more especially after it has sustained the tortures of the chemist's crucible, I care not what is discovered in it. The ingredients are now the result of decompositions and recompositions that have taken place in it after its elimination from the vessels that contained it." (1)

We have seen, that up to the present time, eminent observers hold the same language. (Vol. I. p. 529, *note*, p. 639—641.) Fire, and acids, and the galvanic battery, are the proper agents in inorganic chemistry; but they are surely out of place where organized matter is concerned.

"I have endeavoured to prove," says M. Raspail, "how deceptious reagents are, as applied to a substance so complicated as the blood. Nature abounds in mixtures, whose study the chemist has not entered upon, and which might be capable of presenting on a small scale the appearance of blood. The effects of reagents on spots formed by the albumen of pulletts' eggs in which I had steeped a small bag filled with madder, was precisely the same as on real spots of blood." (2)

Nevertheless, it is held that a chemical analysis of "spots of blood" may aid in the important concerns of medical jurisprudence; whilst the microscope, taking counsel of the foregoing doctrine, has proffered its help of detecting the hypothetical animalcula in "dried spots of semen." And here we cannot forbear adding to our examination of Dr. Prout's views of life in our vol. i. p. 36—40, the following declaration of this eminent chemist.

"With the living, the animative properties of organized bodies, chemistry has not the SMALLEST ALLIANCE; and probably will never, in any degree, elucidate these properties. The phenomena of life are not, EVEN REMOTELY, analogous to anything we know in chemistry as exhibited among inorganic agents." (3)

The farther chemistry pushes its investigations, the more it multiplies proofs that our whole subject belongs to another department of philosophy; and the more we examine the facts, the more have we felt disposed to cancel our note in our first volume, page 30, which had a reference to this essay. We find, indeed, that chemistry is everywhere against the theories which have been founded upon its own principles; and in this respect it will have contributed greatly to the science of life. All that is now necessary to obtain the full benefit of the light which has been thus reflected, is the permission of chemists that it shall

(1) Phila. Jour. Med. and Physical Sciences, 1822, p. 222.

(2) Organic Chemistry, p. 417. (See our vol. i. p. 36—41)

(3) Ut cit. b. 3. c. 4.—See result of Fontana's experience in our vol. i. pp. 484, 508.

take its natural direction. At a darker era of knowledge, it was said that "every new system of philosophy, true or false, must be embraced and introduced into medical science." Asclepiades explained all by the Epicurean or corpuscularean philosophy; Galen, and his disciples, by the philosophy of Aristotle; another eminent sect by the physico-mechanical philosophy of Newton, who, says Bryon Robinson, "discovered the cause of muscular motion and secretion, and furnished materials for explaining digestion, nutrition, and respiration;" (1) whilst Sir H. Davy, and numerous followers, think it "possible that *one law alone* may govern and act upon all matter; a law which might be called the law of animation," (2) &c.

"Chemists, and natural philosophers," says Bichat, "accustomed to study the phenomena over which the physical forces preside, have carried the spirit of calculation into the theories for those which the vital laws govern;" (3) agreeing in this respect with his great original, who says that "the actions and productions of actions, both in vegetable and animal bodies, have been hitherto considered so much under the prepossessions of chemical and mechanical philosophy, that physicians have entirely lost sight of life." "Of all things on the face of the earth," Mr. Hunter adds, "definitions are the most cursed; for if you make a definition, you may bring together under it a thousand things that have not the least connection with it." (4) — (Vol. I. p. 66, *note.*)

"Mr. Locke, I think," says Dr. Reid, "mentions an eminent musician, who believed that God created the world in six days, and rested on the seventh, be-

(1) Treatise on the Animal Economy, p. 45.

(2) Sir H. Davy's Essays on Heat, Respiration, &c. So Dr. Wardrop; on the Nature and Treatment of Diseases of the Heart, Part I.

(3) General Anatomy, vol. ii. p. 54.

(4) Hunter's Lectures on the Principles of Surgery, pp. 15, 16.

In our first volume (p. 66, *note,*) we presented a suppositious case by the editor of Mr. Hunter's works. We advert to that case in connection with our present subject to show the abortive nature of chemical physiology. In the able article upon Mr. Carpenter's Physiology in the British and Foreign Medical Review, (Jan. 1839) we find the same case expressed in the following manner. "Could we place these elements in the same mutual position and under the same agencies as in the organized being; could we, in fact, adjust the temperature, the moisture, the electricity, the light, and the heterogeneous atoms to be acted upon, just as they are employed in the laboratory of nature; we entertain not the slighest doubt, that the ensuing actions would be the same." Thus far, we agree perfectly with this distinguished reviewer; but when he concludes the foregoing sentence with an expression of an equal belief that "they would be found to be governed by the same laws of chemistry as regulate even inorganic processes," we must confess that we see not any ground for the induction. When our reviewer's purpose should have been accomplished, he would necessarily have an organized, living apparatus.

But our main object for placing in apposition these latest and ablest advocates of the chemical hypothesis, is to show the common nature of the demonstration; and how full is the admission that the subject must be abandoned in despair.

cause there are but seven notes in music. I myself knew one of that profession who thought that there could be only three parts in harmony, to wit, base, tenor, and treble, because there are but three persons in the Trinity." (1)

And may we not retort upon the chemist what he is said by Condillac to have done with another philosopher who had the happiness of believing that he had discovered a principle which would expound all the phenomena of chemistry? The chemist, after listening to his philosophy, most provokingly told him that there was but one circumstance adverse to his discovery, which was, that the chemical facts were exactly the opposite of what he had supposed. The philosopher, however, bore it as a philosopher should, and asked the chemist to state what the facts were that he might explain them by *his* system. (2) And to the same effect we have the opinion of Lord Bacon, who says of Cicero, that —

"He went about to prove the sect of academicks to be the best; for saith he, ask a stoick which philosophy is true, he will prefer his own. Then ask him, which approacheth next the truth, he will confess the academicks. So deal with the epicure, that will scarce endure the stoick to be in sight of him; so soon as he hath placed himself, he will place the academicks next him." (3)

The philosopher Brown admonishes us emphatically against the propensity of carrying the theories relating to favourite pursuits into other sciences. (4) And thus Bolingbroke: —

"Metaphysical writers counsel us sometimes very gravely to silence imagination, that they may attend to experience, and hearken to the voice of reason. The advice is good, and they would neither puzzle themselves, nor perplex knowledge, if they took it as they give it." (5)

This is the evil; and —

"What we are to guard against in our professional researches and studies, is the influence of partial and confined views, and those favourite notions and speculations which, like coloured glass, distort all things seen through their medium." (6)

"The philosophers being gone," says an eminent writer, (who, in thus waiting for their absence, was less frank, though more courteous than ourselves,) "I observed to *Crito* how unaccountable it was, that men so easy to confute should yet be so difficult to convince. Make a point never so clear, it is great odds, that a man, whose habits and the bent of whose mind lie a contrary way, shall be unable to comprehend it." (7)

What was said by Hillary of the difficulties of medical sci-

(1) On the Powers of the Human Mind, vol. 2, Essay 6, c. 8.
(2) Trait. des Systêmes, t. 2, c. 12. (3) On Good and Evil, sec. i.
(4) Lectures on the Philosophy of the Human Mind, vol. i. sec. 7 and 9.
(5) Works, vol. v. p. 489.
(6) Lawrence's Lectures on Physiology, &c. p. 72.
(7) Berkeley's Minute Philosopher, p. 380.

ence, is mainly applicable to physiology, and is as true of the philosophy of digestion as of any other process of life, and not less practically important.

"It is much more easy to form plausible imaginary hypotheses, than it is diligently to observe the progress of diseases, and their symptoms, and carefully to watch, follow, and assist nature; as this requires both more time, application, and industry, as well as greater penetration, more judgement, and a more extensive knowledge of nature." (1) Or, as Bacon has it, "post quam homines de veritate invenienda semel desperaverint, omnino omnia fiunt languidiora, ex quo fit, ut deflectant potius ad amœnas disputationes et rerum quasdam peragrationes, quam in severitate inquisitiones se sustineant." (2)

"M. D'Alembert considers the genius which distinguishes the truth only when it stares him in the face, as being infinitely inferior to the genius which distinguishes it, not only when near it, but perceives it afar off, by its slightest and most fugitive marks. This seems to be the reason why great mathematicians (and chemists,) have never been great physicians." (3)

Finally, "Medical opinions, which are sanctioned by authority, are received at last as unquestionable truths, which it would be folly to doubt, and presumption to investigate. The bulk of mankind are too timid for the one, and too lazy for the other." (4) "An hypothesis, when once established, becomes like a sovereign, and receives the same homage and respect as if it were truth itself"; or, according to Bacon, "when men once take up with the opinions of others, they no longer improve the sciences"; and "he that rests upon established consent, as the judgement approved by time, trusts to a very fallacious and weak foundation." (5) "Amicus Plato, sed magis amica veritas."

Nevertheless "we have among us moles that dig deep under ground, and eagles that soar out of sight. We can act all parts and become all opinions, putting them on or off with great freedom of wit and humour." (6)

We have wandered into principles of general import, which chemistry may turn against us. But what has it done for the science of life that can justify its doubts of those great conclusions which have been promulgated by the vitalist? Let us once more hear Mr. Carpenter:

"Those who have attended to the progress of chemical science during the last few years," says Mr. Carpenter, "can scarcely hesitate in the belief that we as yet know little of the laws which govern the changes in the constitution of bodies, compared with what future discoveries will reveal to us. Many phenomena of inorganic chemistry, which can now be readily explained, would have been regarded, within a very recent period, as quite incomprehensible. Would it have been thought possible, for example, by a chemist thirty years ago, that the same substance should act the part of an *acid* in one case, and of a *base* in another? — that *water* should be possessed of such properties? — still more,

(1) Hillary's Inquiry, &c. p. 79. (2) De Argument Sci.
(3) Zimmerman on Experience in Physick, vol. ii. p. 25.
(4) Moore's Medical Sketches, p. 216. (5) Preliminaries, s. 1, 8, 10.
(6) Berkeley's Minute Philosopher, p. 376.

that *muriatic acid* should act as the *base* or electro-positive ingredient in combination with the chloride of platinum? These facts would have appeared to a chemist at the commencement of the present century, totally inconsistent with what he knew of chemical action; but they are now readily comprehended, as results of laws which, being higher and more general than those previously known, include facts that at first sight appeared inconsistent with them. Unless, therefore, a distinct set of laws could be established, regulating vital affinities — which has not been accomplished or even attempted — we are scarcely justified in assuming that these laws may not be accordant with those which we recognize elsewhere." (1)

Now in all the foregoing paragraph we hear of nothing but the achievements of inorganic chemistry. There is enough, and justly so, upon that subject. But what does it all prove as to the intended object of the writer? Nothing at all. On the contrary, considering that there has been no lack of zeal or of skilful labourers in the field of organic analysis for the last half century, the very contrast which is drawn by our author proves the Utopian nature of the pursuit. (2) We might bring a multitude of proofs that the same language, the same expectations, have been held forth for the period which we have specified. Here is one of the most indefatigable and distinguished, Fourcroy, who undertook a reformation of medical science and practice upon chemical principles, without ever having read a medical book, or prescribed for a disease. His first memoir upon the subject, says a reviewer,

"Is only an introduction to what the learned author intends to publish separately, from time to time, on all parts of the animal economy on which modern chemistry has begun to throw any light; with a design to point out what is known of that branch of physicks, as well as that with which some men only pretend to be intimately acquainted, to cultivate a field, promising a prosperous harvest, and to confound those enemies who attack the system with their discordant clamours." (3)

The foregoing design was carried into effect. And now let

(1) Carpenter's Principles of General and Comparative Physiology, &c. p. 147.

(2) Inorganic chemistry, however, has separated from itself, more than ever, the science of life, and is rapidly placing an impassable limit between them.

As to Mr. Carpenter's views of the difficulty of "establishing a distinct set of laws regulating vital affinities, — which has not been accomplished, *or even attempted*," (?) we shall only repeat (vol. i. pp. 42, 43, &c.) that those laws are more clearly "established" than any in chemistry, — since the former are known by far more diversified phenomena, which are the only source of knowledge. But, it turns out at last, as in all instances of strong reflecting minds, that Mr. Carpenter is as much an advocate for "vital properties" as the most consistent observer. (See vol. i. pp. 22--28, 48—49.)

(3) Lon. Med. and Phys. Jour. vol. i. p. 145. 1799.

us ask, where is the evidence of the "prosperous harvest" that was so emphatically promised; since which, also, chemistry has made greater advances than any other science, has had its unmolested sway, and Fourcroy's example has been followed with a corresponding diligence? (Vol. I. p. 639.) Can you point to a solitary instance in which organic chemistry, except in a negative sense, has advanced the science of life?

But there was something redeeming, even in the palmy days of humoralism, among the able practical men of that sect. We hear, for instance, Macbride prophesying, that physiologists would fail in their attempt

"To call in the aid of chemistry to enable them the better to understand the nature of the blood, as well as the animal substance in general; since it must be confessed that chemical analysis does not exhibit the true principles, nor does it show even such as are produced in a state of perfect simplicity."

He therefore concludes that medical philosophy, and its practical application, have nothing to hope from chemistry.(1)

Finally, the chemical and vital theories of digestion may be thus summarily stated.

The chemical doctrine supposes that the organic alimentary matter is first "reduced" by chemical agencies, and that then the forces of life take up the process and imbue the new compound with properties analogous to themselves. The former agencies are destructive of organic matter, and especially so when many substances are brought in apposition. It is therefore consistently supposed that the first act, or the "reducing" and disorganizing process may be as well performed by various artificial mixtures, even after they have been subjected to the crucible and to repeated precipitations and reclamations, or by oxalic acid, or in accordance with the analogous process of putrefaction, as by the living gastric juice.

From these premises it necessarily follows that it is not only the province of the gastric juice to organize inorganic matter, but, that it is incapable of bestowing the act of vitalization till the alimentary organic substances are decompounded according to the destructive effects of chemical operations.

On the other hand, the vitalist maintains that the gastric juice is a substance *sui generis*, endowed with vital powers; that it can be only generated by a living stomach, and that it is liable

(1) Methodical Introduction to the Practice of Physick, p. 11. 1772.

to modifications according to changes which may arise in the vital condition of that organ; that it cannot, therefore, be remotely imitated by art, any more than art can manufacture semen, or the cerebral substance; that its properties, like those of every other vital part, are in perfect opposition to the forces and agencies of chemistry, and that it is capable of protecting the alimentary matter against putrefaction and all other inorganic changes; that animal organization cannot organize inorganic compounds, and, therefore, that it is a violation of a fundamental principle in the highest department of nature, to suppose that alimentary matter which has been elaborated by the vegetable kingdom to prepare it for the vital uses of the animal, should be again resolved, more or less, into its inorganic condition before it is appropriated to those uses for which it had been so exactly prepared in its near assimilation to the organic compounds of animals. And, ascending a step higher, the chemical construction becomes more obviously untenable when it is considered that nutriment of an animal nature requires but little more than the most simple part of the solvent process and the bestowment of vital properties; whilst, in accordance with the chemical hypothesis, it must be even more than vegetable matter subject to the disorganizing agencies, and thus completely removed from its original and near approximation to the compounds of the living animal. (Vol. I. p. 594 — 596.) Nor must we neglect the radical distinction, that

" Chemical solution is only a union of bodies by elective attraction, not a real change of the substances themselves, but of their properties. But digestion is an assimilating process. It is a species of *generation;* but the curious circumstance is its converting both vegetable and animal matter into the same kind of substance or compound which no chemical process can effect." (Vol. I. p. 55 — 78.) " Those who took it up chemically," Mr. Hunter continues, " being ignorant of the principles of the animal economy, have erroneously referred the operations of the animal machine to the laws of chemistry."

APPENDIX TO THE PHILOSOPHY OF DIGESTION.

ON SPONTANEOUS GENERATION.

(See p. 95, *note*, and Vol. I. pp. 18, 100.)

"And God made the beast of the earth after his kind, and cattle after their kind, and every thing that creepeth upon the earth after his kind, and God saw that it was good."

"And every plant of the field before it was in the earth, and every herb of the field before it grew. — GENESIS I. *and* II.

IN concluding our remarks upon the philosophy of digestion, it seems to us appropriate that we should add a few words upon the controverted question of the spontaneous generation of animals. It is in the stomach, as it respects the animal kingdom, that the vivification of dead matter begins; nor can any species of matter, in the economy of animals, become endowed with life even without the primary agency of the gastric juice. This fact, with a corresponding analogy in the vegetable kingdom, where inorganic matter must be first organized before it can be exalted to the animal, leads to the irresistible conclusion that the words at the beginning of this Appendix must be received in their most extended sense. The question as to the spontaneity of generation is also intimately connected with the philosophy of our various subjects.

We do not suppose that the motto which we have here prefixed will have any tendency, abstractedly, to enlighten our opponents; but, whilst we examine the subject under other aspects, we hope to introduce some facts that may so far corroborate the sacred historian, that his affirmation, delivered in the words of the Almighty, may come to exert its rational influence.

In selecting, according to our custom, those authors upon the adverse side whose views are best entitled to consideration, we candidly admit that, upon the foregoing question, we have been embarassed in our discrimination. We at first launched among the German transcendentalists; but the unmitigated fancy of one, the bold assumptions of another, and the glaring atheism of others, determined us to hold our argument with a race of men whose genius is always inclined to the truth, and who direct the ultimate destinies of science.

In approaching this subject, we are struck with the paradoxical circumstance that our eminent opponents are themselves amongst the most distinguished cultivators of physiology; and that they should not have found in the infinitely diversified, the amazingly complex, but always harmonious, organization of nature, and in those powers by which her operations are conducted, irresistible demonstrations of one great First Cause. And however impossible it may be for the limited mind of man to comprehend the infinite duration of such a Cause, yet is it by their own premises a rational conclusion, and far more consonant with

the soundest dictates of philosophy: since in the former case every species of matter, every species of animals and of plants, and every power by which they are governed, are supposed to have distinct self-existences. It is for this reason, perhaps, that they have acquired the name of pantheists; but since there is no *theism* in the case, we should prefer in these instances the old denomination of *atheists*.

There is another, but perfectly analogous, aspect of our subject which may be best conveyed by quoting the logic of one of the most learned and eminent physiologists of the day; and we do it more especially as his doctrine of life is consonant with that of Hunter and Bichat.

"The most probable hypothesis," says Tiedemann, "is, that the substance of organic bodies existed primitively in water, as matter of a particular kind, and that it was *there* endowed with the *plastic* faculty; that is to say, with the power of acquiring, by degrees, different simple forms of living bodies, with the concurrence of the general influence of light, heat, and perhaps also of electricity, &c. and of then passing from the simple forms to others more complicated; varying in proportion to the modification occurring in the external influences, until the point where each species acquired duration by the production and manifestation of activity of the genital organs." But mark: "although we cannot here answer the question, *whence* came the water and the organic matter it contained; yet this hypothesis is the one *which accords best with the facts with which Geology has lately been enriched.*" Let Geology answer the imputation!

There are others, again, who appear to be undecided, yet the whole tenour of their reasoning is adverse to a Creative Power, and the one, therefore, which most claims our attention. It is, also, not a little remarkable, that here, as in the foregoing instance, the former surmises in favour of spontaneous generation have been abandoned for the recent developments in geology, and this science thus rendered by many the parent of absurdities in physiology, and subversive of Revelation. Thus, Dr. Hodgkin remarks, that, "although there appear to me strong reasons against adopting this theory, (spontaneous generation,) in the case of most, if not of all our parasitical animals; yet the idea of the production of a *new species*, or, in other words, spontaneous, or equivocal generation, is not to be wholly rejected, without some examination." Had our author finally rejected the doctrine, according to the foregoing intimation, his important influence would have rendered a service to physiology, as well as to religion.

But our author goes on immediately to say, (and we quote the whole "examination,") "We have strong facts to show that the production of distinct species of animals of the higher classes has not been limited to one spot of the earth, or to one period of time. Different parts of the surface of the globe have their distinct and peculiar animals, as well as vegetables. Many animals are found on the American Continent of the ruminant and carnivorous classes, as well as of the Rodentia and Pachydermata, which are quite peculiar to that continent. In Australia we find a still more distinct production of animals, constituting the class of Marsupialia, and comprising numerous divisions. I am unable to conceive any other explanation of this fact, than the natural conclusion, that the several groups of animals were created in the regions in which they are found, and were there adapted to their peculiar localities.

"Arguments to show that spontaneous generation is not impossible."

"The evidence of the creation of animals having taken place at different times is at least as conclusive as the arguments which I have just adduced in favour of their production on different parts of the globe. The researches of the geologists have shown us the remains of numerous animals once inhabiting the earth's surface, but now extinct. The researches of similar labourers have shown, that at the time these animals possessed the earth's surface, those animals with which we are at present acquainted, with some few exceptions, did not exist. It is not merely one, but several changes, with respect to the species of animals inhabiting it, which the surface of our globe has witnessed. Let it not be supposed that these facts, which extend our view of the operations of creative power, can be applied to man, or that they in any degree militate against the Mosaic record. I have merely adduced these facts to show that the creation of organized beings has not been limited to one spot, or to one period; and that therefore it may not be unreasonable to suppose, that *even now new species may be called into existence;* but it would have the tendency to damp our zeal in the investigation of the operations of nature, and turn us aside from the acquisition of truth, were we gratuitously to call in the assistance of such a theory, to solve difficulties like those which obscure the propagation of parasitical animals. Indeed, the facts of the case are strongly opposed to the admission of this theory *with respect to them.* We have seen that the different animals have their peculiar parasites; and in the case of tænia solium, and bothriocephalus latus, that, in different regions, a different species of parasites attacks the same animal; but, having recognised these different species, we find the same species occurring with strict uniformity of character in a great multitude of individuals; which seems to compel one to admit that they are derived from a common origin, and are propagated by generation; since reference to spontaneous generation would almost necessarily presuppose the multiplication of species as well as of individuals. With respect to some of the parasitical animals of which the mode of diffusion was once obscure, we now possess conclusive and satisfactory evidence. The labours of Bracy Clarke have done this for the several species of horse-botts; and no doubt can remain respecting the passage of the oxyuris vermicularis from one person to another." (1) (Vol. I. p. 706.)

To the margin of the last paragraph is appended the index as occurs in our citation; and although our author speaks of the "operations of creative power," the Almighty Power cannot be intended, since it would otherwise amount to no "argument to show that spontaneous generation is not impossible," would furnish no ground of conclusion for the "spontaneous generation" of parasitical animals whose origin may be "obscure," nor would the exception in regard to man be applicable. On the contrary, taking the argument of analogy employed by our author, if it be allowed that the former and present races of animals were the direct work of Almighty Power, it follows that the hypothetical animals are not of spontaneous origin; and by the same rule of analogy, if the former are propagated *ab ovo*, aut, *e vivo*, so are the latter. The analogy is equally good in all the cases; but it is plain that our author intended to predicate the origin of the higher animals of chance, and thus construct an analogy in favour of the spontaneity of parasites. But in the latter case our author is

(1) Morbid Anatomy of the Serous and Mucous Membranes, vol. i. p. 217 — 219.

met by facts, and gives up the analogy which was therefore, in this sense, uselessly produced in favour of the spontaneous generation of parasitical animals, whilst the argument is left in such a position as to involve a proof of the spontaneity of all higher orders of animals. "Indeed," says our author, "the facts are strongly opposed to the admission of this theory, (spontaneous generation,) with respect to *them*, (parasitical animals.) And, then, take the following in its connection: "yet the idea of the production of a new species, or, in other words, spontaneous or equivocal generation, is not to be wholly rejected, without some examination. We have strong facts to show that the production of distinct species of animals of the *higher classes* has not been limited to one spot of the earth, or to one period of time."

Since, then, "the facts of the case are strongly" against the conclusion as it respects *parasitical animals*, let us now make a better use of the rule of analogy, and conclude, if these beings of very simple organization are not of spontaneous origin, certainly those of a far more complex structure must be exempt from that contingency. However it may appear singular that our author should think it not improbable that the higher classes of animals were of spontaneous origin, and that "it may not be unreasonable to suppose that even now new species may be called into existence," but, that the "theory" is less probably true of the parasitical animals, and even "strongly opposed by facts with respect to *them*," it follows, by our author's rule of analogy, (which is a very sound one in the abstract,) that the probabilities of an Almighty Creation of the higher animals are just in the ratio of the like probabilities that attend the creation of the parasitical animals. This analogy may be carried from one class to the other, and vice versa; and we especially hold it forth to all who believe in the Divine Creation of the higher classes of animals, as a proof that parasitical animals depend upon the same Creative Power.

Thus by the showing of the facts in relation to spontaneous generation, the hypothesis must fall by its own internal evidence of its fallacy. Here we might repose the subject with the propagators of the error; being sure, as in all other departures from the light of nature, and may we not say of Revelation, that each succeeding effort to obscure the truth brings it more and more nearly to the surface.

Suppose, in the foregoing case, our author's principle of analogy were allowed to operate according to its original design, the mere negative fact in the superficial researches of geology would not protect the human race against the conclusion, also, which consigns the origin of the higher orders of animals to spontaneous generation. If analogy be worth anything in the intended application of the latter instance, it should be peculiarly strong in respect to man, since he occupies the highest range in organized matter. The position is, that "facts" are against the spontaneous origin of parasitical animals; but, geology shows the probability that the higher we ascend in the animal kingdom, the greater is the proof of spontaneity.

Our author having adverted to the Sacred Record, we will also follow him there. "I am unable," he says, "to conceive any other explanation of this fact, than the natural conclusion, that the several groups of animals were created in the regions in which they are found," &c. This, even admitting their creation by Providence, certainly appears to "militate against the authenticity of the

Mosaic record," — certainly at least as far as relates to existing animals, and beyond this limit we shall not now argue the subject. We are distinctly told that all animals upon dry land were cut off by the deluge, except a male and female of each species, which were preserved in the ark for the propagation of their respective races. What is the obvious final cause of this arrangement? Clearly, that all future terrestrial animals should be perpetuated from the antediluvian, whilst the act of preservation as distinctly implies that there would be no creation of animals afterwards. This obvious conclusion is also distinctly affirmed in the Mosaic Record, since we are told that the Almighty ended the work of Creation on the sixth day; whilst we have no intimation that He ever resumed it, or ever intended to. And that such is the plain construction of the Record is confirmed by the act of preservation in the Ark. It was the obvious design of this preservation, that the species of each animal might continue to be perpetuated according to the laws which were ordained on the day of Creation; and, considering the analogies which prevail in the Operations of Providence, this method of perpetuation must be regarded as a proof that there has never been but one creation of animals, and that no one has sprung into existence spontaneously. This consideration operates, also, as a proof of the Record, since nothing could have been easier than for God to have made another Creation after the flood, and nothing more natural to an impostor than to have affirmed such an act of Creation, whilst the account by Moses is, abstractedly, in the highest degree improbable. But, that all he has stated on this subject is literally true, has been subsequently rendered in the highest degree probable by the absolute silence of all profane history as to the appearance of any new species of animals, or of plants, in countries whose natural history has been long explored.

As to aquatic animals, our author's rule of analogy which he employs on hypothetical data, operates with peculiar force, and leaves the unavoidable conclusion, that the present races are descended from the antediluvian.

Observation thus corresponds with all the foregoing inductions against the doctrine of spontaneous generation. No man has ever been yet able to say, that the species of this animal, or of this plant, or of this earth or metal, or of this simple element, was not created at the time which is indicated by Moses. Even as to the extinct species, the whole proof in relation to their anterior creation rests upon contingencies about which geologists are utterly discrepant. The very extinction of a multitude of distinct species of animals, could the fact be shown, as it certainly cannot, would be a proof of their independence of spontaneous generation; since there are others living which have the closest affinities with the reputedly extinct, yet a whole genus cannot recall the species it has lost. But, that new animals have been created since man began to record his observations of nature is the sheerest hypothesis. The species, too, in respect to organization, remain exactly the same that they were two thousand years ago, have the same functions, habits, &c., no partial transmutation of one into another, whilst the seeds which are preserved with the Egyptian mummies produce exactly the same plants that now flourish, and their flowers emit exactly the same odour. And since it does not appear that the same plan of preservation was observed, or was necessary, in respect to plants, as was adopted with animals at the flood, and since we have no facts to the contrary, but every

reason to believe that no new plants have come into existence subsequently to that era, this is a farther proof of the universal accuracy of the Mosaic account of Creation, and conclusive against the doctrine of the spontaneous generation of animals. If analogy be good in any of our cases, it is especially so where the premises are right, if it be assumed to be good where the premises are wrong.

Let us hear even Bolingbroke, and admire the toleration of our own age. "Whichsover way you turn yourself, you will meet with God. Deum videbis accurrentum tibi, (as Pliny has it,) and may say to the divine what the good man, whom Socrates mentions in his ecclesiastical history, said to the philosopher, my book is the nature of things, which is always at hand when I am desirous to read the words of God; and in that we shall find no foundation for a scheme, like the foregoing. We shall find the course of things to have been always the same. We shall find that He, Who made, preserves the world, and governs it on the same principles, and according to the same invariable laws which He imposed at first. Invariable they are no doubt; and that difference of events, which gives occasion to the distinction of ordinary and extraordinary, is nothing more than the natural effect of them. The laws we speak of are so truly invariable, that the same face of nature, and the same course of things have been preserved from the first, in heaven and on earth, under the direction of the same general Providence. The celestial bodies moved in the same order five thousand years ago, that they move now. The inanimate parts of our globe, the vegetable and animal world have been constituted, maintained, and propagated in the same manner; and whatever difference the most ancient patriarchs, or the first of men, if they were to come into life again, might find in the works of art, they would find none in those of nature." "As little as certain philosophers approve the natural constitution of the world, they are unable to say how it might be altered in any particular, except for the worse; and, therefore, they must be reduced, at least, to assert, that goodness and justice require the whole should be altered, as they required originally that there should have been no such system made." (1)

How the animals that are peculiar to America, or Australia, or, indeed, any others, ever got there is certainly more than we can tell; though not more difficult than how the white man became black. But since, "with respect to some of the parasitical animals of which the mode of diffusion was once obscure, we now possess conclusive and satisfactory evidence," may we not conclude that means were also provided for the "diffusion" of the "higher orders" of animals, even should the darkness of the human mind fail of discerning them?

With very rare exceptions, the advocates of spontaneous generation scout the doctrine of vital powers, and without accounting for the existence of inorganic matter, they not only assume the capacity of physical agencies in the organization of that matter, but in providing it with "properties" which they allow, with mysterious contradiction, to be "vital." And then follows from this imputed prerogative of physical forces the more remarkable conclusion, that they not only relinquish their control over the living beings which they create, but absolutely bestow an ascendency upon those "vital properties"

(1) Bolingbroke's Works, vol. v. pp. 417, 471, 472.

with which they endow the organized machine. Nevertheless, it is maintained that the physical forces carry on the work of life, whilst nothing more appears to be assigned to the "vital properties" than to hold an unintermitting conflict with the former. (See Vol. I. pp. 23, 36, 49, 51, &c.) Here, too, the forces of physics have imposed on themselves a still farther restraint, in rendering animals incapable of generating organic matter out of simple inorganic elements, or even from binary compounds.

Here is another writer, just from the press, an able man, who gives us the whole summary of the doctrine as generally entertained by our opponents. "Those causes," he says, "which are known to operate on the living body, influencing its growth and its functional movements, *if we know nothing to the contrary,* it is most natural to infer *are those which gave origin to it.*" "That the Deity (!) created living beings by the slow and gradual operation of certain laws, instead of calling them into existence at once by a word, we infer from the general analogy of nature." "If, in the second place, the body is formed by certain laws, it is most natural to infer that those laws are the same which affect the body after it is formed, and to which it responds. Now, the laws or agents which affect the body are those of gravity, light, heat, *sound, smell, touch,* &c. Therefore, other things being equal, it is more natural to infer that *they formed the body* than not." (1)

Here we have, as usual, a begging of the whole question, — a denial of what is called the vital or organic force. "If we know nothing to the contrary," says our author, — the "contrary" being the very gist of the proof upon which the vitalist relies for his conclusions. And then as to our author's creative powers, we regard the three first, viz: "gravity, light, and heat," as mere "agents," which excite the vital forces, bring them into action; and as to "sound, smell, and touch," if they must be creative "laws or agents," we think our author will admit that they did not create themselves. But allowing, "in the present state of medical science," the foregoing "laws or agents" to have been the immediate cause of the existence of animals, by which of them, — or by all of them, — does the worm crawl? If by neither, whence came the power of crawling, and what is it? Whence came the understanding of man, and what is it? Is it "gravity," vis inertiæ? What was it that created those creative "laws or agents." Perhaps the "Deity," by His immediate Fiat. Is there, then, nothing abrupt in these instances, — nothing to supply some analogy that conflicts with the "general analogy of nature," — nothing to show that there is no violation of analogy in denying that animals and vegetables came into existence, originally, "by the slow and gradual operation of certain laws"? Allowing, then, as it must be, that the physical powers, and the foregoing "laws and agents," and inorganic matter, were the results of One Great Creative Power, He certainly might have endowed them with the property of generating animals and vegetables. But, taking the analogy which our opponents employ with so little hesitation, the conclusion that He did so endow them debases philosophy, and the mind itself; since all that we know of the forces of physics, and of inorganic matter, assures us that the former presents nothing analogous to the phenomena of life, and that the latter is utterly at variance with organization. The theory, therefore, supposes the creation of a

(1) Dr. Haskell, in New-England Med. and Surg. Journ. No. 20, 1838.

structure and of powers, of which the agents are totally destitute. Take even the most opportune condition of matter for its vivification, an organized machine of the most simple or the most complex structure, either animal or vegetable, as soon as the principle of life is extinguished; can any physical power, or any known agent reanimate that machine? On the contrary, the more those forces and agents are brought to operate upon it, the more rapidly is it resolved into its primary elements. (Vol. I. p. 92.) It follows, therefore, by that philosophy which guides our adversaries, that all organized matter was, primarily, a direct emanation from That Power Who created the forces of physics and inorganic matter; and, in the words of Byron, —

"Such as Creation's dawn beheld, thou rollest now."

But, "all matter has certainly arisen out of the globe, for we find it returning to it again"; (1) and this is another proof of the truth of the Mosaic Record of Creation and of our position.

The most objectionable part of our author's logic, and of his school, is that which does away with the primary Acts of Creation. Who does not stand appalled, when employed in this process of reasoning, as it flashes upon his eye, and into his soul, — "AND GOD SAID LET THERE BE LIGHT; AND THERE WAS LIGHT." Nor was it alone the speaking into instant existence the light of day which distinguished the beginning of Creation. In the same way God soon after created that intellectual light, which was probably intended in part to enable man to appreciate the other Acts of Creation.

Who does not fear, that when those slowly creative "agents," shall have ceased their vivifying operation, and shall have again laid their fabric in ruins, that this may be the last of the history of man!

We turn for a moment to another distinguished source, and with the greater satisfaction, as the subject is presented strictly in relation to organized matter of a low grade, and as generated by prëexisting organization of higher degree.

It is suggested "as an important question in *general physiology*, whether plants or animals of a high degree of organization are capable of producing from various parts of their tissues beings corresponding to those of the inferior orders of their kingdoms." (2) This proposition is profoundly interesting to a numerous class of our most able inquirers. (See Vol. I. p. 706.) Without again referring to what we have said in opposition, when speaking of the microscope, it appears to us that the query may be answered by the very philosophical analysis in which it occurs; and perhaps we have already said enough when considering the subject in connection with the argument in favour of spontaneous generation as derived from geology.

But this question may be tried on strictly physiological grounds. Every inferior animal or plant is as perfect, according to its species, as the most complex individual, whilst, also, its organization is essentially different. According to the analogies in developmental anatomy there should be, at least, a close affinity between the organization of the supposed parasite and that of the tissue from which it springs; and this, indeed, is universally true of all that is known of healthy formations. The products which follow wounds, in any part of the

(1) Hunter's Lectures on the Principles of Surgery, p. 13.
(2) British and Foreign Med. Review, No. 7.

body, partake of the nature of the tissue in which they occur. Even in grafting, "it is well known that it is essential to the success of the process, that there should be a *close alliance in species* between the two plants to be united;' that "there should be an analogy betwixt the modes of growth in the stock and graft." (1) Some animals are propagated by dividing them into pairs. But the organization of each is the same. Again, others have the power of forming and casting off small portions of organized matter; and, although these detached parts undergo a variety of transmutations, they come at last to present an exact resemblance of the parent. And so of vegetables. And, as every part thus abstracted has its organization from the very incipiency, it is clearly absurd to assume the phenomenon, as has been done, in proof of spontaneous generation.

Nor is similarity of organization alone necessary; since it is "hence inferred that the ascending sap varies in composition in different families." (2) And if similarity of sap be necessary to the success of grafting, how little probable is it, and how opposed to physiological facts, that the blood of "an animal of high degree of organization," would be fitted for the development and growth of "beings corresponding to those of the inferior orders" of this kingdom; for we take it, by the supposition, the blood must be the common source of nourishment. Indeed, we have already shown, (Vol. I. pp. 650, 698, &c.) that the blood of animals belonging to different genera is fatal to each other, and by no fact or sophistry can it be shown that animals very remote from each other in organization are allied in vital constitution. We say nothing of the objection which relates to the want of analogy betwixt the imaginative source of *production* and that in which "the corresponding beings" originate, in their ordinary modes of *reproduction*. When one plant of a different species or genus springs from another, it originates from roots, which are capable of decompounding and assimilating nutriment in a very different manner from animal organization; and the sap of the sustaining plant bears the same relation to the parasite as the water, &c. of the earth does to the former. It is utterly changed in its character as soon as it enters the roots of the parasite, and is the same as appertains to it when springing from the ground.

We agree with our reviewer that "the rule is certain, that plants, for want of culture, degenerate to be baser in the same kind", though not in organization; nor, according to Bacon, that "they sometimes change into another kind." (3) In the latter instance, although a conclusion of the great lawgiver in philosophy, we cannot but think we should be giving new laws to Nature. Such, too, were the dreams of De Maillet, of Buffon, La Manch, and of others blest with the light of later science, who would carry back the existing races of animals through a descending series of transmigrations, till they arrived at the original being in the state of an animalcule, or, as Tiedemann has it, the matter of organization. But this doctrine is only a parasite of the hypothesis of the self-existence of matter; both alike in organization, and both equally a contradiction of all experience, of all philosophy, of all reason, and of Sacred History.

It becomes, also, an easy matter to apply the foregoing considerations to the formation of monstrosities, and without adverting to the want of all analogy in

(1) British and Foreign Med. Rev. No. 7. (2) Ibid.
(3) Bacon's Natural History, vol. i. p. 202.

the known products of inflammation, they are equally conclusive against the hypothesis that the real hydatid, as originally suggested by Viet, (1) and Jager, (2) is the product of inflammation. Bremser, (3) and Rudolphi, (4) adopting this opinion, proceed to argue by a most defective analogy, that intestinal worms are of spontaneous origin. They deny, however, the specific vitality of the acephalocysts, which destroys all the attempted analogy betwixt them and the lowest animals and their modes of formation. There is wanting, too, an analogy in the process by which the reproduction of parts in the inferior animals takes place, which never depends on inflammation.

In respect to the numerous genera of animals which are supposed to be bred in every tissue of the human species, (Vol. I. pp. 701, 706, &c.) let us look at a single example, and let analogy determine the question. The itch insect is said to belong to a tribe which propagate themselves by eggs. This would be sufficient to bring the cutaneous species under the general rule. But we have the authority of its original discoverer, Bonomo, (Vol. I. p. 706,) for saying that he saw it "drop white eggs, like a pineapple seed, from its hinder parts." Allowing, therefore, the real existence of this fabulous animal, shall we not conclude that all the others are propagated in the same way, and that, having a fabulous existence in the external world, their eggs are introduced from without into the human circulation?

If we allow of spermatic animalcula, they supply no ground of analogy to the question before us. Their generation would then result from an essential part of the constitution of animals, and must be classed among the other organic formations of every species.

There is yet another, and a numerous class of philosophers who have lately introduced some new laws into physiology. These we have variously considered in different places, especially in our Vital Powers, and Humoral Pathology. They mainly construe the actions of life in accordance with the forces which prevail in the world of dead matter. In relation to our present inquiry, they suppose, in the language of M. Andral, that fibrinious concretions "possess an *independent* vitality, by means of organs they have themselves created." Upon this violation of fundamental laws in physiology, some have proceeded to argue in favour of the spontaneous generation of animals; and M. Andral "compares the concretions to zoophites." (5) (See our Essay on *Venous Congestion*, Sec. IX.)

Although we are disposed to give a liberal construction to the Holy Scriptures, we think there should be no violation of any direct statements which they make, however they may appear incapable of explanation, or adverse to the researches, or the learning, the philosophy, or the ambition of man. In our investigations of the works of nature, it should ever be a primary object to render our discoveries subservient to the Revelation which respects Creation, and the extension of true philosophy will surely follow. And, should we now and then meet with apparent obscurities, they should be rather regarded as tending to establish our general position, since it is God alone who is the Author of

(1) Archiv. fur die Physiol. t. 2, p. 436. (2) Ibid. t. 6, p. 495.
(3) Trait. Zoolog. et Physiol. sur les Vers intest. de l'Homme. (4) Entozom. Hist. Nat.
(5) See Dr. Hughes on Fibrinious Concretions, in Guy's Hospital Reports, April, 1839.

mysteries ; and whenever they have been clearly expounded, they have always appeared consistent with whatever had been known of His Providence, and the most obvious import of Revelation. "It is the glory of God to conceal a thing, and the glory of the king to find it out." But, above all does it behoove the geologist, the physician, the chemist, and all others who are employed in the investigation and interpretation of nature, to be faithful to the lofty trust which is committed to their care. They should be cautious in breaking up the great chain of Creation, and in reducing the noble parts to the most ignoble. It appears to us that it is to the adverse doctrines, leading to the hypothesis of spontaneous generation, &c. that medicine is mainly indebted for its reputed tendency to substitute skepticism for true religion. Least of all can any philosophy endure which is opposed to the fundamental acts of Creation, because it would not then be founded upon Nature. Whoever, therefore, may be an unbeliever will find it for his interest as a philosopher, to admit the Attributes of a Creative Power. We are fully sensible, however, that, in the ardour to account satisfactorily for anomalous events, we may unintentionally misinterpret the established order of nature. Of this character, doubtless in some quarters, is the philosophy that recognises spontaneous generation, but which appears to us no better than placing chance and the Almighty on an equality. It must, therefore, fall; and, although no such evil intention have existed, it nevertheless impedes the progress of truth, and brings an odium upon natural philosophy. Equally, too, may we deprecate that ambition which professes to have generated animals out of silex by the aid of galvanism. This is alike the climax of irreligion, for it takes even from "nature" the prerogative of creating after her good will and pleasure, and substitutes man as her chief engineer. And when a general affirmation, like the following, is made of the philosopher who is concerned about the most sublime work of creation, and in opposition to the plainest phenomena of nature, the less speculative part of mankind are apt not only to believe that natural philosophy tends to skepticism, but to take the declaration as it stands. We allude to Dr. Prout's statement, that "the desire of the physiologist to ascribe to the agencies of *inorganic* matter those operations carried on within living bodies, is only a display of that innate propensity of the human mind, which leads to seek after first causes." (1) And, referring once more to Dr. Prout's system of life, as cited in our 1st vol. p. 36, we see not why the term *life* should not be equally appropriate to any process that may be conducted in the laboratory. At least, we have a jealousy in this matter, and should a little rather that the animal chemist would *abandon life*, and try and get on with the help of chemistry alone. The experiment, we think, would bring him over to the vitalists, and make him well content with their system of life.

Still more to the foregoing effect is the manner in which Müller has filled up his *warp* of life with the woof of materialism. This we have stated in our Vol. I. pp. 85, 86, and *note*. In answer to his interrogatory in that *note*, we reply in the sublime language of the heathen philosopher, that the soul wears out her suit of clothes, and then departs to the world of spirits. (2) This is the philosophy of Lawrence, and Elliotson, who, throwing of all disguise, would bring the religious sense of the age below that of the Grecian and Roman phi-

(1) Bridgewater Treatise, Book 3, c. 4. (2) Socrates, Vide Phæd. in Platon.

losophers. Employed about those pursuits which forever lead them "to look through Nature up to Nature's God," there can be no pretext for the materialism with which they have imbued their expositions of the laws and phenomena of nature.

We claim not the right to molest any private opinions; but when fundamental doctrines are obtruded upon the world, and this, as we shall see, in ribald derision of holier views, we exercise but a common right in protecting, at least, against the general imputation of such opinions those observers who are engaged in the culture of natural philosophy.

Mr. Lawrence's sentiments are well known to the medical public, from whom in respect to his genius and erudition, they have received an unwonted indulgence. We shall, therefore, only present one of his best examples of substantiating his views. "Some hold," he says, "that an immaterial principle, and others, that a material, but invisible and very subtle agent, is superadded to the obvious structure of the body, and enables it to exhibit vital phenomena." This is all very well; but now comes the application in a higher sense. "The former explanation," he continues, "will be of use to those *who are conversant with immaterial beings*, and who understand how they are connected with and act on matter." Our author then proceeds to make an indefensible application, as it appears to us, of Mr. Hunter's doctrine of the vital principle, and having thus brought it in aid of his own opinions, dismisses it under a shower of ridicule. (1)

After a similar strain of burlesque about the existence of the soul in the human fœtus, he concludes that "the Roman Catholic Church has cut the knot, which no one else could untie; and has decided that the little mortal, on its passage into this world of trouble, has a soul to be saved. It accordingly directs and authorizes midwives in cases of difficult labour, where the death of the infant is apprehended, to baptize it by means of a syringe, &c., and thus to save it from perdition." (2) Little as we may approve this singular doctrine, if it be really practised, it affects in no respect our author's intended slur upon every thing that is connected with religion and the moral dignity of man.

But, our author has one specious argument which deserves some notice. "If," he says, "the intellectual phenomena of man require an immaterial principle superadded to that of the brain, we must equally concede it to those more rational animals which exhibit manifestations differing from some of the human race only in degree. If we grant it to these, we cannot refuse it to the next in order, and so on in succession to the whole series, — to the oyster, the sea anemone, the polype, the microscopic animalcules. Is any one prepared to admit the existence of immaterial principles in all these cases? If not, he must equally reject it in man." (3)

In our essay upon the Vital Powers, we endeavoured to show that brute animals are endowed with an immaterial principle, which, in them, is the cause of instinct, as the soul is of the manifestations of mind in the human race. We also endeavoured to show how they are contradistinguished from each other, and how the former is probably perishable, whilst the soul is immortal. (See

(1) Lectures on Physiology, &c. p. 78, &c. It is remarkable that Dr. Elliotson has done the same thing; — having rendered Mr. Hunter subservient to his own interest, he turns him adrift after the example of Mr. Lawrence. — *Human Physiology*, Part I. p. 31, *note*.

(2) Ibid. p. 97. (3) Ibid. p. 99.

Vol. I. p. 86 — 106.) We shall not, therefore, review this ground, but limit ourselves to a few remarks upon our author's argument.

The reasoning of our author is analogical, and we should admit the great force of it in establishing the identity of the soul of man and the instinctive principle of "the highest order" of brutes, were it not manifest that our author has assumed a basis for his analogy which has no existence. But, there are certain phenomena common to both, which show us the immateriality of the instinctive principle, according to our proof in the foregoing place. (Vol. I. p. 97 — 98.) We agree perfectly, therefore, with our author, though upon other analogies than assumed by him, that if we reject an immaterial principle in brutes that manifest any of the phenomena which are common to the spiritual part of man, we must equally reject in man also. But, this in no respect implies that the immaterial part is the same in both; whilst, on the contrary, as we have endeavoured to show, their constitution is distinct.

Starting with the fact that man is endowed with an immaterial soul, and admitting the assumption of our author, that certain animals of the "highest order" "exhibit manifestations of reason differing from some of the human race only in degree," we should then be inclined to believe that such animals are endowed with an immaterial principle more allied to the soul of man, than we have been disposed to admit upon truer premises. But our author has not only violated the fair ground of argument in assuming premises that do not exist, but he has even carried his proof from analogy far beyond its most absolute limit. For, he says, "if we grant an immaterial principle superadded to the brain of those more rational animals which exhibit manifestations differing from some of the human race only in degree, we cannot refuse it to "the next in order," and so on down to the "microscopic animalcules." Here, then, by admission, all analogy ceases as soon as we leave "those *more rational* animals"; and, according, therefore, to our author's own reasoning, the rule is only applicable to "those animals," and not to such as exhibit "no rational manifestations." The argument falls of course, upon its own ground.

But did our author ever reflect, that his premises, ("manifestations of reason" being the basis,) and his mode of reasoning may be carried on in the other direction, and that angels and the Almighty Being must come in for their share of his materiality?

> "Of systems possible, if 'tis confess'd
> That Wisdom Infinite must form the best,
> Where all must fall or not coherent be,
> And all that rises, rise in due degree;
> Then, in the scale of *reasoning life*, 'tis plain,
> There must be somewhere, such a rank as man;
> And all the question (wrangle e'er so long)
> Is only this, if God has placed him wrong?" (1)

Seeing, therefore, that animals evidently stand in a class distinct from man, and that the rule of analogy should be applicable to the instinctive principle among all animals, and that, if there be any manifestations of reason amongst the highest order, they disappear as soon as we enter the lowest ranks, we infer, according to our author's rule of induction, that what are assumed to be "manifestations of reason in the highest," are, in truth, only the manifestations

(1) Pope's Essay on Man.

of that principle of instinct which loses its illusory signs of reason among lower orders. Indeed, the natural understanding of our author finally conducts him to this very conclusion, by which it appears that he was not sincere in his representation of the affinities betwixt the mind of man and the instinct of "superior animals;" since in his luminous discussion of a question more strictly physiological, he affirms that, "though the external senses of brute animals are not inferior to our own, and *although we should allow* some of them to possess a faint dawning of comparison, reflection, and judgment, *it is certain that they are unable to form that association of ideas in which alone the essence of thought consists.*" And again, "the *peculiar characteristics* of man appear to me *so very strong*, that I not only deem him a distinct species, but also put him *in a separate order by himself.* His physical and *moral* attributes place him at *a much greater distance from all orders of mammalia, than those are from each other respectively.*" (1)

And so it is with every writer who has attempted to debase the human soul, or the Attributes of its Creator. Analyze their arguments, and you find them a tissue of assumptions, which they fully admit by their palpable contradictions as soon as they come to the investigation of some kindred, but more philosophical question.

Our author "willingly concedes to every man what he claims for himself,— the fullest range of thought and expression; and it is perfectly indifferent whether the sentiments of others on speculative subjects coincide with or differ from his own." (2) This might imply a *right* to propagate such doctrines as we have just examined. Were they of a "speculative" nature, and had no tendency to impair the intellectual, religious, or physical condition of man, the right could not be disputed, but would be more or less commendable according to the opposite tendency of such "speculations." But when an author comes forward with speculations that strike at the foundations of society, and the future well being of man, and brings upon his associates in the walks of philosophy the imputation of infidelity, he cannot plead the right of independent thinking. We have no objection to his forestalling the critics as to opprobrious epithets, in saying that "the practice of calling hard names in argument has been chiefly resorted to by the fair sex, and in religious discussions;" (3) but, we do complain that, when he assails the settled opinions of the Christian world, — we had almost said the *heathen*, — he is regardless of his own rule, and substitutes, on almost all occasions, sarcasm for facts and argument.

As might be expected, and as prevails in all analogous cases, our author's system of physiology is imbued with derision of the Hunterian philosophy of life, and abounds with maledictions upon all but the prerogatives of dead matter. This construction of the properties of life, we may charitably believe, was antecedent to his adoption of materialism in a higher sense; and we put it forth among other instances, as showing the probable tendency of the physical sciences, when applied to the science of life, to lead to skepticism in graver matters. The progress towards infidelity is always slow, — at least apparently so in a Christian land; and, whenever the consummation may take place, regard for reputation, and a more successful propagation of the doctrine, will surround it with reservations, insinuating analogies, and perhaps with some

(1) Lectures on Physiology, &c. pp. 203, 117. (2) Ibid. p. 15. (3) Ibid. p. 21.

show of religion, either for the affected purpose of an insinuating impartiality, or to furnish a loophole of retreat, should the enemy crowd hard.

The last eminent physiologist to whom we shall pay our respects, as having contributed some share to the general charge of infidelity against natural philosophers, is Dr. Elliotson.

We certainly agree with this writer, that the brain is the organ in which the soul, if there be any, is seated, and that without brain there would be no acts of intellection. It is also manifest, that the *fullest* exercise of mind depends not only upon the general integrity of the brain, but on its perfect development. It is, too, an equally rational conclusion, that vigour of mind will be more or less commensurate with the size of the organ; and we think it probable that the phenomena of intellection may be variously affected by the varying proportions of different parts of the brain. There is nothing in all this that leads to materialism. On the contrary, to regard the soul as a specific existence, or immaterial substance, implanted in the brain, and acting in cöoperation with it, we think, in a general sense, is entirely supported by the common plans of Providence. And, although our author admits the possibility that "something immortal, subtle, immaterial, may be diffused through the brain," and that his "assertion does not imply that the resurrection from the dead is impossible, or even improbable"; (1) nevertheless, the whole gist of his argument is intended to establish the doctrine of materialism. This concession of possibilities, and even an apparent leaning towards Revelation, are matters of expediency with this class of writers, who have sufficient tact to understand that it is the only method by which they can obtain a hearing from sincere believers, or carry their stratagem with the unsuspecting.

"A physical inquirer," says Dr. E. "has only to do with what he observes. He finds this power, (the mind,) but attempts not to explain it. He simply says the living brain has this power, medullary matter though it be." "In contending that the mind is a power of the living brain, and the exercise of it the functions of that organ, I contend for merely a physical fact; and no Christian who has just conceptions of the Author of Nature, will hesitate to look boldly at Nature, as she is, lest he should discover facts opposite to the pronunciations of revelation; for the words and the works of the Almighty cannot contradict each other." (2)

Now, such may be the habits of the mere "physical inquirer;" but they are not those of the rational physiologist. The latter observes in the phenomena of mind a thousand indications that a substance is connected with the brain upon which the intellectual results of that organ mainly depend; and this is forcibly demonstrated by the disappearance of all those phenomena when death is produced in such a way as shall in no respect affect the organization of the brain. Nor can we permit you to argue, that so, also, disappear the organic functions of the brain and the whole assemblage of vital actions. On the contrary, such is the radical distinction betwixt the *organic functions* of the brain and the *acts of intellection*, that there must be great obliquity in him who does not see that they depend upon existences which are totally distinct. That the soul and the vital powers disappear together at the moment of death, probably arises from the fact, as we have endeavoured to

(1) Human Physiology, Part I. pp. 39, 41. (2) Ibid. pp. 38, 39.

explain in another place, of the former being connected with the brain through the medium of the latter; and it is upon this circumstance that we have founded an argument as to the specific nature, and the real existence of the vital forces. (Vol. I. p. 96.)

Again, the manifestations of mind, by admission, appertain to the brain, nor can any other part of the body produce a single act of intellection. But, the brain enjoys, also, in the highest degree, the powers and functions that belong to other complex organs,—has its circulation, nutrition, secretion, and presides, more or less over the organic functions of other viscera. All these are manifestly *organic* functions, which have their analogies in various other parts. There is *something*, however, superadded to this organ, to which there is nothing analogous in the rest of organized matter; whilst all other organs have the plainest analogies in their several functions. It is clear, therefore, that the phenomena of mind are the result of the combined action of that *something*, which rational philosophers call the *soul*, and the material part. The same arguments which are employed in another place to show that the powers of life are *something*, and not a mere matter of fancy, are equally applicable for demonstrating the real existence of the soul as contradistinguished from *nothing;* and we think the proof is the same, and as palpable, in one case as in the other.

The sarcasm of our author, therefore, in the following involved sentence, is neither justified by the nature of the subject, nor by his own professions. "Seeing," he says, "the brain thinks, and feels, and wills, as clearly as that the liver has the power of producing bile, and does produce it, and a *salt* the *power* of assuming a certain form, and does crystallize, (observe the parallels) the physical energies leave others at liberty to *fancy an hypothesis* of its power being a subtle, *immaterial*, *immortal* substance, *exactly as they fancy* life to be a subtle fluid, if subtlety is immateriality and immortality; elucidating the subject no more than in the case of life, and equally increasing the number of difficulties; as though we were not created beings, or not altogether ignorant what matter is, or of what it is capable and incapable; as though matter exhibited nothing but extension, impenetrability, attraction, and inertness"; (1) and so on to the end of the chapter.

And yet our author has the temerity to say, that his "assertions" do not imply "that this power (the mind) cannot be a something immortal, subtle, immaterial diffused through and connected with the brain."

Finally, although we think "the argument of Bishop Butler, that the soul is immortal and independent of matter because in fatal diseases the mind often remains vigorous to the last," is not true in its limited import, we do not consider it, with our materialist, "perfectly groundless." (2) There certainly can be no doubt that the mind is apt to be impaired when the organic functions of the brain are disturbed; yet, who has not seen the former unimpaired during the most extensive organic lesions of the latter, and far beyond what has ever been known of the functions of the liver, the stomach, and the heart, in analogous lesions of those organs? (3) Who does not know, that in the worst forms

(1) Human Physiology, Part I. p. 39. (2) Ibid. p. 37.

(3) We shall present a few examples, mainly derived from a single periodical.

In the Medico-Chirurgical Transactions, (a) Dr. Sims has furnished a table of 50 cases where

(a) Vol. 19.

of the cholera asphyxia, and in many cases of the dying, when all organic functions have nearly ceased, the mind often retains the exercise of its powers?

It appears, therefore, that an almost expiring state of the vital forces is sufficient to maintain the connection between spirit and matter, so that the former shall continue to manifest its functions fully, when those of the latter are nearly extinct. (See Vol. I. p. 96.) Under these circumstances, we must allow that the soul may perform its functions actively, with a very feeble cöoperation of the material part. We have stated something analogous to this in respect to the vital forces, where the dried, and inflated heart of a fish pulsated till it crackled like parchment. (Vol. I. p. 17.) There are difficult mysteries attending these associates with common matter; but the more mysterious their ways, and discrepant with the ordinary results in physiology, the greater is our reason to consider them as distinct from the matter itself.

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there was an effusion of serum or blood within the head, but no cerebral symptoms. — "A morbid structure of the brain," says Hutchinson, "may exist a long time without much inconvenience to the patient" (*a*) — "The brain," says Yelloly, "has a remarkable power of accommodating itself to a gradual derangement." (*b*) — Stranger relates a case in which no cerebral symptoms took place till a year after a blow on the head, when the patient suddenly died with coma. "Ten or twelve ounces of pus were found in the brain, and the vertebral canal was filled with the same fluid." (*c*) — The celebrated Saussure was affected with extensive disorganization of the brain for the space of five years without any sensible alteration of the intellectual powers. (*d*) — Mr. Hunter, jr. states a case of fungus hematodes cerebri of four years duration, where the only remarkable symptom was an abolition of the senses. (*e*) — And so Cruveilhier of caninomatous tumours of the brain. (*f*) — In a case by Howship, four ounces of medullary substance were displaced by the presence of a tumour, without occasioning any remarkable symptoms. In another instance, in consequence of a slight blow on the head, the whole middle lobe of the brain was found in a state of scirrhus forty years afterwards. But, with the exception of occasional pain, the subject had no other symptoms till towards the decline of life, when she became gradually sleepy and stupid. (*g*) — Mr. Earle has related a case of extensive abscess of the brain, which was unexpectedly discovered on dissection; (*h*) and, in another work, he presents an instance where seven large tumours of a firm consistence, (the largest of the size of an orange,) were found in the brain of a child who had no cerebral symptoms till about ten days before death. (*i*) Dr. Rodgers relates, that the breech-pin of a gun, "three inches in length, and exactly three ounces in weight, was lodged in the brain for the space of 27 days, without coma or any serious mischief," — the patient having also lost a portion of his brain. (*j*) — "Lamotte gives a case where, by the stroke of a sabre, the scull was deeply cleft, the right parietal bone to the depth of two inches, and the left to between three and four, nearly down to the ear. This severe wound was cured in less than three months." (*k*) — "S. W. aged 17 years had his head fractured by the limb of a tree, four and a half inches in diameter, which had fairly imbedded itself in the upper part of the head. The whole arch of the cranium was shattered into small fragments, from two inches square down to half an inch. To make him look more seemly, I put his head into something like its original shape, and left him to die." In three days after, a messenger was sent for the physician, who found his patient perfectly sensible; and from that time his convalescence went rapidly on. (*l*) — Here is a case which interests, also, certain physiological and phrenological doctrines. A lad, aged 11 years, received a kick from a horse, which fractured the os frontis. "In two hours after, he recovered every faculty of his mind, and they continued vigorous for six weeks, and to within an hour of his death, which took place on the 43d day." "He sat up every day, often walked to the window, frequently laughing at the gambols of the boys in the streets," &c. On dissection, in presence of other physicians, "the space of the scull, previously occupied by the *right anterior and middle lobes of the cerebrum*, presented a perfect cavity, filled with sero-purulent matter; the lobes having been destroyed by suppuration. The *third lobe* was much disorganized. The *left hemisphere* was in a state of *ramollissement* down to the corpus callosum. (*m*) — This case should be compared with the celebrated one by O'Halloran, where there was great destruction of the brain without any derangement of intellect.

(*a*) Ibid. vol. iv. p. 203. (*b*) Ibid. vol. i. p. 218. (*c*) Ibid. vol v. p. 24. (*d*) Ibid. vol. vii. p. 211. (*e*) Ibid. vol. xiii. p. 93. (*f*) Anat. Patholog. liv. 8. p. 4. (*g*) Obs. in Surgery and Morbid Anat. pp. 78, 89. (*h*) Med. and Phys. Journal, No. 132. (*i*) Med. Chir. Trans. vol. iii. p. 59. (*j*) Ibid. vol. xiii. p. 231. (*k*) Hennen's Military Surgery, p. 284. (*l*) Western Jour. of Med. and Phys. Science, vol. viii. p. 213. (*m*) American Med. Intelligencer, vol. i. p. 1. See, also, another case in *Ibid.*

The discussion with which we began this appendix naturally conducted us to that of "materialism." The subjects being intrinsically of a popular nature, we may, for a moment, descend from the altar of science and approach the precincts of the pulpit. This we do for the purpose of saying that physiology should become an element in the education of clergymen. The enemy of religion, or the well meaning cosmographer, takes advantage of your want of familiarity with this department of knowledge. They tell you that the living system has no forces peculiar to itself, but that it is wholly amenable to such as rule in the inorganic world; and they conduct you at last, by these premises, to an almost irresistible admission that living beings may be created by their power. And we have already shown you, when thus prepared, how easy a matter it is to spread before you, without greatly shocking the religious sense, a plan of creation which ascribes the origin of animals to "spontaneous generation," as it is called in preference to "chance."

The steps are gradual from the incipient errors in natural Philosophy to a disbelief in the Mosaic Record of Creation. When we have ultimately reached this brink of the precipice, there is but one dreadful plunge, and we are then in the vortex of atheism. We may begin, as we have said, by a simple denial of the living powers of organized beings, when it will become, at last, an easy argument upon this, and analogous premises, that the Almighty had but very little, if any agency, in the most sublime part of existences. But, when you shall look at physiology in its true aspect, you will see that the living, organized kingdoms are governed by laws totally different from anything that is known of the inorganic. This will assure you that there can be no "spontaneous generation," — that the forces of physics can have had no lot in the creation or in the perpetuity of animals; but, on the contrary, it is their work *to lay waste the whole fabric of Creation.* You come, then, to enjoy the undisturbed conviction, that the creation of every original species of animals was a special Act of God, and that they are in every vital sense contradistinguished from inorganic matter. And when you shall have thus studied Nature as she is, you will find her in perfect harmony with your religious impressions; nor can she fail to exalt your religious fervor.

When, however, comparatively little was known of the laws which govern organized matter, the Sacred Record was the basis of faith, and skepticism was less indebted for aid to physiological speculations. The operation of the latter was essentially negative, till science became *the work of all,* and its principles were everywhere brought into practical application.

Let *philosophy* interrogate nature to its fullest satiety, under the direction of its heaven-born principles; but, let it be consistent, and maintain its dignity. And should it sometimes, as it must in its wide range of nature, come in contact with miracle, — this is its limit, contented that it begins at the confines of Creation; yet, still may it stretch into the regions of Eternity, — past and to come; but now it is employed in its nobler work of sacrificing its relations to second causes, and in establishing relations with the First Cause of all.

ON THE THEORIES OF INFLAMMATION.(1)

"The act of inflammation appears to be an increased action of the smaller vessels. It is commonly supposed to be contraction of the vessels; but I have shown that their elastic power also dilates them; and I have reason to believe their muscular power has a similar effect." — HUNTER *on the Blood, &c.* (2)

SECTION I.

IT is usual, at least with many, to begin the consideration of the theory of inflammation with an account of the blood-vessels, the circulation of the blood, and other anatomical facts, and to bring up the preface with a multitude of experiments where nature may be seen in her most artificial aspects; but, most of all, with microscopical explorations. To the latter, under the auspices of the physical theories of life, are owing, mainly, the mechanical doctrines of inflammation. Since the time of Hunter and Bichat, we hear but little about the philosophy which relates to the vital powers, either in the processes of disease, or in the healthy actions of the system. Consequently the inquiry is apt to terminate in the simplicity of physical results.

The human organization is either regarded as "a chemical laboratory," or, "our philosophy would teach us that there is but little difference between a man and a sieve." The mechanism is considered the agent itself; whilst the invisible existence which truly presides over all the movements of life, and whose phenomena are so multifarious as to defy enumeration, and which no other power can resolve, is ridiculed as a creation of

(1) The substance of our remarks on the Theories of Inflammation was published in the American Journal of the Medical Sciences for May, 1838.

(2) P. 278.—We use the ed. of 1794.

fancy, because, like the more sensible parts, it is not a subject for the scalpel or the microscope. (See Vol. I. pp. 11, 33, 36, 75, &c.)

In prosecuting our inquiry, we shall be content with the knowledge that the process of inflammation is conducted by blood-vessels and other inscrutably organized instruments; but most of all, that these instruments are actuated in all their movements and results by powers which have no analogies in inorganic matter. And were it our specific purpose to construct a theory of inflammation, we would take especially the vital phenomena as the basis. To these we would make the physical appearances subordinate; yielding their aid where susceptible of interpretation by our standard of life.

But, it is less our object to substitute a theory of inflammation, than to speak of the fallacies by which the Hunterian doctrine has been opposed; and although we shall defend the vital, and therefore active, nature of this disease, we shall employ ourselves mainly about the physical facts in which the mechanical doctrines have had their origin. By these facts we shall try the hypothesis; and if they cannot abide their own test, the theory of vitalism must unavoidably ensue. In a practical sense, if we except the humoral pathology, and, perhaps, venous congestion, there is no question in medicine of equal importance, and none which has so extensively exercised its sway as the theories of inflammation.

It is also our object to render this article subservient to that which follows, on venous congestion. In treating of the latter disease we shall often have occasion to borrow illustrations from the phenomena of common inflammation. But these facts are more or less invalidated by the doctrine which imputes the pathology of inflammation to a mechanical condition of the blood-vessels. It is our desire, therefore, to remove as much ambiguity as we may; and by endeavouring to show the vital nature of inflammation, to derive from this source some important light upon venous congestion. Nor is it less our object to extend those analogies which prompt us to distinguish the actions and phenomena of life from the manifestations of common matter; by which we may see our way the more clearly in our interpretation of nature, and in applying the means by which we endeavour to direct her laws.

Having trusted, in early life, to the general accuracy of micro-

scopical observations, we were disposed to conclude that there is a languid state of the circulation in inflammatory affections. (See APPENDIX on MICROSCOPE, Vol. I.) Still we could as little reconcile the phenomena of inflammation with the principles of mechanical or chemical philosophy, as those analogies that are supplied by other diseases. We were, therefore, irresistibly led to revert to the laws that govern living matter, whenever we attempted an explanation of its results, or the modus operandi of remedies. We turned our attention to the experience of different observers with the microscope, and found but little agreement amongst them, excepting as to the dilatation of the vessels, and an increased volume of blood; and even the latter phenomenon, like pain, is a contingent result, and not a necessary element of inflammation. (See Essay on *Venous Congestion*, Sec. 7.) Nor is it a little remarkable, that the doctrine of Celsus, "redness, swelling, heat, and pain," (1) has been copied and perpetuated to the present day of anatomical research.

It is said by one of Mr. Hunter's admirers, that—

"It appears not a little singular that this great man should have contented himself with reasoning on the state of the vessels in inflammation, rather than attempting to ascertain by experiment their actual condition." (2)

But it appears to us to have been characteristic of Hunter's genius, that he rested his views of inflammation upon great physiological principles, and the natural pathological phenomena. That this was the true process is rendered probable by the constant failure of experiments that are intended to imitate nature, or to illustrate her laws and actions, in her department of organic life; and this is shown to be especially true of experimental inquiries into the nature of inflammation, by the exactly opposite results in the hands of different observers.

We shall take Mr. Earle's very lucid digest of the existing doctrines of inflammation (3) for our guide; and shall consider that part of the theory of passive relaxation, which he has propounded in regard to the products and supposed terminations of inflammation, as irresistibly resulting from the fundamental principles of the doctrine. We do not, therefore, advert so much to Mr. Earle's individual opinions, as to their distinct exposition of a common and important hypothesis.

That part of the doctrine which regards the essential condi-

(1) L. 3, c. 10, p. 139.
(2) Morgan's Principles of Surgery.
(3) Lon. Med. Gazette, vol. xvi.

tion of the vessels immediately concerned in the process of inflammation supposes,

1st. A passive relaxation of the vessels, a suspension of their natural action when they are admitted to possess any, and an increase of their diameters.

2d. A progressive accumulation, stagnation, and coagulation of the blood within the vessels.

3d. An enlargement of the collateral vessels, proportioned to the redundancy of blood transmitted to the part, occasioned by its presence and the force of the vis a tergo.

4th. That the blood is propelled through the collateral vessels by the action of the heart; though all do not assent to this proposition.

5th. That the vessels being paralyzed in their action, and mechanically obstructed, can perform no part in generating the products, or in what is called the terminations of inflammation; and thence follows the corollary which Mr. Earle has unavoidably deduced, and which, as will be seen, is common to Mayo, Alison, Philip, Nauman, and others.

It may be proper, however, to state, in the language of others, a summary of the doctrine, as we believe it to be generally received:

"The observation," says an enlightened writer, "which I have quoted from M. Gendrin, when taken in connection with the facts previously stated, simplifies the theory of inflammation, and satisfactorily explains the alliance of all its leading phenomena. *The initiatory effusion of serum and lymph, dependent upon the visible obstruction of the circulation;* the lymph, with attenuated inflammatory fibrine; the consequent occasional mixture of blood with lymph; the formation of pus, secondary to and later than the secretion of serum and lymph, *being the result* of protracted inflammatory action; the solid particles in pus, although larger, yet of the same remarkable figure with *those of the blood, and doubtless the same enlarged;* the occurrence of pus mixed with it in streaks, or generally diffused through it; the organization of lymph by extension of vessels, some at first containing pus, others a thick red liquid; and, lastly, inflammatory gangrene, proceeding from the vessels being, in certain cases, irrecoverably obstructed; are phenomena which may be declared to be grouped under one law." (1)

"The result," says Dr. Philip, "was still the same. As soon as the inflammation began, the vessels began to enlarge, and the motion of the blood became languid; these changes going on till in the most inflamed parts the vessels were enlarged to *several times* their original diameter, and the motion of the blood *ceased altogether.*" (2)

(1) Mayo's Outlines of Human Pathology, p. 431.

(2) Experimental Inquiry into the Laws of the Vital Functions, p. 260, and 267.

Prof. Alison states, as "the general results of microscopical observations," that there is, "1st, an acceleration of the movement of the blood in the vessels of the inflamed part, sometimes attended with a distinct, though slight constriction of these vessels; but, more generally, at least soon after its commencement, attended with dilatation of the vessels. 2d. Within a short time, the state of the circulation, in the part chiefly affected, gives way to *retarded* movement in the *dilated* vessels, and ultimately to *complete stagnation* in some of these. 3d. As the blood stagnates in the vessels of the most inflamed parts, it gradually *concretes into irregular masses*, in which the *distinction of the globules* is no longer perceptible. 4th. The serous effusion, and afterwards the effusions of lymph and pus from the inflamed vessels, take place chiefly while the movement of the blood is slower than usual." Of course, if the blood be completely stagnant and concrete. "5th. If the inflammation subsides without sloughing, the blood in the part most inflamed is gradually set in motion again, its globules reappear, and the capillaries containing it gradually contract to their former dimensions. 6th. In the more advanced stages of inflammation, the actual conversion of some of the *decolourized globules of blood, which adhere closely together, into the larger and yellower globules of pus*, which have a free motion on one another, may be traced." "This transformation may go on either in the interior of the vessels, or in the exudations which have taken place from them." "It may begin almost from the beginning of the inflammation, and in other cases hardly takes place in any stage of it, however protracted." (1)

It is the opinion of Prof. Naumann, that "when inflammation is once established, the globules of the blood appear crowded together and *motionless* in the *greatly distended* capillary vessels, which must, therefore, have become *impermeable*." "At the seat of inflammation, circulation is *at an end*." (2)

By another it is said, "when inflammation succeeds to congestion of the capillaries, the circulation is *completely interrupted*, the blood coagulates, clogs the vessels," &c. (3) And yet our author concedes that the globules become "*more numerous* in the small veins."

We believe that all of this school concur in saying, that the vessels become more and more obstructed with coagulated blood as inflammation advances, and less and less capable of any independent action. (4) It appears, also, to be universally admitted, that in parts which carry red blood, a preternatural quantity is transmitted in inflammations, either through the seat of the disease, or the collateral vessels. This has been demonstrated by Mr. Lawrence, (5) and is shown by other unequivocal facts

(1) Alison's Physiology and Pathology, pp. 431, 432, 439.

(2) British and Foreign Med. Rev. No. 5, 1837, p. 214.

(3) Cyclopædia of Prac. Med. Art. Inflammation, p. 713. Our italics, as they are in most instances. See vol. i. p. 418, *note*.

(4) "That practical philosopher, Mr. Reeve, has anticipated the coming state of the profession, and we must shortly admit with him, that there is nothing stirring but *stagnation*." (*a*) See APPENDIX.

(5) On Diseases of the Eye, p. 64. — This appears from the increased quantity of

(*a*) Med. Chir. Rev. Lon. vol. xxx. p. 360.

which we shall ultimately mention. It is shown, also, by the profusion of blood which often follows the application of leeches to an inflamed surface, and the high florid colour and throbbing of the part. Objections have been often made to scarifications in erysipelas, on account of the great hemorrhage to which they are liable. In phlegmon, the blood flows from the incisions in the same way and with increased force. If the blood of an inflamed part be expelled by rubbing, it will return with unusual velocity. The preternatural generation of heat is an evidence of an increase of vital action, and augmented circulation, as is, also, the profuse secretion of different fluids, and their specific nature. The veins proceeding immediately from the part carry more blood than in the natural state, and this blood is more florid than natural; whilst it should be darker in proportion to its stagnation in the arterial capillaries.

The serous membranes, in their healthy condition, contain no red blood. This, and some other sensible appearances, have led to a denial of their organization. (See Venous Congestion, Sec. 7.) But, "in chronic and acute inflammations, they exhibit a vascular net-work, so full of blood, that their redness is often deeper than that of the muscles." (1) So, also, of cartilage, tendon, &c.

blood which flows from divided veins that issue from a part inflamed, as well as from the communicating arteries. The former fact has been denied by Morgan, but without the least show of proof; whilst its truth is known to every physician who has bled from the arm, in an inflamed state of the hand. In Lawrence's experiment, the quantity of blood was three times greater than from the sound arm.

(1) Bichat's General Anatomy, vol. ii. p. 7.

Dr. Billing, and the school who maintain the identity of the "nervous influence" and galvanism, dispose of the phenomenon of blushing, which has been set forth by the vitalists to illustrate the modified action of inflammation, by "attributing this giving way of the capillaries to derivation of the nervous influence (or galvanism,) which, being directed to or expended in the brain more freely by mental emotion, robs, for the moment, the capillaries of the face of their energy." (*a*)

The mechanical theorists, finding that the simple conditions of mere matter are inadequate to the results of inflammation, call in the aid of the nervous system; without, however, adducing even any remote analogy to illustrate the influence of the nervous power upon the supposed mechanical results. But this hypothesis, had it any foundation, is inapplicable to the essential principles upon which inflammation depends; since all its phenomena may take place in parts whose nervous communication with the brain is destroyed. Indeed, the division of nerves is an exciting cause of inflammation. Camerer, Magendie, Stanley, and Robinson, after dividing the par vagum and sympathetic nerves, found the mucous coat of the intestines or other parts disorganized as after inflammation under ordinary circumstances. Conjuncti-

(*a*) Principles of Med. p. 25—29.

Again, no mechanical explanation has ever accounted for the throbbing of an artery leading to an inflamed part, (1) much less for the general pulsation which often attends the part itself. The former often begins at an early stage of inflammation, when it is considered that the seat of disease is least obstructed, and subsides at the very time when the circulation in the capillaries is said to be most completely arrested. This is subversive of the mechanical rationale. But we have other facts of a stranger and coincident nature. The pulsation of the communicating arteries is often greatest when the general circulation is prostrated, as in local inflammations complicated with venous congestions; and here, again, the pulsation subsides, as the force of the general circulation rises, whilst the local inflammation goes on increasing. The varying phenomena are clearly owing to successive changes in the vital properties of the vascular system.

The cases of depression, also, to which we have just alluded, are the very ones in which general bloodletting, &c. is most important; and it is familiar to all, that whilst the force of the general circulation rises under the influence of the lancet, and

val inflammation has been very rapidly produced by dividing the nerves, especially the sympathetic, by Cruikshank, (*a*) Dupuy, (*b*) Brachet, (*c*) Mayer, (*d*) Magendie, (*e*) Petit, (*f*) &c. Bichat, by dividing the spermatic nerves, brought on "inflammation and a deposit of matter in the testicles." (*g*) (Pp. 26, 29, 30, and vol. i. p. 687.)

On the other hand, important nerves have been divided without any apparent injury to the parts which they supply. "I have always observed," says Bichat, "that in producing palsy, neither exhalation, absorption, or nutrition, have been impaired in any sensible or sudden manner in the palsied part." (*h*) A perfectly paralyzed limb may be nourished as well as ever, — as ever liable to inflammation, and to participate in constitutional fever.

These various results appear to confirm the principles which we have defended in relation to the nervous and organic properties, (See vol. i. pp. 158, 474 — 480, 568 — 572, &c.) and to invalidate the nervous, as well as mechanical doctrines of inflammation.

(1) The more this circumstance is considered, the more are philosophers convinced that no physical explanation can obtain. Thus, Dr. Graves: that "the larger arteries certainly dilate can scarcely be doubted by any one who has observed the state of the temporal arteries in phrenitis or apoplexy." (*i*) The supposed physical obstruction necessary to such a result must be speedily fatal in such an organ as the brain. The carotid often beats more sensibly on the side adjacent to an inflamed tonsil than on the other.

(*a*) Philos. Trans. 1795, Part 1, p. 177. Exp. 2, 3, 4. — (*b*) Journ. de Méd. t. xxxvii. p. 340. — (*c*) Fonctions du Système Nerv. Ganglion. c. 9, p. 368 — 401. — (*d*) Grafe u Walther's Journ. t. x. p. 3. — (*e*) Journ. de Physiol. t. iv. p. 304. — (*f*) Hist. de l'Acad. Roy. des Sciences, 1724, t. i. p. 9. "M. Petit a fait un grand nombre d'expériences sur des chiens vivans à qui il a coupé intercostat. Ceux qui appartenoient aux yeux ont été beaucoup plus constans, la conjunctiva s'est enflammé," &c.

(*g*) Recherches Phys. sur la Vie, &c. p. 515. (*h*) Ibid. p. 521.

(*i*) Lectures in Lon. Med. Gaz. July, 1838, p. 605.

although the local affection continue to increase, the action of the communicating arteries may have been sensibly reduced.(1)

(1) Mr. Morgan, in his Principles of Surgery, thinks that this pulsation of the communicating arteries, "is, without exception, the most unfortunate argument that could have been brought forward; and in place of being a sign of an increased circulation through an inflamed part, it is an obvious sign of obstruction." Abstractedly considered, which is but too frequently the case with both parties, it appears to us to prove nothing. It may then be equally supposed to arise from a propagation of increased action from the capillaries, or it may depend upon their supposed obstruction. The fact must be connected with all the proofs which relate directly to the state of the capillaries, and with all the facts appertaining to living organized matter, which everywhere contradict the application of mechanical principles to the problems of morbid action. Till a part is dead, it continues to be under the direction of the vital properties. Nothing requires so much an enlightened and comprehensive survey of physiological premises. Above all, however, is it important to consider the specific facts which we have stated in our text in connection with the morbid pulsation of arteries. Still it is sufficient to have shown that an increased volume of blood passes through the capillaries, or even through the communicating arteries, to render the pulsation of the latter, as in whitlow, an absolute proof of increased action in both series of vessels, certainly in the larger, and thence by analogy, as well as numerous facts and physiological principles, in the smaller series. The author, too, whom we quote, has collected a variety of evidence to show that the arteries possess a "vital contractility, which consists in the power which the fibres of the middle tunic possess of *shortening* and *elongating* themselves, by which means the diameter of vessels is *increased* or *diminished* so as to admit more or less blood, and by which, also, the circulation of that fluid must be influenced;" that "the evidence leaves no room to doubt that arteries possess a power of acting totally distinct from elasticity." Why then, depart from admitted principles which form the basis of physiology, to expound phenomena which, more than any other, illustrate and establish those principles. True, we hear of a physiological and a morbid condition of parts as something in direct opposition; and this substitution of the former term for that of healthy has carried its illusion. Still we believe that sound philosophy can only recognise one state as a modification of the other, without any radical change of principle.

We now come to an admission in behalf of nature, which we find to be common among able writers who more or less embrace the mechanical doctrines. "We believe," says Mr. Morgan, "that every practical surgeon will acknowledge, that if the artery leading to an inflamed part be divided, as the digital branch in whitlow, the blood will be ejected to a greater distance and flow more impetuously than from a vessel supplying a similar healthy part. But, although an increased activity in the larger arteries is thereby distinctly shown, it does not follow that those at the seat of disease are similarly affected, and our objections here are the same as those already offered. It is no proof of the state of the circulation through an inflamed part that it contains more blood than one in a sound condition, for the contending parties admit that point without dispute; nor is it any evidence of either one state or another, because the blood flows more freely when an incision is made; for were the vessels distended from whatever cause, the same thing would result on their division."(*a*)

Here then, is all we require, to settle our position, even upon the mechanical principles. Our able author has overlooked some important facts as to the immediate instruments of disease, and appears to assume that the escape of a redundant quantity of blood from the divided part relates only to the excess that may exist in the

(*a*) Morgan's Principles of Surgery, pp. 16, 25, 57.

Many specific facts might be brought to sustain our conclusions. Thus, in a case where Dr. Warren tied the right carotid on account of an erectile arterial tumour situated at the internal angle of the right eye, this vessel and its opposite fellow and their branches pulsated with violence. The ligature on the right carotid removed the pulsation of the left, although "the vibrations were more conspicuous on the left than on the side originally deranged." "The perfect success, from tying the right carotid, showed that the affection of the left side was altogether sympathetic." (1)

Many observers positively declare that the blood is accelerated in the capillaries when stimulants are applied to them; and the alternate actions of contraction and dilatation have been often witnessed. (2) — (See *Venous Congestion*, Sec. 10; where this

capillaries at the moment of the division. But this is not so. The excess continues for a long time, and the most so in those cases where the obstruction and stagnation are supposed to be greatest. Other circumstances which are not taken into consideration will appear as we advance with our text.

(1) Surgical Observations on Tumours, p. 403.

(2) Schwann has discovered that the capillary arteries possess a coat with circular fibres, similar to those of the larger vessels, and that they contract under the influence of cold. — *Müller's Archiv.* 1836.

Müller denies the muscularity of arteries; but he neglects the anatomical fact and relies mainly on the effects of galvanism. This, however, has often failed to excite the heart itself. He denies, also, all independent action to the capillary arteries, and considers it "irrefragibly proved, that the motion of the blood through the capillaries is affected solely by the action of the heart." He subsequently states, however, that Baumgaertner observed the blood in the frog's foot continue in motion from three to five minutes after ligature of the artery. Our author also admits, that having destroyed the vitality of the heart of a frog by liquor kali, he could, by means of the microscope, for some time perceive motion of the blood in the capillary vessels. Again, "in the instantaneous injection of the cheeks with blood in the act of blushing, and of the whole head under the influence of violent passions, the local phenomena," he says, "are evidently induced by *nervous influence*. The *active* congestion of certain organs, of the brain, for example, while they are in a state of excitement, is a *similar* phenomenon." And again, "it is certain that nervous influence is the principal cause of the accumulation of the blood in the capillaries of a part during the state of vital turgescence."(*a*) This is all we contend for; and we value it the more as it is apparently the second thought of our enlightened observer.

Here is an unequivocal fact. Dr. Stevens, after plunging a bodkin into a rabbit's brain, opened the chest; when he found the heart motionless, but a branch of a coronary artery was pulsating forcibly, and continued so till its blood was entirely expelled.(*b*)

Haller, Bichat, Philip, Hastings, Hunter and others, have seen the blood moving

(*a*) Physiology, &c. pp. 202, 206, 208, 218 — 225, 233. (*b*) On the Blood, p. 57.

subject is farther examined, in connection with the action of the veins.)

We entertain no doubt that the blood is often more or less coagulated by the experimenters in the delicate vessels of the

freely in the mesenteric capillaries of rabbits, &c. sometimes for more than an hour after the removal of the heart. (*a*)

Burns mentions an instance where the whole circulation must have been carried on mainly by the vessels; since both ventricles of the heart were as fully ossified as the cranium, except about a cubic inch at their apex. (*b*)

Johnson maintains that the leech has no heart. In any event, he "has seen a well marked systole and diastole in the abdominal, lateral and dorsal vessels, by the naked eye;"—about ten pulsations in a minute. (*c*) This is confirmatory of Dr. Hall's observations to the same effect, who thinks he has shown that "the apparent effect of alternate contraction and relaxation of the artery after the ligature of the aorta, is certainly most powerful;" and he has pointed out "an artery in the frog and toad which pulsates independently of the heart." (*d*) Müller, however, says this is "a pulsating lymphatic heart." — *El. of Physiol.* p. 203. — Nevertheless, it has the character of an artery; and since we have here an instance where nature is ascending from a simple to a complex organ, and the former performing the functions of the latter, the analogy is very appropriate to our purpose. And again, he says "the dorsal vessels of insects, and the principal, though not all the vascular trunks of the annelides,—for instance, the leech,—certainly contract by muscular force. But these parts are *hearts*." "In the lower animals, the heart is nothing more than a dilated part of the vascular system endowed with contractility." And yet our author thinks that analogies of this kind "are of no weight;" and without any other show of proof than what is derived from the supposed failure of "galvanic and electric stimuli" to produce contractions in the arteries, (*Venous Congestion*, Sec. 10,) he concludes that all the imputed vital contractions of these vessels are fallacious, and resolves their actions upon mere physical principles. — (*Ut cit.* p. 205.)

Very sensible contractions and dilatations of the arteries have been produced by Zimmerman, (*e*) Verschuir, (*f*) Lorry, (*g*) Socmering, (*h*) Hastings, (*i*) Parry, (*j*) Dennison, (*k*) Vandembas, (*l*) Jones, (*m*) Bikker, (*n*) Thomson, (*o*) Hunter, (*p*) Spallanzani, Rossi, Guens, Aldini, and others, by irritating agents. Hastings states that "an increase of dilatation and contraction in the exposed part of the vessel, whilst the stimulus was applied, was not an uncommon occurrence;" but, "the arteries do not contract from acids, &c. after death. This is more or less confirmed by the foregoing observers. The arteries, indeed, are well known to have contracted when irritated by the scalpel. (*q*) They sometimes close remarkably when divided in the living subject; always more so than after death. (*r*) Verschuir has seen them move with increasing activity when exposed to the air. (*s*) When an irritating cause is removed, the contractions cease. (*t*) The arteries are strongly contracted by elec-

(*a*) Philip's Exp. Inquiry, &c. Exp. 67. Hastings, and others, as cited in our Venous Congestion, Sec. 10. — (*b*) On some of the most frequent and important Diseases of the Heart, p. 117—120. — (*c*) On the Medicinal Leech, pp. 116, 117. — (*d*) Hall on the Powers which circulate the Blood, pp. 54, 58, &c. — (*e*) De Irritabilitate. — (*f*) De Vi Arteriar. et Venar. Irritabil. — (*g*) Vandermonde Rec. period. t. 6. — (*h*) Gefæsslehre, p. 67. — (*i*) Exp. Inquiry of the Nature of Inflammation. Exp. 38, 39, 40, 41, &c. — (*j*) Additional Exp. on the Arteries. — (*k*) De Vasorum Irritabilitate. — (*l*) Dis. de vivis human. Corp. solid. Exp. 10, &c. — (*m*) De Arteriæ sectæ Consecutionibus, p. 29. — (*n*) Dis. de Natura Humana, p. 45. — (*o*) On Inflammation, &c. p. 75—89. — (*p*) On the Blood, &c. p. 158. — (*q*) Hastings, ut cit. Exp. 2, 3, and Table 1st. Verschuir, ut cit. — (*r*) Hunter on the Blood; Verschuir, Exp. 8. — (*s*) Op. cit. Exp. 17. — (*t*) Ibid. Exp. 18. Also Jones, Hastings, Hunter, Thomson, &c.

membranous parts to which the active chemical agents, or other penetrating matter, are applied, — especially heat. This is, probably, one source of the deception. In such a case, the blood would readily force its way into the collateral vessels; whilst the others would remain clogged up till their vital forces may so far recover themselves as to overcome the obstruction. This, however, is any thing but a natural process of inflammation. The blood is not only chemically coagulated, but, probably, the organization of the capillary tissue is often simultaneously altered, and the minute extremities of the arterial and venous systems completely astringed. The process of inflammation, therefore, is not carried on by these vessels till they may recover their action, but, by others in their vicinity where it is necessarily admitted the circulation is accelerated. Hence the reason is obvious, why, in this mixed state of things, the ultimate products may be more or less analogous to those of natural inflammations, after the vital forces shall have thrown off the artificial results. If, however, the violence be carried beyond a certain limit, the vital properties are destroyed in all the vessels, the tissues crisped till they often become dry, the blood everywhere coagulated, and we are then told that this is the degree of inflammation which produces mortification. The only parallel in the case, however, appears to consist in the death of the part, which has been brought about by direct violence in one case, and by the mysterious operations of nature in the other; just as we kill an animal in one instance by a blow on the head, and in another by exciting inflammation of the brain.

We have seen that the prevailing hypothesis of inflammation ascribes a far greater destruction of the capillary circulation in

tricity, (*a*) even when applied to the nerves; (*b*) however some may have failed of this result. Home saw the carotids beat with violence, after irritating the sympathetic nerve with potass. Brodie obtained the same result by irritating the intercostal nerves. (*c*) Philip certainly influenced the action of the arteries by stimulants and sedatives applied to the brain. (*d*) The pulse sometimes fails in one arm when there is no mechanical obstacle to the circulation, and returns when disease subsides. This is particularly true of cerebral affections. Blood is projected from an artery during the disastole of the ventricles. It circulates where the heart is wanting; and nutrition is fully performed in the fœtus that is destitute of this organ. (*e*) The independent action of the extreme capillary arteries has been also experimentally shown by most of the foregoing observers.

(*a*) Verschuir, Guens, Bikker, Vandembas, ut cit. — (*b*) Guiulio in Mém de l'Acad. des Sci. de Turin, t. 4. p. 50. — (*c*) Philos. Trans. 1814. — (*d*) Exp. Inquiry, &c.; and Essay on Sleep and Death, p. 72. (*e*) See St. Hilaire Hist. des Anomalies de l'Organization.

the *living* state of vessels, than when they are visibly dead. It is stated by Dr. Philip that

"The motion of the blood in the smaller vessels continues for a long time after what we call death, although immediately after it a ligature be thrown round all the vessels attached to the heart." "An hour and a quarter after the heart had been removed, he brought part of the mesentery, which had long been *quite cold*, before the microscope, and still found the blood in some of the capillary vessels moving quite freely." (1)

Dr. Philip's proofs, abstracted from the microscope, of the independent action of the capillaries, appear to us irresistible.

It is remarkable that the advocates of debility, and stagnation of blood, as the pathological state of inflammation, generally maintain that the capillary vessels possess no action, and that their circulation depends upon the power of the heart. This condition supposes even a removal of all physical contraction from the extreme vessels, and of all possible obstruction to the circulation. Nevertheless, they fully admit even much beyond our own doctrine, the agency of the "nervous influence" in inflammatory actions. (See Vol. I. pp. 162, 163, and *erratum*, 157, 568—572; Vol. II. p. 26—28, *et passim*.) How far it is philosophical to connect the nervous influence with physical agencies, we have endeavoured to show on various occasions. (See Vol. I. p. 714, Vol. II. p. 30, &c.) Again, if vital action did not exist, there should be no varieties of inflammation. The vital phenomena, and the physical products, should be always the same; the same in all tissues, and constitutions. Nor should we have the remarkable and diversified sympathetic influences of inflamed parts upon the system at large, or upon some particular organ that may naturally sustain peculiar relations to the part inflamed. We need not say, how far it is otherwise, and how impossible it is to explain the foregoing phenomena upon any other principle than that which supposes the essence of inflammation to consist in some modified state of the vital forces and actions. (Vol. I. p. 575.)

It is said by Dr. Davies, in his excellent work, that the doctrine which ascribes an increase of vital power to the vessels in inflammation is "contrary to every analogy. It would be assuming, contrary to known fact, that the effect of disease is to render the body stronger than it was in a state of health." And then follows a perfectly physical illustration about the action of

(1) Exp. Inquiry into the Laws, &c. pp. 210, 269; et seq.

sulphuric acid upon carbonate of soda; which is certainly the whole amount of the proof. He contends for a "weakening" of the vital powers by all morbific causes. (1) We have introduced this subject on account of its importance, and the general application of the foregoing philosophy. We may say, in the first place, that the doctrine which Dr. D. opposes does not "assume that the effect of disease is to render the body stronger than it was in health." It applies only to particular parts in a state of inflammation, not to the body whose vitality may be greatly depressed by sympathetic influences, or by general febrile action. But what are the *facts* in inflammation? If they are opposed to "every analogy," it is because inflammation is an unique affection, and has no analogies excepting in such conditions as approximate it. How are irritability, mobility, sensibility, sympathy affected? Certainly *all* more strongly pronounced than in health, and we find our author arguing at times in this very manner. The fact is obvious in almost every case of inflammatory action during its positive stage. And have we not stated many proofs which show that "every analogy" is really in favour of our conclusion? But, this is a small part of the altered condition of the vital powers. They are actually *altered in their character*, and this, too, according to the peculiarities of every tissue, and the diversities in the remote causes. This is the foundation of rational pathology, and of the effect of curative agents. These *alterations* have no necessary connection with *strength* or *weakness*, whilst either, in their proper physiological sense, may be perfectly compatible with the changes in *kind*, and with whatever is known of the same properties in their natural state. Whenever, therefore, we may speak of an exalted state of the vital powers from morbific causes, we mean to imply more particularly that they are modified in their *kind*, though exalted in degree, when vascular action is necessarily increased. *Strength* and *weakness* are wholly inapplicable terms to express the condition of a vital part. They have been fallaciously borrowed from the properties of dead matter, and have not been a little instrumental in bringing living matter into its opposite condition. (See Vol. I. pp. 31—35, 81, 279—293, 311—326, 454—460, 693.) The only admissible sense in which the foregoing terms can be applied to the properties of organic matter

(1) Selections in Pathol. and Surgery, &c. Part I. 1839.

is with certain qualifications, as in the following instances by Mr. Hunter:—

"Fever shows *powers* of resistance; the other symptoms show weakness sinking under the *injury*. This is like the effects of the cold bath; yet we see it calling forth, or rousing up to action, some peculiarity in the constitution, or a part," &c. "The following case is another strong instance of *great action* in a *weak, irritable* habit." "It would seem that violent actions of a strong arterial system required less blood than even natural actions; and even less still than a *weak* or *irritable* system; from whence we must see, that bleeding can either relieve inflammatory action, or increase it, and therefore not to be used at random." "When I described inflammation, I observed, that there was either an increase of life, *or an increased disposition to use with more violence* the life which the machine, or the part was in possession of;" (1) and some other analogous expressions as noticed in our first volume, p. 242—246.

And, how will the mechanical philosophers expound the *weakening* effect of bloodletting, and cathartics, in almost instantly imparting *strength* to the whole system when that *strength* has been greatly prostrated by disease? Is not Hunter right when he says that there may be "an inconvenience" attending bloodletting; "by bringing the inflamed part upon a par with health, the sound parts may be brought much lower, so as to be too low"?

We see, therefore, that there is no analogy between the exaltation of power in inflammation and the natural state of the vital forces, and that it is wholly an assumption to affirm a *weakness* of the powers because they are invaded by morbific causes. The whole natural action of the inflamed part is subverted, and the only ground of reasoning in the case as to the exaltation or the depression of the vital powers is the phenomena which may be manifested; whilst an argument founded on the abstract fact that the part *is diseased*, or upon the action of sulphuric acid upon carbonate of soda, is an abandonment of nature.—See Essay on *Venous Congestion*, Sec. 9.

We shall recur to this inquiry; and shall only now add, before proceeding to the mechanical principles which relate to the doctrine under consideration, that it is candidly admitted by some of its strenuous advocates, that

"In opposition to the hypothesis of direct debility of the vessels, the exciting causes and treatment of inflammation coincide better with the idea of excessive, than of defective action." (2)

Is not the whole weight of this proof in direct collision with

(1) On the Blood, &c. pp. 334, 335, 336, 341, 398.
(2) Cyclopædia of Prac. Med. ut cit.

the modern doctrine? Does not the increased *irritability* denote an active condition of the vessels? (1) And may we not found an argument upon the nature of the vital properties; and should not analogy be allowed to be very strong, since, if effusions, analogous to those of inflammation, which occur in health, depend on vascular action, they must depend upon the same cause under all circumstances of disease? If this be not admitted, physiology must be abandoned as inapplicable to pathology, and as having no consistency. We must look with new eyes upon nature, and renounce all respect for her general laws. "What," says Bichat, "can we conclude from an isolated phenomenon, which is in contradiction to all those which nature daily presents?" (2)

Besides the obstacles which the microscope has opposed to the inductions which are prompted by all the natural phenomena of life, and even by such as appertain to inflammation, there is one other fundamental, though hypothetical, objection to the doctrine of vital action. It is thus stated by Prof. Alison:—

It is difficult to understand, how an increase of the only vital power which we have reason to ascribe to arteries, viz. tonicity, or tonic power of contraction, can lead to distension of the vessels, and at the same time to an increased flow of blood through them." (3)

We shall make but few remarks upon the foregoing objection, at present, since we shall have occasion to examine at length the action of both systems of blood-vessels in our Essay on Venous Congestion, (Sections 10 and 11.) We will only now say, that "it is difficult to understand," how the vital power of contracting can exist without a corresponding power of dilating; especially since the vessels must otherwise exist in a state of permanent contraction, and, of course, without any vital action other than that which maintains them in a state of tonic spasm. This is no action, and must form an absolute obstacle to the circulation. If action mean anything, it means motion, and this means alternate contraction and dilatation. The difficulty in "understanding the tonic power of contraction" consists in having assumed that unintelligible condition. Nor can it be conceded, that the power of the heart would exert any adequate effect, as an equivalent to the action of dilatation.

The capillary vessels, then, performing at all times in health

(1) Mr. Earle observes, "when I shall have occasion to say that a part is *weak*, I shall not deem it necessary to call it *irritable*." What affinity is there in these terms? Is not the former as unintelligible as the latter is full of meaning?

(2) Gen. Anat. vol. i. p. 341. (3) Outlines of Physiol. and Pathol. p. 435.

the actions of contraction and dilatation through vital forces, there can be no reason assigned, why, when those forces are exalted, the contractions and dilatations should not be increased in a corresponding manner.

A passive dilatation of the vessels supposes a state that facilitates the transmission of blood. Nor is there any obstacle surmised that shall either counteract the effect of the enlarged diameters, or oppose the circulation before that enlargement takes place. Müller, Carswell, and others argue, indeed, that "the blood must, *cæteris paribus*, flow more slowly in a dilated vessel than in one more contracted."(1) This may be plausible enough; but it cannot be conceded that the *cæteris paribus* exist, as will appear from our ultimate facts. These facts demonstrate that the extreme vessels, as well as the larger arteries, acquire an increased action, so that the blood, as also shown by direct facts, may be more accelerated in the enlarged vessels than in their natural state. Besides, is it any proof that the blood coagulates because its momentum is moderately diminished? On the contrary, is not the conclusion opposed by the results of ligatures upon the principal arteries of the extremities? Does not the blood remain long fluid in a vein when arrested by two ligatures?(2) Does not inflammation constantly spring up when the circulation is greatly accelerated?

Concurring, also, with the foregoing causes in promoting a free circulation of the blood is the more attenuated state of this fluid in inflammations, and its diminished tendency to coagulate.

Thus, then, we have the affirmations, in opposition to mechanical laws, upon which the theory mainly rests, that the blood stagnates in the enlarged vessels in proportion as their diameters become increased; although, too, the blood is urged on by their preternatural action, and by an increased force of the vis a tergo, whilst the blood may be not only greatly thinner than natural, but less disposed to coagulate.(3)

(1) Müller's Elements of Physiology, vol. i. p. 228. — Dr. Billing compares the current to the velocity of a river according to the width of its channel. But there is nothing shown by such comparisons but what relates alone to physical causes. Suppose, however, the narrower part of a channel to be upon a dead level, and the wider at an angle of 45°, we shall have a greater velocity in the latter instance. Just so is it in inflammation. We have here, too, a power superadded.

(2) See Hunter on the Blood, p. 25. — Sixty-five days in the t. vaginalis.

(3) This diminished coagulability is denied by some; but in opposition to common observation. See Müller's Physiology, vol. i. p. 116.

The doctrine of resistance in proportion to the increased force of the vis a tergo is

This, however, is only the beginning of the conflict of the mechanical laws. There is a preternatural volume of blood determined towards the seat of inflammation; sometimes, as has been shown, at least thrice the usual quantity. This is said to find its way through the collateral vessels. Of course, it must begin to enlarge these vessels before stagnation commences in the direct instruments of disease; otherwise, the latter, being passively dilated, would permit the redundant blood to follow their channel, and no stagnation could happen. We see not how this difficulty is to be obviated; since it would be absurd to suppose that dilated vessels can constitute an obstruction to the increased volume of blood which is constantly determined upon the part, and this, too, by an augmented vis a tergo. But the hypothesis also supposes that the dilated state of the vessels is not only adequate to arrest the increased determination of blood, and its increased momentum, but, that it is able, also, to overcome the resistance that is opposed by the tonic and elastic properties of the collateral vessels,(1) whilst it is one of the most palpable laws in physics, that in proportion as a vessel dilates, obstacles are removed, and vice versa. It should also be considered, that whilst the collateral vessels are in full possession of their tonic forces, those of the diseased vessels are so enfeebled that they permit a passive dilatation from the beginning. Why the blood, therefore, should take a course in the direction of the greatest impediment, the mechanical hypothesis does not deign to explain. And here we may say that the argument founded upon the supposed diminished velocity of blood in the dilated vessels, which are the instruments of disease, is farther shown to be worthless by the dilated state of the collateral vessels, and in

thus expressed by Mr. Morgan, who belongs to the school that allow the instruments of disease to be in a state of "increased action" in the first stage of inflammation; which, it will be seen by our argument, only renders the mechanical hypothesis still more offensive to its own principles. Now mark the progress of results. As "the vis a tergo increases," says Mr. Morgan, "there is a gradual subversion of the phenomena present, and a new and different series of morbid changes arises. The blood becomes more viscid, and this, with the *more vehement action* of the larger arteries behind, tends, in particular, to destroy the tonicity of the extreme branches, which become *still farther dilated*, and lose their control over the fluid within them. In many of the extreme vessels the blood is now at rest." (*a*)

(1) True, Mr. Mayo assumes that the capillaries may have a ready power of relaxing themselves. This might have been excused in Hunter; but it will not hold in a theory so mechanical as the one under consideration. Morgan, of the same school however, has answered the conjecture by six substantial objections. (*b*)

(*a*) Principles of Surgery, p. 46. (*b*) Ibid. p. 29.

which, also, it is admitted that the velocity is increased. But, since the collateral capillaries become dilated, we ask for the reason why this condition should not equally occasion a stagnation and coagulation of blood as the like condition of the immediate instruments of disease? Upon what possible ground can it be assumed, that dilatation of one series of vessels causes stagnation, and that the same state of others in immediate contiguity is attended by an accelerated movement of the circulation? We see not but our friends must consent to be impaled on both horns of this dilemma.

The process of inflammation, according to the hypothesis which we are examining, appears not to have been systematically traced beyond its primary stage, until Mr. Earle took up the principles on which the doctrine is founded, and explained the manner in which the various results take place. (1) It appears to us, as we have intimated, that Mr. Earle's exposition is in perfect conformity with the laws which are supposed to operate in the introductory stage, and that the whole theory is perfectly consistent and unavoidable in all its parts. Various authors, however, have more or less extended the philosophy through the ultimate details of inflammation, but no one had pursued it methodically till undertaken by Mr. Earle. We shall, therefore, follow this observer along his well beaten path, in the hope that we may cull some flowers by the way to grace the genius which presides over the destinies of life. But we speak not of Mr. Earle individually, but as the representative of a prevailing and important doctrine.

In conformity, therefore, with the foregoing premises, it is contended that serum and lymph, arising from inflammation, are merely mechanical exudations from the blood; and as to pus, Mr. Earle adopts the no less prevailing belief that "the opinion of its being a secretion cannot be admitted without carrying our deference to Mr. Hunter's authority to an unwarrantable extent."(2) And since the laws of life are supposed to have no connection with the formation of pus, it is considered, as by many

(1) It has been said by an able reviewer, that "Mr. Earle's lunges against the doctrine of Hunter are like the exploits of Ajax." We only ask what Ajax asked from the gods, παρα λευκὸν εὐαμερον,—"give us but light;" when we doubt not that the arms of our modern Ajax will be again adjudged to some future Ulysses.

(2) Essays in London Med. Gaz. vol. xvi. p. 136.

This is only conformable with what we have seen to be alleged of many natural secretions, in our "Humoral Pathology."

others, to be a component part of the blood; and this is consistently supposed to make its escape in a purely mechanical manner.

Mr. Earle has illustrated this subject by a comparison.

"Would it be a matter of astonishment," he says, "if the several parts of a fluid like the blood, upon being pressed through an excessively fine sieve, were to come through in the order of their fluidity; that is, the finest and most fluid first, and the largest and coarsest last?" (1)

Lest this hypothetical illustration might seem to be conclusive, we have seriously put it to the test of experiment. But every species of the finest texture was permeable by the serum, and admitted, also, the passage of the red globules. (2)

But we have seen that the vessels are clogged up and rendered impermeable by coagulated blood; and without a chance, from the want of vital action, of relieving themselves. This must be especially the case, considering

"The impossibility of detecting any pores, or openings in the sides of the minute vessels, the impossibility of the processes of nutrition and absorption being carried on through the coats of the vessels"; and, that "there are no new vessels formed, nor any new disposition of the old, which can be termed glandular." (3)

In this condition of the vessels, therefore, it is difficult to understand how even so thin a fluid as serum should find its way from the extremities of the vessels, especially in any remarkable quantity; and this difficulty increases when we consider that no fresh supply of blood enters the obstructed vessels. We learn, also, from Professor Alison and others, that it is under this embarrassment that "the serous effusions, and afterwards the effusions of lymph and pus from the inflamed vessels take place." (4)

But, besides this mechanical obstruction, the effusions, as well

(1) Op. cit. p. 141. "Our philosophy," indeed, "would teach us that there is but little difference between a man and a sieve." (See vol. i. p. 75.)

Professor Alison finds in the foregoing effusions a "strong indication of an actual increase of vital action in the vessels of an inflamed part;" though it will be seen that he resolves them upon the mechanical principles. "The immediate effects of inflammation on the contents of the vessels of the part," he says, "the exudation first of serum and afterwards of coagulable lymph, are effects which may naturally be expected from increased constriction of the coats of these vessels." (*Outlines of Pathology, p.* 434.) But why not adhere to the philosophy of "increased vital action" which is so "strongly indicated"?

(2) Berzelius says they pass readily through filtering paper. — *Trait. de Chimie*, t. vii.

(3) Mr. Earle, Op. cit. pp. 8, 142, 168. (4) Op. cit. p. 431.

as the hypothesis, have to contend with other obstacles. In a wound "there is a thin layer of parenchyma between the external air and the blood in the adjacent vessels, which is sufficiently dense to prevent the egress of blood as blood; but it cannot prevent its exuding in detail." Then it is hypothetically stated, as we have seen, that the parts of a fluid, like the blood, upon being pressed through an excessively fine sieve, would come through in the order of their fluidity, the finest first, the coarsest last. (1) But we have already shown, that although this be true in respect to a sieve, and the object for which it is employed, it does not hold good in regard to the blood. Could we have endowed our sieve with the powers and actions of life, we might have made out the parallel. (Vol. I. pp. 117, 659, *note*.) The conclusion, therefore, was forced upon us, that the simile is defective from the defect of the sieve in those endowments. Nor should it be overlooked, that the parallel of the sieve appears to suffer another radical defect in the supply not being kept up, whilst there must be a constant renewal of the raw material in the inflamed vessels. In the former case, nothing remains at last but the coarser matter, whilst in the other instance, there is constantly presented the finer as well as the coarser to be perpetually strained off; and, therefore, in the latter case, according to the hypothesis, coarser matter should never exude. True, the hypothesis supposes that the vessels are so clogged, that there are no successive renewals of blood; but the prodigious effusions which often occur, when the vessels are supposed to be most obstructed, prove by the principles upon which the hypothesis is founded, that it has violated the most palpable laws in physics. (2) But we leave this matter to be settled by farther experiments, and pass on to other difficulties.

Again; after the serum is strained off by the force of the

(1) Müller makes, apparently, an ambiguous compromise with the vitalists. "The exudation," he says, "of the fibrinous fluid in inflammation might be explained by the *obstructed circulation*, but the *quality* of the exuded matter depends on *other* causes." (*a*) He does not say, however, that the other causes are vital; and perhaps, therefore, the chemists have a right to the construction.

(2) Aretæus appears to have anticipated the present hypothesis, and our objection. Thus: "Sed magnum profecto miraculum, quomodo id fiat, ut ab exili tenuique membrana nihil crassitudinis habente, membrana inquam costas interius obtegente tanta copia pueris effundatur, multum enim colligi in multis deprehensum est. *Hujus rei causa est inflammatio ex sanguinis multitudine oriens.*" — De Diuturn. Affect. l. i. c. ix.

(*a*) Elements of Physiology, vol. i. p. 255.

heart alone, as is generally held, (impinging against "stagnant and coagulated blood,") through the vessels that are clogged up and farther obstructed by the external layer of parenchyma, and whilst also, according to the doctrine, the blood has become still more stagnant in the instruments of disease, coagulable lymph, a much grosser part of the blood, begins to follow its pioneer. But here, it is manifest the order should have been reversed, if mechanical philosophy is to resolve the phenomenon. (1) Besides, what becomes of the serum which is constanly presented, as from the beginning, along with the fresh supplies of blood? What imparts to the lymph its new properties, its exalted vitality?

"The surface," it is said, "now becomes covered with an amorphous deposition of coagulated fibrin, and becomes thicker, so that a greater impediment exists to the expulsion of the globules than before." But whilst this, and other obstructions form an impediment also to the effusion of serum, they are no obstacle to the expulsion of pus; for it is said "the fluid now becomes thicker, and assumes the appearance which is known by the name of pus." (2) And, notwithstanding all these increasing obstacles, purulent matter is often poured out in a torrent, altogether exceeding the primary effusions of the more attenuated parts of the blood. (3) Indeed, the size of the globules of pus, which is stated by many to be twice that of the red globules, (4)

(1) When it was observed that blood did not coagulate in inflammations, as it dropped from an orifice, the mechanical physicians explained it "in their own way, by supposing that the thicker part of the blood can flow out only in proportion as the orifice is large, the thinner particles being supposed to be thrown out towards the sides of the vessels, while the thicker blood circulates in the centre." (*a*)

(2) Mr. Earle, Op. cit. pp. 141, 142.

(3) In the serous and mucous membranes, however, we have other phenomena. Here the "finer parts of the blood" are produced in such abundance as to render it equally certain that there is, at all times, a greatly augmented volume of blood circulating actively in the instruments of inflammation. And what is here, too, remarkable, the "finer parts of the blood" are thrown out when inflammation is least, and the vessels are supposed to be least obstructed; but the "coarser parts," or the lymph and purulent matter, are only effused from these membranes when inflammation is more active, and the vessels are supposed to be in a more obstructed state. We would also oppose this last consideration to the late hypothesis that the preternatural effusion of lymph is the result of a healthy process.

(4) M. Donné in Archives Génér. Août, 1836; M. Gendrin, Hist. Anatom. des Inflam. t. ii. p. 489; and Dr. Hake, ut cit.

(*a*) Zimmerman on Experience in Physic, vol. i. p. 306.

and the increased density of the stratum are supposed to facilitate their escape. (Vol. I. pp. 686, 702.)

"There are two reasons," it is said, "why the globules are the last to be effused upon the surface of a wound. The first is to be found in their *size*, the second in the *deposition of lymph*, by which the thickness of the stratum through which they have to pass is increased." And again it is said, "the passage of the blood to the central point of the inflamed part, which was *at first perfectly free*, now becomes gradually *more and more* obstructed and difficult. As this happens, the globules lose their colouring matter in passing through the tissue thus thickened." (1)

But, what strikes us as a far greater defect in this application of mechanical principles, is the arrest of the serum, or "finest and most fluid part of the blood," after "the largest and coarsest parts" have opened a channel for themselves. Here again, too, the *sieve* is brought in our way. It supposes that the vessels, containing nothing but coagulated blood, are as fully exhausted of serum as the clot of blood that had been mechanically filtered. But, if there be not a great and incessant renewal of blood in the instruments of disease, we again ask, whence proceeds the torrent of pus? And if so, what keeps back the "finer parts"? Will the advocates of "stagnation" answer; bearing in mind, at the same time, that effusions of pus sometimes continue for months and for years? Nevertheless, it is a rational conclusion that, if stagnation of blood be necessary to the first production of pus, it is equally so to the end of the year.

Again, following up the idea suggested by the *sieve*, Mr. E. in behalf of his school, accounts for the removal of the colouring matter from the globules of blood in their conversion into pus, upon the ground that, "it is certainly possible that it may be merely mechanically wiped off in their progress"; and this opinion is variously enforced. (2) But we here refer to our contradictory experiment, and to that of Berzelius with filtering paper; (p. 159.) Something may be also inferred as to the effect of friction from the almost fluid nature of the red globules and that of

(1) Mr. Earle, Op. cit. pp. 142, 188.

(2) Op. cit. pp. 142, 188, 219. This, however, appears to have been suggested by Gendrin, who says, "ils se dépouillent de leur matière colorante qui parait par striés dans leur interstices." — *Hist. Anatom. &c.* t. ii. § 1471. The imputed process is analogous to an opinion entertained by some, that the absorbent glands supply the central particle of the red globules, by straining it off from the lymph or chyle. According to Hodgkin and Müller, the colouring matter is only disposed to leave the globules when water is applied. — *Pogendorf's Annals, t. xxv. p.* 513, and *Philos. Trans.* 1818.

the tissue which composes the extreme vessels, as well as from the size of the globules, which Leuwenhoeck estimated at $\frac{1}{1,000,000}$ part of a grain of sand, and which, according to later microscopists, would take 10,000,000 to make a square inch. Hewson saw thousands of them in an insect whose entire bulk did not exceed a pin's head. (1) It should be recollected, too, that this wiping by the blood-vessels is contradicted by the principles of the mechanical doctrine; since, in rejecting entirely the vital theory of absorption and ulceration, it supposes that the surrounding tissues are broken down by the force of the vis a tergo:

"The *first effect* of inflammatory infiltration," it is said, "being to destroy the original firmness and tenacity of every tissue in which it occurs, so as to cause it to be readily broken down and give way on the *slightest pressure*." (2)

It is, therefore, the more difficult to imagine that degree of mechanical resistance or friction that must be necessary to detach the colouring matter from the globules of blood. And since the vis a tergo is said to be sufficient to break down the inflamed tissues, and to force an abscess to the surface, it is not a little singular that such a power should not occasionally expel the red globules into this unresisting matrix.

But, if the colouring matter be wiped off, as supposed, what should prevent each succeeding globule of pus from pushing it out of the vessels? Or, if it be still insisted that the colouring matter cannot follow its globule, with the aid of the ejecting power of the succeeding globule, we then ask what disposition the hypothesis makes of the vast amount of colouring matter that ought to be accumulated in the "obstructed" tubes? Can the hypothesis explain the long continued disappearance of the colouring matter and other principles of the blood but by admitting that pus is a new formation, and that it depends upon forces and actions which have no analogies in the inorganic world? Nor should it be forgotten, that at this stage of the process, it is universally concluded that the blood has "concreted into irregular masses in which the *distinction of the globules is no longer perceptible*." (3)

Suppose, however, that Home, Hodgkin,(4) Autenrieth, Brug-

(1) See Appendix on Microscope. Zimmerman asks, "what would Aristophanes have said, had he seen the moderns analyzing the blood of a flea?" — *On Experience in Physic, vol. i. p.* 111.

(2) Mr. Earle, Op. cit. p. 189. (3) Alison, ut cit. p. 431.

(4) Hodgkin says the globules of pus "bear no resemblance to those of the blood."

mans, Lister, and some others, are right in their statement that the globules of pus are formed from a gelatinous fluid that is first effused, it would be equally fatal to the hypothesis we are considering. (1)

It appears to us, that, not only the exclusion of the red globules from the suppurating vessels, but also from the serous and lymphatic vessels, may be most philosophically explained upon vital principles. Taking the mean of the estimated size of the globules, it is impossible to imagine the existence of organized vessels so small. In another, and more exact science, they would be called "mathematical lines." We are willing to allow that some conjecture may be formed as to the diameter of the red particles; but we hold it to be absurd to attempt the measurement of vessels, which, according to the hypothesis, must be as invisible to the naked eye as the particles themselves. (2) But since the ultimate vessels must have their coats, and these their vasa vasorum, probably carrying also red globules, their sensible dimensions must be admitted; and we must, therefore, seek for another principle which shall explain the exclusion of the red globules from vessels of a greater diameter. This is to be found in the different modifications of their irritability, in Haller's, Hunter's, and Bichat's sense of the word; just as is manifested in the elective function of the excretory vessels of the liver, kidneys, &c., and, as seen in the refusal of the lacteals to take up bile, or cathartics. (3) (Vol. I. pp. 509, 526, 548 — 575, 601 — 608.) Hence is it, that the products of inflammatory

(1) The supposition that the globules are of extra-vascular formation is, prima facie, inadmissible. It violates a great principle in physiology; since it is established that all other animal globules depend upon the action of vital forces. It appears to us that such a law ought not to be sacrificed to the microscope. Müller has an assumption about it, founded upon the hypothesis that the capillaries have no orifices. — *Elements of Physiology, vol. i. p.* 425. — See our Appendix on Endosmosis, vol. i.

(2) Treviranus and others, make the diameter of some of the capillary blood-vessels only 0,0049 millimetres, whilst they estimate that of the red globules at 0,005 or 0,006 millimetres. — Hake says "the globules and capillaries are of the same size."

(3) Bichat has given us a most intelligible explanation of this phenomenon, as exhibited in inflammations, in conformity with the laws of life. — *See his Recherches Phys. sur la Vie, et la Mort*, p. 125 — 136.

Breschet's idea, (*Le Système Lymphatique, &c.* c. 3,) that the globules of blood are too large for the orifices of the lymphatic vessels is founded upon too many premises that are assumed. Besides, the globules of blood, and the larger ones of pus, are known to find their way into these vessels. Again, do they enter without corresponding orifices?

action are constantly varying, or red globules escaping from the vessels, according to the modifications of the vital properties; and these, again, are remarkably denoted by coincident variations of the vital signs.

By some, it is admitted that the introductory stage of inflammation is simply one of arterial congestion,

"Characterized, at first, by an increased activity of the vessels and influx of blood, various degrees of turgescence, &c.; and, secondly, as the congestion increases, a laboured, slow circulation arising from the over-distension of the vessels, and an increased thickness and viscidity of the blood."

It might be supposed, that an increased diameter of the vessels, (and whilst the blood is admitted to retain its fluidity, and the vessels to possess an increased action,) would be particularly auspicious, upon the mechanical hypothesis, to the various effusions, as well, also, to an undisturbed circulation of the blood. But, it is not so considered.

"The period of inflammation," the writer continues, "is characterized by an entirely new order of morbid changes. The circulation is completely interrupted, the blood coagulates, clogs the vessels; some of the vessels are ruptured, (how?) and there is an extravasation either of blood or of coagulated lymph in the parenchyma; lymph and serum are also exuded, and the deposition of the new products leads to a decided change of structure." New vessels then appear, which are said, in philosophical conformity with the hypothesis, to be "mechanically formed." (1)

This writer, however, appears to adopt M. Gendrin's explanation of the formation of pus, which differs but little from that of Mr. Earle, but which is so far even less intelligible, considering especially the imputed state of the vessels. The reader will not fail to remark, also, the collision which exists here in the report of the microscope and that which affirms the extra-vascular formation of purulent globules, and the absence of vascular orifices.

"After the inflammation," he says, "has attained its height, the circulation remains for some time stagnant, the vessels and intermediate cellular texture being both filled with coagulated blood and lymph; the colouring matter gradually disappears, the part assuming more and more an opal tint; small, yellow, soft molecules may be seen interspersed through the coagulum, and some of them agglomerating in large globules evidently purulent. A slow degree of motion gradually becomes apparent by the oscillation of some of the globules in some of the vessels; whilst a passage seems to be made for others through the softened lymph by the formation of new vessels, especially near the surface." (2)

M. Gendrin, however, very justly supposes that in this state of the vessels there can be no vital action, and ascribes the forma-

(1) Cyclopædia of Prac. Med. Lon. Art. *Inflammation*, p. 713.
(2) Ibid. p. 716.

tion of pus to a mere physical change of the blood, although we do not learn from him the philosophy of the change, nor *how the blood begins to move, how the colouring matter is detached and disposed of, nor how the pus, all at once, makes its escape* in such prodigious quantities from vessels which are receiving no fresh supply of blood, which are blocked up with the coagulum, and which are destitute of all independent action. Nor does it appear to us an improvement of the philosophy, that M. Gendrin admits that, in less embarrassed states of the vessels, pus depends upon a vital process, and that it is then "une véritable sécrétion morbide. (1) Most of the able writers of the school with which we have now the honour of holding our argument, have agreed as to the *intra-vascular* conversion of the stagnated blood into globules of pus. Thus, Professor Alison :

"It is by a subsequent transformation of globules of the blood which have stagnated, and been partially decolourized, that the globules of pus are formed." (2)

Nor do we see that Professor Naumann's explanation does more than confirm the difficulties.

"The physiological rationale of the development of inflammation," says this philosopher, is "briefly as follows. The blood accumulating gradually in the capillary vessels, owing to the congestion in a part, becomes more and more a source of abnormal irritation to the nervous ramifications. The globules of blood become more and more crowded together. Owing to the impeded motion of the blood, its fibrin, (from its organic affinity to the cruor,) will be most easily held back, whilst the rarer parts will find their way into the venous stream. When the increasing stagnation ceases to admit of this, the serum of the blood at length penetrates the coats of the capillary vessels, and collects itself within the cellular texture, in the shape of what has been called puriform serum. At the same time the fibrin and the globules are retained in close conjunction with each other in the capillary vessels." (3)

The foregoing is also nearly the rationale of Mr. Earle, but wants its simplicity in bringing to the pure elements of physics the "abnormal irritation of the nervous ramifications," without any intelligible connection. (Vol. I. p. 714, &c.) How the coarser fluids ultimately escape, does not appear in the journal from which we make the extract. But the Professor, being of Müller's school as to the non-existence of vascular orifices, was probably arrested by the mechanical obstacle. Mr. Earle, and

(1) Hist. Anatom. de Inflam., t. 2, § 1461, 1463, 1471, &c.
(2) Outlines, &c. p. 439.
(3) British and Foreign Med. Rev. No. 5, p. 214.

others, however, state that inflammation does not decrease with the suppurating process; that Mr. Hunter was wrong in considering this process a termination of inflammation, (1) and that, of course, the vessels remain obstructed as usual. And this is perfectly philosophical. For if stagnation and coagulation of blood were necessary to the first effusion of pus, it must be equally necessary so long as this product continues to be formed, however enormous may be the discharge. Such is ever the attitude of physiologists when they attempt to resolve the processes of life by physical laws. We may say, also, that it appears to have been overlooked, that when the effusions become most abundant, the force of the vis a tergo, which is the only agent the hypothesis acknowledges, is on the decline; whilst there may be little or none when the heart is pulsating with the greatest violence. The same, too, may be often affirmed of hæmorrhages, the secretions of bile, urine, serum, mucus, &c. The plain reason is, the motions of the capillary vessels are essentially independent of those of the heart and arteries, and are varied according to the existing state of their vital forces. (See Essay on *Venous Congestion*, Sec. 11.)

We now come to that part of the prevailing doctrine of inflammation, which expounds the philosophy of the removal of parts

(1) And yet, strangely enough, Mr. Hunter is represented by some as regarding suppuration as an absolute inflammatory process, when he everywhere designates it as a "termination of inflammation." Mr. H. considered it a specific mode of action growing out of inflammation, as he did, also, that which results in a preternatural formation of lymph. He did not regard the latter at any time, as some late writers have done, as exactly analogous to the process of nutrition, and therefore the result of a healthy condition of the vessels. The analogies, which have been brought by some philosophers from animals of the lowest organization and the vegetable kingdom, cannot obtain till it be more fully shown that the organization and vital properties of complex animals are equally allied to the former. We allow a great latitude to analogies where organized matter is concerned; but this is one of the results where the analogies must extend at least as far as organization to justify the induction. The attendant phenomena denote a special modification of action, which not only require our premises to resolve the physiology into a process of nutrition, but, having obtained them, we see not then how the phenomena are to be set aside without a disregard of paramount facts. We may carry this doctrine a step farther, and affirm, in the language of Wilson, that although "another action arise in a diseased part, which has a tendency to restore it, this is different from the natural action of the part; and, although it be not in itself a diseased action, it takes place only in consequence of disease." (*a*) We may find an easy solution of this apparent mystery by a proper reference to the nature of the vital forces.

(*a*) Wilson on the Blood, &c. p. 229.

interposed between an abscess and the surface, and which explodes Mr. Hunter's theory of the agency of the absorbents.

This hypothesis ascribes the phenomenon to the previous results of inflammation, and the mechanical power of the heart, by which all surrounding tissues are broken down; although it is not shown how parts that disappear in this process are removed. It is said by the mechanical philosophers, that,—

"As the course of the blood in the capillaries is, generally speaking, from the centre towards the circumference, such is the direction of the vis a tergo, and such must be the usual course of the pus. It cannot penetrate from the circumference towards the centre, because there is no force by which it can be impelled in that direction." "The first effect of inflammatory infiltration is to destroy the original firmness and tenacity of every tissue in which it occurs, so as to cause it to be readily broken down, and give way on the slightest pressure; bone, cartilage, fibro-cartilage, are all affected alike." "It is evident, that, although there must be a general yielding over the whole of the inflamed part, it will be most decided precisely upon that point upon which the greatest degree of pressure is exerted." (1)

In the first place, it appears to be here overlooked that suppuration occurs "precisely at that point" where the instruments of disease are said to be most obstructed; and, therefore, according to the hypothesis the *least* degree of pressure should be exerted upon this particular part. But this is certainly the only consistent solution that the mechanical doctrine of inflammation will admit. It must adhere to its fundamental principles, however facts may conflict with each other, and the principles with facts. A single exception, also, to the foregoing rationale must be fatal to it. In peritoneal inflammation, the force of the vis a tergo is as much towards the cavity of the abdomen as the surface of the body; and yet, when suppuration takes place, the matter penetrates through fascia, muscle and skin, although the peritoneum is the only obstacle to be overcome in the other direction. We may also suppose that the downward and increasing pressure of the accumulated matter is quite equal to the vis a tergo in the capillaries; especially as the latter is generally supposed by the mechanical theorists to depend wholly upon the heart. The same reasoning is applicable to many other parts, where the force of the vis a tergo is directed as much below an abscess as toward the surface. A striking instance exists in abscesses of the liver.

As to the manifest agency of the absorbents, it is no more remarkable that their operation should be restricted to the anterior

(1) Mr. Earle, Op. cit. p. 189.

wall of an abscess, than that they should be limited to the absorption of chyle in one part, or that blood should be excluded from the whole, or than many other peculiarities of the absorbent system, which so clearly demonstrate the vital nature of its functions. We cannot interrogate nature as to the principles upon which her vital phenomena depend, beyond the phenomena themselves, or their analogies. We must be content with the results, and through them to infer the existence of laws as indispensable to the phenomena, as the soul is to thought or to voluntary motion. But we may find a satisfactory reason, upon vital principles, for the greater activity of the absorbents of the anterior parietes of an abscess in the greater activity of inflammation in this part; whilst some tissues, especially the serous, are but little liable to ulceration; a phenomenon which is also greatly at variance with the mechanical rationale. Mr. Hunter is certainly ambiguous, when he ascribes the progress of an abscess towards the surface to a species of intelligence. Still it is a figurative explanation, and like his analogous illustration of the properties of life, was designed for reflecting minds; as was also his exactly opposite, and more unhappy comparison of the organized system to the mechanism of a watch, and other contrivances which depend upon physical powers. He appears to have been sensible of the philosophy, that the progress of an abscess towards the surface is the result of a general law, by which inflammation is more readily excited, and ranges higher in superficial parts, than in the deep seated. (1)

We come next to the granulating process; and this should be as much a mechanical one as the antecedent. Accordingly it is said, —

"The same simplicity reigns in this as throughout every preceding part of the process of inflammation." "It is as easy to explain the whole process of ulceration and mortification, as it was to account for the opening of an abscess, without even hinting at the existence of the lymphatic system."

(1) This subject might be greatly amplified. We might inquire, "if it be the first effect of inflammatory infiltration to destroy the original firmness and tenacity of every tissue in which it occurs, so as to cause it to be readily broken down and give way on the slightest pressure," why this disintegration is so often absent in inflammations where great infiltrations occur; why it only takes place in the declining stage of inflammation, and becomes most rapid after the force of the vis a tergo has more or less abated; why suppuration is always necessary to the formation of granulations on a superficial ulcer, &c.

And with scarcely more than this simple declaration; the all-important subject of absorption is dismissed; it being considered

"Unnecessary, and would be *very inconvenient*, to inquire into the foundation of Mr. Hunter's opinions on the subject of absorption in this place." (1)

This is evidently a stumbling-block, and is wisely avoided by consistent philosophers of the mechanical school; who see in the imprudence of some that have adopted Mr. Hunter's philosophy of ulceration, an admission that the antecedent stages of inflammation are likely to be as little dependent upon mechanical causes.

"As the fresh fibrin," it is said, "exudes through the original layer by the channels or *perforations*, it will coagulate around them in little *heaps*." (2)

But we are not told upon what mechanical principle the fibrin, or the "little heaps," make their appearance, all at once, after an indefinite continuance of the suppurating process; nor is there any comment upon the curious fact that the granulations themselves emit a purulent matter. It is stated, however, that "there can be no essential difference between suppurative and ulcerative inflammation." (3) But there is certainly this manifest difference in the results, (as the terms are here employed,) that pus is formed in one case and granulations in the other; that one is a destructive, whilst the other is a restorative process; though, in reality, the generation of pus and of granulations may go on simultaneously, and the latter become the instruments by which pus is produced. Nor should it be overlooked, that the mechani-

(1) It is of no moment to our present purpose, whether the lymphatics or the veins perform the office of absorption. Before the discovery of the lymphatics, absorption was attributed altogether to the veins; and they are now supposed to be equally concerned in absorption by those who deny the special agency of the lymphatic system. The objection, therefore, which has been raised to the vital theory of ulceration upon the ground of error as to the function of the lymphatics is clearly untenable. (*See Mr. Earle, ut cit. p.* 218.) But the observations of Fohmann, Lauth, Hodgkin, Lippi, Double, Breschet, Amussat, Meckel, Blizzard, Lobstein, Lizars, Mertens, Panizza, Werner, Caldani, Carson, and others, as to the termination of lymphatics in small veins, are likely to remove all ambiguity from this subject, and restore the doctrine of absorption as illustrated by Hunter, Hewson, and Cruikshank. We may say, also, that analogy is opposed to the doctrine of venous absorption. Every system of vessels, so far as known, has but one function, however that may be modified in different parts, as seen in the lymphatics and lacteals. The distinction depends either upon structure connected with the modifications of common vital forces and their relative adaptation to the properties of the different fluids; or structure may be apparently less concerned than the latter, which is one of the most universal and important principles in physiology.

(2) Mr. Earle, Op. cit. pp. 218, 250, 219.

(3) Ibid. p. 222.

cal hypothesis, true to its principles, makes no distinction betwixt pus and lymph.

But suppose, instead of a healthy granulating ulcer, we consider a destructive one, such as the syphilitic or cancerous. How, we would ask, is the removal of parts effected in such instances? Why is it often necessary to employ constitutional and local remedies, sometimes mercury, and sometimes lunar caustic, to promote the effusion of coagulable lymph? How are the supposed mechanical powers affected by these agents?

From whatever parts of the body granulations spring up, they have all originally the same appearance; the same in bone as in muscle, &c. But they are ultimately changed into the nature of the tissue from which they proceed. We know not the cause, but we see the effect; and this effect assures us of a specific action in the different granulations, which, with other accidental, though analogous modifications of the vital properties, accounts, in part, for the differences in purulent matter, as well as its common generic nature, and derides every attempt to solve the problem by chemical or mechanical laws. (Vol. I. p. 627.)

Finally, we come to mortification. And here we must not be surprised, having hitherto so entirely lost sight of the vital powers and actions, that they are allowed no part in the curious process that attends the separation of the dead from the living substance. The vis a tergo, the heart alone, admirably counteracted by a complete stagnation and coagulation of blood in the seat of the mechanical operations, is the only agent by which all the phenomena are carried on, from the beginning of inflammation to the ultimate stage of sloughing. But, it is said,

"The separation of the dead from the living parts has, since the time of Mr. Hunter, been supposed to be performed by the agency of the absorbing vessels; nor does any one (so far as known to Mr. Earle) doubt the correctness of this branch of Mr. Hunter's favourite doctrine."

Our author is the only philosopher who has carried out all the details of the hypothesis in conformity with the rules of science and the laws of nature. With all others of the mechanical school, there has been some demand, in difficult emergencies, upon the powers of life, even by those who argue against the existence of such powers. But our author sees that their occasional introduction must be fatal to the whole hypothesis; and, accordingly, we are told that the absorbents have no more to do with "the separation of the dead from the living parts,"

than they have with the process of ulceration. And the same reason for rejecting the absorbents that was before stated is here repeated, viz. because "a full and complete explanation of every circumstance connected with mortification can be given, without even alluding to the existence of the lymphatic system." (1) If nothing can be easier or more simple, certainly nothing can be more consistent. Fluids, as we have seen, are mechanically expelled.

"The first effect," it is said, "is that of destroying the natural tenacity of the solids, of rendering them, as it were, *rotten*, so as to be easily broken down by the slightest pressure. Of course, this effect must be most apparent at the point where there is most effusion, which must be exactly where the living touches upon the dead part. Thus it happens that *the force of the vis a tergo, by whose pressure the effusion of the different parts of the blood is effected*, impinges precisely upon those points which are the weakest and most likely to give way; by which means the dead part is gradually detached from the living, and, as it were, washed off by the discharge." (2)

Since we have endeavoured to show that the mechanical hypothesis, as applied to ulceration to explain a similar process, and indeed at every stage of inflammation, is not only deficient, but contradicted by facts, and by all analogies in relation to living organized matter, it may be considered equally groundless as it regards mortification. There is nothing more marvellous in the supposition that dead parts may stimulate the absorbents, whose action is already modified, and thus prepared to remove the contiguous living parts, than there is in any other vital phenomenon. The principle is admirably shown in the manner in which hepatization of the lungs gives way to a return of the healthy structure.

But, it is important to recollect that the vessels in mortification, extending within the confines of the sound part, are supposed to be more than ever obstructed with coagulated blood, and that it is in virtue of this accident that the part dies. (3) It

(1) Mr. Earle, Op. cit. p. 250. (2) Ibid.

(3) It is a remarkable fact, that the experiment with quicksilver, by M. Cruveilhier, is quoted by M. Andral, (*a*) and others, to substantiate this doctrine. Cruveilhier "succeeded in producing gangrene of a limb, by injecting the minute arterial ramifications of the part with quicksilver." Doubtless; and by thus mechanically destroying the actions of the vessels. But does it therefore follow that nothing but a mechanical cause can destroy their vitality? Or, allowing that the hypothesis of stagnation may be possibly wrong, where is the analogy between blood and mercury? What shall be said of the admitted fact, that "gangrene, in numerous cases, is di-

(*a*) Patholog. Anat. vol. i. p. 32.

does not, therefore, so clearly appear how the forcing influence of the heart is to reach the sloughing part.

We might go on to illustrate this subject by analogies drawn from the vegetable kingdom; and inquire how far the removal of their sloughing parts depends upon the mechanical action of the *vis a tergo;* how far their reproduction upon chemical or mechanical laws, &c.; and, lastly, we might farther show that one hypothesis has been built upon another, and these resting upon assumptions, to account for the precise, yet diversified and unique phenomena of inflammation.

As to the cause of mortification, we need not the aid of the mechanical doctrines, excepting so far as the circulation ceases when the part dies. It is as easy to understand how the life of a part may be destroyed by the alteration of its powers and functions which constitutes disease, as to comprehend the fact of that alteration, or the manner in which constitutional fever extinguishes, almost at the moment of its invasion, the entire life of the body. There is something, indeed, in the rapidity with which decomposition sometimes goes on in mortification from inflammation that is without any analogy in chemical philosophy, and which can be only explained, if we may hazard the expression, upon vital principles. (1)

rectly proportional to the alteration of the nervous influence." (*a*) May not the imputed stagnation be the consequence, and not the cause of death? Some philosophers do not consider that stagnation which constitutes inflammation, or the progressive operation of its cause, sufficient to explain the destruction of parts. Still the theory is made consistent. Thus, Sir C. Bell: "when phlegmon terminates in the death of a part, it is the effusion and infiltration into the cellular membrane which checks and hinders the action of vessels." (*b*) And so, Dr. Carswell. Mortification "depends, in a great measure, on the mechanical influence exercised by the effused fluids on the capillary circulation. These fluids must compress the neighbouring veins to a degree that will prevent the return of the blood poured into the capillaries." (*c*)

But, is there any proof of the foregoing? Is the pressure, where effusion is greatest, in any degree comparable with what may be artificially exerted upon a part without inducing mortification? Besides, mortification often occurs without any remarkable effusion, whilst another, and greater objection consists in the every-day fact that far greater effusions, and these too of solid lymph, are suddenly taking place without producing the slightest tendency to mortification.

But, after all, it appears to have been quite unnecessary to have superadded the foregoing case, since we are told by its advocates that "in the second stage of inflammation, the blood which distends the capillaries ceases at last to circulate." (*d*) It is not said by any writer of this school, what maintains vitality afterwards.

(1) "Even when the life of organic bodies is extinct, we should consider the quali-

(*a*) Andral, Op. cit. p. 33. (*b*) Institutes of Surgery, &c. vol. i. p. 38.
(*c*) Illustrations of the Element. Forms of Dis. Fas. 7. (*d*) Carswell, Ibid. Fas. 12.

In having thus endeavoured to sustain the vital doctrine of inflammation, we certainly do not recognise a *vis medicatrix naturæ* in the sense in which it is ridiculed by Mr. Earle and others; nor do we think that the construction which is forced upon those who adopt it is justified by any fair interpretation. Provident nature has so ordered many of the actions of disease, that they shall ultimately terminate in a sanative process; and instead of the interposition of any latent and mysterious agent, the final result is brought about by the same vital powers, and by the same instruments, that had been engaged in the morbid process.

"The good God that created the animal stamped upon his organization a tendency to actions, and a power of originating, adapted to the repair of a certain amount of injury." (1)

This, we believe, is all that medical philosophers of the present day, with rare exceptions, intend by the *vis medicatrix;* nor did Mr. Hunter employ it in any other sense, however he may have figuratively ascribed to it a principle of intelligence for the purpose of strongly illustrating those results which are more allied to the operations of instinct than to any physical processes.

We perfectly agree with Mr. Earle, that, "although the progress of improvement may be, for a time, retarded by too great a deference to authority, it is impossible that men's minds should remain impervious to the direct evidence of simple facts." But, should it be otherwise, and the mechanical doctrine of inflammation ultimately prevail, then every process of the living body, whether morbid or healthy, must be construed by the same rule; unless, perhaps, a different principle be necessarily admitted to preside over the heart, just as the Berkeleyan skeptic is forced to the recognition of consciousness. Such, however, is our confidence in the principle of the foregoing extract, we are content that the whole subject of inflammation shall be tested by those mechanical laws which our friends have assumed as its foundation. We have seen, indeed, that the doctrine of passive relaxation of the vessels, and the consequent stagnation and coagulation of blood, is contradicted by its own premises; by the phenomena of increased heat, — throbbing in the immediate instruments of disease, when the force of the heart is even pros-

ties which they possess, from the time of death to the complete resolution of organization, as the results of the *organic powers* which have been active in them." — *Tiedemann's Comp. Physiol. p.* 29.

(1) Dr. Johnson, in Med. Chir. Rev. vol. 20, p. 328.

trated, — the greatly increased volume of blood circulating in these instruments, and its general increased fluidity, — the augmented force with which the blood escapes from the vessels when divided, showing not only an undisturbed circulation, but a positive action of the vessels themselves, — the remarkable varieties of inflammation, and their peculiar products, — the effusion of serum, lymph, pus, and finally, of blood, especially in the inverse order of the diameters of their globules, — the exclusion of the more fluid parts, whilst the grosser alone escape, — the disappearance of the colouring matter, — the extraordinary processes of ulceration, reproduction, and sloughing, — and, finally, the causes and curative means of inflammation. On the other hand, whilst the foregoing doctrine leaves nothing to the vital forces and actions, these are capable of explaining every phenomenon of inflammation.

We shall now notice an experiment lately made by Professor Alison, since it is supposed to be an *experimentum crucis*. To ascertain whether "the arteries leading to an inflamed part are really endowed with a greater vital power of contraction than sound arteries," Professor A. at different periods after killing the animal, compared the vessels of the inflamed limb with the corresponding ones of the opposite sound limb. At a second examination, sixteen or twenty-four hours after the first, the artery of the inflamed limb still remained larger than the other. That of the inflamed limb retained a considerable quantity of blood, whilst the other was almost empty. (1) Here is no room for mistake, no microscope, no vital forces to be equivocally explored. We cannot think, however, that the experiment proves anything more than that the arteries contract after death, as is generally believed, partly by virtue of their elastic property. This property had been impaired by the diseased state of the vital forces; and no induction can therefore be made from it as to the state of those forces which are nearly or wholly extinguished. Indeed, it was a part of the experiment to prove that the elasticity of the artery of the inflamed limb was impaired. In the natural state of parts, that property is evidently modified by the forces of life; and it may even be that it is increased in inflammation, however its manifestations are diminished after death. The error consists in supposing that the same morbid actions exist after death as before. Nor should it be overlooked

(1) Lon. Med. Gaz. vol. xvi.

that the proportion of blood in the instruments of inflammation may be thrice as great as in the healthy capillaries, and, therefore, to place them on a par after death, thrice the amount of contraction should affect the former.

From the foregoing experiment a new hypothesis comes up; that the vessels in inflammation are not only in a state of relaxation, but that there is an increased action by which the blood is moved in the capillaries of the part, resulting from powers inherent in the blood itself, and independent of any contraction of the blood-vessels, "the action being not *in the vessels, but within the vessels.*" This principle has been supposed by many to appertain to the blood since the time of Harvey, and Haller; but it is now asserted that it is increased in inflammation.(1) The increased exertion of the powers of the blood leads to distension, and a diminution of the tonicity of the vessels. How far this may be a matter of speculation, or inferable from facts or from reason, we shall not attempt to show. We cannot help thinking, however, that it is so diametrically opposed to the doctrine of stagnation and coagulation of the blood, that it betrays the hypothetical nature of every theory which departs from the fundamental principles of life. Nor should we neglect to observe the imposing contrast which is presented by the denial of all action, in inflammations, to those vessels upon which the formation of blood and nutrition essentially depend, whilst the blood itself is considered not only the cause of its own motion, but, through its increased velocity, of the loss of motion in the vascular system.

Admitting that experiment has failed to demonstrate the actions

(1) This hypothesis seems allied to that of Dr. Curry, who supposes that, in inflammation, the circulation is promoted by an electric repulsion between the blood and its vessels. (*a*) But, as to any independent motion of the blood, as surmised by the foregoing physiologists, by Tiedemann, Dœllinger, Wolff, Gallini, Treviranus, Schultz, Dutrochet, Schrœder, Bohn, Carus, Pander, Prevost, Dumas, Bichat, Albinus, Kielmeyer, Desterreicher, Alison, (*b*) Hawley, (*c*) and others, as well as by a class, especially the French observers, who regard the globules as animalcula, we may safely take the authority of Müller upon this question, who — "during the last ten years has examined the circulation of the blood in the most various parts, *at every opportunity*, and with different instruments," but "he has not discovered the slightest appearance of spontaneous independent motion of the individual red particles." Other conclusions he naturally supposes are founded on microscopical deceptions. (*d*) — (See vol. i. p. 707.)

(*a*) Syllabus of Lectures at Guy's Hospital, 1810.
(*b*) Trans. Brit. Association, 1835.
(*c*) Edin. Journ. Oct. 1836.
(*d*) Elements of Physiology, vol. i. p. 143.

of the extreme blood-vessels; we are allowed, by the common rules of philosophy, to infer their existence from the phenomena. These, indeed, as they are naturally presented, are the safest grounds for every conclusion. And why are the minute vessels so eminently endowed with the vital forces, unless for the performance of independent actions? Coming to disease, we find new orders of vital phenomena, from which we rationally infer some change in the foregoing properties, which as clearly denote corresponding modified actions. Thence we see how groundless is the objection predicated of analogy by M. Sommé, and others, in saying "it has been supposed that the extreme arteries can be no longer under the influence of the heart, and a property has, therefore, been ascribed to these minute vessels which has been denied to the larger arteries,—that of contracting independently." (1) Here it appears to be overlooked, that the extreme vessels are mainly the seat of the vital phenomena, and that the larger vessels serve, principally, as conduits to the former.

It is said by an able writer, that—

"The question whether these (capillary) vessels do, by their vital contractions, add directly to the force which *propels* the blood, is perfectly distinct from the question whether they do, by vital actions of some kind, regulate the *distribution* of the blood. The first question, we conceive, must be answered in the negative; because, admitting them to possess a vital power of contraction, the exercise of this power would antagonize the action of the heart, instead of aiding it." (2)

The writer does not state the ground of this conclusion, and we think it would be difficult to establish it even by any process of reasoning. Analogies in respect to mere physics are out of the question; and who shall say that the contractions are not conducted in such a manner as to subserve the general circulation? May there not, by possibility, be a corresponding dilatation, which is not taken into the account? (See *Venous Congestion*, Sec. 10.) That we are right in this philosophy is shown by our author himself; for he answers the second question affirmatively. Thus—

"If we consider the capillaries as endowed with *a vital power* (3) of diminishing their tube, and thereby lessening the quantity and *augmenting the velocity of the blood which passses through them*, and of dilating and thereby increasing the quantity and diminishing the velocity, we must admit that these vessels, by

(1) Etudes sur l'Inflammation.

(2) British and Foreign Med. Rev. Oct. 1839, p. 335.

(3) We like this expression. See our vol. i. pp. 11, 714.

their vital actions, contribute most powerfully to the circulation of the blood," &c. "Do the capillaries, then, possess such powers? We think there is evidence, little short of demonstration, that they do."

Here, then, are all the elements that are necessary to independent action, and we see not why the "vital power," or as the reviewer has it in another place "the vital principle," should not produce a successive series of alternate contractions and dilatations; and since the act of "diminishing the tube augments the velocity of the blood," we see not but it must follow that "the capillaries, by their vital contractions, add directly to the force which *propels* the blood," even if it were not ultimately admitted that they "contribute most powerfully to the circulation of the blood." (See *Venous Congestion*, Sec. 11.)

If the vessels in inflammation be actually weakened, passively dilated, clogged up with coagulated blood, when does their tone return, and by what cause is it rëestablished? If by the vital forces, when and how came they again in operation?

It being supposed that the vessels are weakened in inflammation, it has been necessary to assume that the causes are debilitating; and it were well that the subjects, also, should be amongst the feeble instead of the robust. It seems, however, to be generally in all respects otherwise. For instance, pneumonia affects mostly the robust and plethoric. Cold, when suddenly applied is one of its exciting causes, which, in its ordinary operation, is tonic. The subjects of fever are mainly of the same class, and inflammations are apt to supervene in proportion to the vigour of the patients. Stimulants and direct tonics scarcely fail, in these instances, of developing inflammation. Were the causes, also, really debilitating, or the pathology of the imputed nature, the remedies should be invigorating; but, the only efficient ones consist of bloodletting, cathartics, tartarized antimony, &c.

"Repletioni evacuatio, evacuationi repletio, labori quies succurrit, et quietis medelam ex labore reperias; atque ut breviter dicam, *contraria contrariorum sunt medicamina*," (*τα εναντια των εναντιων εστιν ἰηματα.*) (1)

(1) Hippocrates, L. de Flatibus, ver. 25. — That most frivolous and pernicious doctrine of the age, homœopathy, has surreptitiously borrowed from Hippocrates one half of his philosophical principles as to the curative means of disease, whilst it has utterly perverted the other half in imputing it to the Hippocratic school as an exclusive dogma. We shall say nothing here as to the supreme absurdity of the infinitesmal doses of the Lilliputian school; but proceed to quote from the illustrious founder of the best principles of our art a passage which embraces the whole doctrine in its proper connection. It has never been surpassed in modern philosophy. Thus:

"Febris, quæ propter tumefactionem ex pituita fit, aliquando quidem ab iisdem

Nor can it be contended that debilitating means are necessary to remove the increased momentum of blood, since the

fit, et sedatur; aliquando autem a contrariis. Et si quis vult medicamentum bibendum dare, alvum subducens, et vomitorium, eadem modo et a facientibus sedatur, et a sedantibus fit. Nam si quis homini vomenti, aquam multam bibendam dare velit, eluentur ea, propter quæ vomit una cum vomitu. Et sic quidem per vomitum vomitus sedatur. Rursus autem propterea quod sedatur, quia inferne procedere ipsi fecit, ex illo id, quod intus est, vomitum facit, et *ambobus contrariis modis homo sanis fit.* Et si quidem sic in omnibus haberet, stata sane ac certa medicina esset. Ita alia quidem contrariis curare oportet, qualia tandem sint, et a qua causa fiant; alia vero similibus, qualia tandem sint, et a qua causa fiant."—*L. de Locis in Homine*, s. 2, ver. 241—248.

And here is an illustration of one part of the doctrine. "Quum vero omnis generis morbi fiunt eadem tempore, et palam victus singulos singulis causas esse, curam sane facere oportet ex contrario instando adversus morbi causam, quemadmodum et alibi a me relatum est," &c.—*Ibid. L. de Natura Hominis*, ver. 180.

All the writings of Hippocrates are imbued with the foregoing philosophy, which is also fundamental in modern physic. (*a*)

(*a*) It has occasionally happened that gentlemen who belong to the science of medicine have so far recognised homœopathy as to justify an experimental inquiry. But we have generally found that this tolerant part of the profession have either no definite views in physiology, or are prompted by what we cannot but regard as a mistaken liberality. (Vol. i. p. 388—389.) That we may not be suspected of any personal allusion of disrespect in what we have now said, and that no factitious importance may be given to what we have considered a misguided liberality, we will say that it is wholly upon the latter principle that a distinguished professor of the New-York school of medicine was induced to suggest "the propriety of making the comparative merits of allopathy and homœopathy the subject of a prize dissertation," (*aa*) thus, by implication, even conceding the possibility of superior claims to the latter.

From the foregoing circumstance, and from homœopathy having obtained some consideration in the city of New-York through the influence of its novelty, and having abandoned itself to that last expedient of charlatanry, the dissemination of its pretensions through popular journals and newspapers, and above all, as it represents itself as still on the ascendency in Europe, we shall quote from the Medico-Chirurgical Review of July, 1835, the following abstract of a discussion at the Royal Academy of Medicine, which had been recently held in Paris:

"M. Andral, senior, exclaimed, with much emotion, 'I much doubt whether the Minister has any right to require a report from the Academy on an absurdity. The President should write to the Minister, and expose the cheats and juggleries of these rogues, who call themselves homœopathists. I protest against the appointment of any commission, and, therefore, move the order of the day.

"M. Londe.—Write to the Minister that they have been tampering with his credulity, and that the Academy cannot condescend to have anything to do with such charlatans, who will, no doubt, avail themselves of the very present discussion to announce, in to-morrow's paper, that the Academy were engaged with the consideration of the very important subject of homœopathy.

"M. Lepelletier.—Yes! homœopathy is, indeed, a very imposture; but here we have an opportunity of exposing and abolishing it. Let us accept the challenge that is offered.

"M. Keraudren suggested that an application should be made to some of the German societies, for information relative to the indigenous homœopathists.

"M. Marc.—In Germany, homœopathy has fallen into the most utter contempt. A distinguished German professor assured me, the other day, that in Berlin there were only three homœopathists—one rogue and two fools.

"M. Breschet.—I was lately at a meeting of upwards of 600 German physicians and surgeons. Some one wished to discuss the question of homœopathy; but the proposal was rejected with such general disapprobation, that even the bare mention of a subject, which is believed only by quacks and impostors, was immediately scouted."—(See our vol. i. p. 632, *note.*)

Homœopathy having become extinct throughout Europe, we have thought it our duty to bestow only this notice as a record of its lingering existence in America, and as one of the signs of the times. A modification of the practical feature of homœopathy is undergoing an introduction to the

(*aa*) Professor Mac Naughton's "Annual Address" before the New-York State Medical Society.

whole inflammatory condition existed before the increased force of the general circulation began;(1) whilst it often happens, as inflammation advances, especially when complicated with idiopathic fever, that the vis a tergo is prostrated, but again roused by the lancet, which simultaneously overcomes the local affection. These are obstacles which no contradictory hypothesis can withstand.

Finally, there can be no compromise with the mechanical doctrines of inflammation as attempted by some philosophers.(2) The hypothesis which allows an increased circulation in the primary stage encumbers the second, or period of stagnation, with greater difficulties, especially as it supposes that the increased momentum of blood is a concurring cause of its ultimate failure. The elements of the hypothesis are so diametrically

(1) Why does a more extensive inflammation "of an external part, than of a vital part, excite less increased action in the larger arteries of that part, and often none at all in those of the system in general?" (*a*) Why does inflammation often move suddenly from one part to another, when we see no cause either increasing the action of the capillaries of the inflamed part, or weakening those of the part secondarily affected? Why do these secondary inflammations spring up amongst remote organs which sympathize most with each other in their normal state? Or why is inflammation so apt to spread to neighbouring parts, between which and the part first affected there is no direct communication of vessels? What produces the horror and rigor, and the sympathetic fever? Is there anything in the mechanical hypothesis, or in the doctrine of debility, that will explain the phenomena? If nervous agency be admitted, this is a full concession to the vitalists, in itself. It implies the ulterior agency of other forces than those of physics, since it appears absurd to suppose that the latter are controlled by nervous power.

(2) "I shall consider inflammation," says Dr. Carswell, "as a disease, the essential phenomena of which present themselves in two successive stages or periods, each of which is referable to opposite conditions of the physiological properties or functions of the affected part." "In the first stage of inflammation, the circulation is accelerated, and a greater quantity of blood than natural passes into the capillaries; in the second, the circulation becomes impeded, and the blood which distends the capillaries ceases at last to circulate." — *Illustrations of the Elementary Forms of Disease, fas.* 12.

public, under the alluring name of the "American System of Homœopathy," at the moment our printer has reached this page; and we even "stop the press" to announce the public renunciation of "allopathy," by Dr. C. Tieknor, and the appearance this day, (March 3, 1840,) of a popular defence of homœopathy from the pen of this gentleman in a letter addressed "To the Hon. ——." An attempt is made to show that it has some remaining advocates of consideration in Europe. The names of such as are at all recognised in medical science have no more respect for the doctrine, than we have seen to have been affirmed of them by M. Andral, and others, in the foregoing quotation.

Having thus dwelt, for a moment, upon this unhappy subject, we shall conclude with a sentiment addressed by Seneca to a certain part of the medical profession: —

"Gravissima infamia est medici opus quærere, multi quos auxerant morbos, et incitaverant, ut majori gloria sanarent, non potuerent discutere, aut eum magna miserorum vexatione vicerunt." — *L. de Benef. c.* 36. (See vol. i. p. 673.)

(*a*) Philip's Exp. Inquiry, &c. p. 262.

opposed, we cannot but think that it exposes the absence of the necessary data in both.

Admitting the hydraulic illustration, so utterly inapplicable to living vessels, there is nothing in their simple dilatation that can tend to promote coagulation of the blood; since it is manifest that the motion of the blood can be but slightly retarded upon the hydraulic principle, and it is well known that only a very slow movement of the blood is necessary to its fluidity; whilst the enlargement of the vessels necessarily supposes an increased facility for the transmission of blood. But, it has been overlooked in this part of the mechanical hypothesis, that inflammations often spring up when the circulation is going its round at the rate of 120 or 140 pulsations in a minute, and when the blood has been reduced almost to the state of serum. In some constitutional fevers, it is a result to be apprehended almost in proportion to the velocity and force of the general circulation. (See APPENDIX, on *Fever*.)

SECTION II.

WE shall close these remarks with an argument in favour of the Hunterian theory of inflammation, drawn from the nature and formation of pus. If it can be shown to differ from the component parts of the blood, or to depend on a secretory process, it would appear to substantiate an independent action in the instruments of inflammation.

Mr. Earle, and the friends of the new school of inflammation, find it necessary to identify pus with a component part of the blood, and to explain its formation upon mechanical principles.

They contend that neither inflammation nor pus being met with in the healthy system, pus is not, therefore,

"A healthy secretion," and "it is as little entitled to be called a morbid secretion, because by this term is understood some change or deviation from that which is usually recognized as the healthy condition of a secretion. The term morbid secretion, therefore, applies to black bile, and to diabetic urine; but not to pus, which is an entirely new formation." (1)

This would appear, as we have said, to follow from the new

(1) Mr. Earle, in Lon. Med. Gaz. vol. xvi. p. 137.

doctrine of inflammation. For secretion is a process that depends on an active state of the vessels. But since the vessels are relaxed and encumbered with coagulated blood, it is reasonable to conclude that pus depends upon some other function more compatible with such a state of the vessels, and that, therefore, it is not a secreted fluid.

But there is nothing to show, save the objection we have stated ourselves, that new formations may not be as purely the result of a secretory process as bile or urine. We do not see the logic of the foregoing conclusion. Indeed, the universal admission that pus is a new formation is subversive of the mechanical hypothesis; since all analogy in relation to other new products from the blood declares the dependence of the supposed exception upon the same forces that are concerned in the generation of all others. (Vol. I. pp. 56, 57, *notes*, 585 — 590.) Man may bestow names upon natural phenomena; but in excluding analogous ones that may depend on a new condition of the powers and functions of the body, he must show their incompatibility with those laws of vitality upon which the natural phenomena depend. If there may be a morbid state of action, it seems very philosophical to suppose a morbid secretion. We cannot otherwise reason from analogies supplied by the healthy system, to diseased states of the system. In the latter case the modified power must be regarded as performing new functions. New products, therefore, may arise; and however we may choose to restrict the term *secretion* to healthy products, it does not follow that the morbid ones, although wholly new, may not be equally entitled to that appellation.

But do we not find in the very argument that is urged against the formation of pus as a secreted fluid, an illustrative fact that it is really the product of morbid secretion? It is admitted that there may be morbid bile, depending upon a morbid process. Now healthy bile is one thing, and morbid bile another. One action is natural, the other unnatural. Morbid bile has no more existence in the healthy system than pus, and it cannot take place without the morbid process. We see not, therefore, why, in a uniform modification of the vital powers and functions, as in common inflammation, the natural secretions of the parenchymatous capillary arteries may not be, in like manner, changed into pus. Mr. Earle and his friends consider pus the nucleus of

the red globules, and that nucleus is supposed to be coagulable lymph. And yet it is maintained that pus is a new formation.

But, since it is admitted that pus is wholly a "new formation," we ask for the proof that such formations do not depend upon a secretory process. An assumption has been made, and a syllogism invented to sustain it. But this cannot pass in opposition to a better syllogism; and, as we believe, to direct and unquestionable facts that are supplied by analogy, as well as by the laws that govern the living body. Does bone ever form in the healthy membranes of the brain; melicerous, steatomatous, carcinomatous, hæmatodic, and other specific products, take place in any part of the body without specific conditions of disease, and that disease in all probability, of an inflammatory nature? And will it be denied that these are the result of a secretory process?

Upon the hypothesis just quoted, we might, also, equally argue that pus is not separated mechanically from the blood, since such a phenomenon never occurs in the healthy system; and it is not pretended that its mechanical origin is supported by any analogy in disease.

Again, it is said,

"There is this essential distinction between a secretion and pus; the products of the secernent function are different in every membrane, texture, and organ of the body; mucus is secreted from the blood in vessels of mucous membranes, &c.; whilst no such variety is observed in the formation of pus."(1)

Admitting the latter statement to be true, we cannot see the force of the analogy. The liver secretes bile; the kidneys urine; the lachrymal, salivary, and cutaneous glands, produce tears, saliva, sweat, &c.; because the several organs are made up in a distinct and complex manner for this purpose. For this reason we should say, *à priori*, that the products would be different, and would vary in their composition according to the complexity of the organs. But in inflammation, it is not the glandular apparatus that is concerned, but the circulatory, whose action must be supposed to be very similar in every part of the body. Müller, however his statements may be incompatible in respect to the secretions, (see our Vol. I. p. 56, note,) goes so far as to say that

"The sole secretion, of which the constituents do not exist as such in the blood, but which can nevertheless be formed at all times and in all parts of the

(1) Mr. Earle, ut cit.

body, is *pus;* the *organ* for its production being *generated anew* in the process of inflammation." (1)

We should, therefore, in this case, also infer, *à priori*, that in the same conditions of inflammation, the products would be the same; that in one condition we should have serum, in another lymph, and in another always pus. And we may take from Mr. Earle, what he intended as an objection, another evidence that our conclusion is right. "As the blood is the same from whatever artery it is taken, and also, as inflammation is essentially the same, in whatever texture it may occur, the effect thereof must be the same in all." (2)

But we believe it is not true, that simple structures, which are admitted by our friends to perform a secernent function, do not have their natural products changed into pus by inflammation. Take the very one quoted above, by which the adverse hypothesis is sustained. Do we not often find, in inflammations of the mucous membrane of the lungs, that the secretion of mucus is converted into a copious effusion of purulent matter, and this too, where there is no ulceration of the membrane? And here we should consider the analogy which exists between a suppurating sore and the mucous membrane, when simple inflammatory action converts its natural products into common pus. The same fact may be affirmed of the serous membranes.(3) That the liver never secretes pus, except in its nutritive and circulating parenchyma, is highly probable; for here the secernent apparatus is complex, the laws specific; and the effusion of bile, or, in the kidneys, of urine, is a direct impediment to the formation of that kind and degree of inflammation in the secerning apparatus, that are necessary to the formation of pus. And yet, in conformity with our general law, "there is not one of the parenchymatous tissues in which pus has not been found." (4)

"The term secretion, "say our friends, "ought never to have been applied to the fluid which is called pus; for this term in its true signification is expressive of something which results from the operation of the nervous influence on the blood. Now it has already been shown that the secernent function of the nerves is put a stop to, in the very commencement of inflammation, of which pus is only one of the results." (5)

If we admit the latter proposition to be true, of which, how-

(1) Elements of Physiol. p. 475. (2) Op. cit.

(3) Thus Bichat: "Serum and pus very often flow at the same time from the serous membranes without any kind of erosion."— *General Anat.* vol. 2, p. 14.

(4) Andral's Patholog. Anat. vol. i. p. 302. (5) Mr. Earle, ut cit.

ever, we are hardly satisfied, except in cases where the nervous communication has been uninterrupted, we must certainly reject the former. It will then follow according to the foregoing reasoning, that since, as we shall show, the nervous influence is not necessary to the function of secretion in the natural state of a part, it is not necessary to it when the part is inflamed.

Now, we doubt not that the secretion of any complex organ, like the liver, or the kidneys, would be suspended or greatly diminished by cutting off the nervous influence. This influence is necessary to maintain a harmony of action amongst so many parts as are here concerned in the process of secretion; and it is but reasonable to suppose that when this concert of action is violently disturbed, we shall witness some remarkable change in the natural product. It has been also shown by Dr. Philip in more simple organs, as in the mucous membrane of the lungs and stomach, that the secretions of those organs are modified by a division of the par vagum, especially in the latter organ. But so far from the secretions being suspended in consequence, they are actually increased, and this constitutes the main peculiarity in the lungs. Nor can any appreciable change from the natural state of the mucous or the gastric juice be detected; whilst there may be, also, simultaneously induced a state of inflammation by dividing the nerve. "The lungs," says Dr. Philip, whose authority is so justly invoked in this matter by Mr. Earle and his school,

"Are found after death *distended* with a frothy fluid, which fills the bronchia and air cells, and prevents the lungs from collapsing;" and as to the stomach, "it deserves notice, that although the eighth pair of nerves have been divided, the food is found covered with apparently the same semi-fluid which we find covering the food in *a healthy stomach*." (1)

Here we are contemplating simple structures, though even less so than the apparatus of inflammatory action. We find the

(1) Experimental Inquiry, &c. c. 5, s. 1. — This is confirmed by Swan, (*a*) Reid, Bichat, (*b*) and others.

Mr. Reid says, "the congested state of the blood-vessels of the lungs and the effusion of frothy serum in the air cells and bronchial tubes, may be considered as the characteristic and only constant appearance after death from the section of the pneumogastrics." "When the bladder is paralytic, the mucous juices pour into it as before, oftentimes more copiously." (*c*)

It appears, too, that there is no reason to doubt that this increased secretion is owing to a state of muco-pulmonic inflammation, induced by a division of both pneumogastrics. It is stated by Reid that animals do not appear to suffer any immediate

(*a*) On the Connection between the Action of the Heart and Arteries and Nervous System.
(*b*) General Anat. vol. iii, p, 222. (*c*) Bichat's Gen. Anat. vol. iii. p. 195.

secretions at least not diminished by removing the nervous influence; and the only known change that takes place in those of the stomach is inferred from the food being more or less undigested. The whole matter, therefore, is resolved into the simple fact, that the nervous influence is necessary, and only necessary, to impart to secreted fluids some unknown specific property that may be designed for some great final cause. This abstract fact has, of course, no relation to the ultimate effects which the privation of nervous influence may exert upon organic functions. Sooner or later, the independent properties of a part may suffer from its abstraction, as they may be suddenly and perniciously affected by impressions propagated through the nerves. (Vol. I. pp. 474 — 480.) A difference, also, in results will probably be determined by tying or dividing a nerve. But, that an organ may subsequently recover its perfect functions, is shown by the complete restoration of digestion after the abolition of the nervous influence, by exciting the organic properties of the stomach by some stimulant, as by galvanism, in Philip's experiments, (Vol. I. p. 109.) In dividing the renal nerve, as in Brachet's experiments, blood is sometimes effused by the kidneys, to which the urine again succeeds. (1)

It being thus shown that the nervous influence is not neces-

bad consequences from a division of the pneumogastric nerves. (*a*) But, when inflammation ensues, the increased product implies, in itself, as in other cases, the existence of an active state of the blood-vessels, which may, therefore, exist independently of the nerves. We think, however, that few physiologists will agree with Reid that digestion is not impaired by a division of the nervi vagi. That the dogs were nourished for a definite time by animal food is not remarkable; since vitality still remained in the stomach, and, therefore, this organ was doubtless capable of exercising some vitalizing influence. As to the test which was proposed by Owen and Bird, — whether albumen was discovered in the thoracic duct, — we think it would prove nothing, since we have shown that chemistry is inadequate to the explanation.

The mistake is constantly made of supposing that Dr. Philip had demonstrated the dependence of the secretions upon the "nervous influence;" and, what is more remarkable, it is as constantly made by himself. Bichat appears to have settled the facts.

(1) "It may, we think," says a philosophical observer, "be stated as a general fact, that even when sensation and volition have ceased their manifestations, and nervous energy is totally extinct, each one of the organic functions may continue as long as vital properties remain in the tissues which perform it, and the necessary means are supplied." — *British and Foreign Med. Rev. vol. v. p.* 104.

The nervous system appears to be no more necessary than the heart to complex ani-

(*a*) Edin. Med. and Surg. Journ. 1837; and Trans. of British Association, 1838. Breschet, and Leuret, and Lassaigne had rendered the same probable. — Recherches pour servir à l'Hist. de la Digestion, p. 133.

sary to the function of secretion, in its general sense, the same process, *a fortiori*, may go on in inflammation and result in the formation of a new product. By the foregoing facts and reasoning, also, if the nervous influence be modified in inflammation, it is rendered more probable that a change should take place in the natural secretions of parts when inflamed; that instead of serum we should have lymph; and purulent matter instead of lymph and mucus.

It is also said "that whilst the utility of every part of the secernent function is obvious, no one has been able to offer even a probable conjecture as to the use of pus." (1)

Here the implied analogy is obviously defective; since the final cause of healthy secretion must be clearly different from the morbid. As well, too, might we say, that vitiated bile, or diabetic urine, are not secretions, because their uses may be unknown. But if any final cause appertain to the secretion of pus, we shall, no doubt, find it bearing a strong relation to the curative means of inflammation. (2) And, accordingly, we find that it is actually instituted by nature for the removal of that mode of disease; as is most distinctly evinced in all suppurating phlegmasiæ. Who does not know that inflammation of the breast, of biles, &c. is greatly diminished as soon as suppuration has taken place? Sympathetic fever declines; all the vital func-

mals whilst they are nourished in utero. (*a*) It appears, therefore, especially designed for the uses of the body when it begins an independent existence; and even then it is not improbable that its functions, in respect to *organic* life, are designed more to establish relations among the various organs of the system, when independent life begins, than to subserve their special uses. Even, therefore, if the nervous system were not also immediately subservient to the organic processes, its interruption from the nature of the imputed office, must necessarily inflict a direct violence upon the actions of a part, and indirectly by impairing the sympathetic relations. In this inquiry it should be considered that the fœtus has scarcely more than one function to perform, that of simple nutrition. (Vol. i. pp. 157 — 160, 568 — 572.)

(1) Mr. Earle, Op. cit.

(2) Mr. Earle says, "it is merely the inevitable consequence of the original stagnation of blood." (*Op. cit. p.* 190.) Perhaps it may be so, though it might be difficult to show that it is. But may not the same be affirmed of every cause and effect? It does not at all alter the question as to the modus operandi.

(*a*) See Philos. Trans. vol. lxxxiii. p. 154, 1793. In this case by Dr. Clarke, the whole nervous system, and heart, are said to have been wanting. We may safely conclude of the former, that the deficiency was nearly equivalent to an absence of the nerves. True, it has been said, that where a nerve is wanting, so is its appropriate organ; but facts must be here multiplied to render them the ground of a general law. Even then, however, it would not invalidate our view of the nervous functions. See, also, Brachet's collections of acephalous monsters, wanting the spinal marrow, in Rech. Sur le Système Ganglionaire.

tions more or less improve. Just so, too, with dropsical effusions that follow inflammation. So entirely is the vascularity often overcome by the effusion, that physicians frequently declare that such cases have had no dependence on inflammation; and this, perhaps, in the very face of the most characteristic symptoms and the depleting treatment.

That such is the final cause of suppuration is rendered farther obvious by the subsequent beginning of the restorative process, or the process of granulation; which, indeed, never commences upon an exposed surface till a full suppuration has nearly removed the inflammation.

It appears, therefore, that only a low degree of inflammation, or a different action growing out of inflammation, is necessary to the formation of pus, or a morbid secretion of serum; and this may be maintained as well by the influence of habit as by other causes. Even the new granulations take on the suppurating action through the influence of sympathy.

M. Donné considers pus a chemical product resulting from the action of an acid upon albuminous matter. With others he estimates the globules at twice the size of those of the blood, and considers them of a different shape and structure. (1) Hodgkin says "they bear no resemblance to those of the blood." (2) Sir E. Home, (3) and Mr. Liston, (4) maintain that the globules are formed after the matter is secreted. M. Gendrin makes pus to depend, in the most obstructed state of the vessels, upon a mere physical process, not yet well known; being then wholly independent of any vital influence of the inflamed part. In less embarrassed states of the vessels, he considers it a true secretion, and often speaks of it as "une véritable sécrétion morbide." At other times he thinks that extravasated blood may be equally changed into pus. (5) This is nearly the opinion of Mr. Key, who believes, also, it may sometimes depend upon the "breaking up a tissue, and its gradual conversion into pus," and that secreted lymph sometimes changes spontaneously into that substance. (6) Dr. Hughes thinks it "probably dependent on degen-

(1) In Archiv. Générales, Août. 1836.

(2) Anatomical Catalogue of Guy's Hospital, Obs. on sec. ii. part I.

(3) Wilson on the Blood, &c. p. 291. (4) Elements of Surgery, p. 30.

(5) Hist. Anatom. des Inflam. t. ii. s. 1461, 1463, 1443. Yet he thinks it similar to the nucleus of the red globules.

(6) Med. Chir. Trans. vol. xix. p. 142, &c.

eration of fibrin," when occurring in fibrinous concretions. (1) And thus Dr. Craigie:

"But it also results from the phenomena of suppuration in the filamentous tissue, and in the substance of the glands, that portions, more or less extensive, may be converted into purulent matter by a species of destruction or dissolution of their tissue," &c. (2)

But, has there been any attempt to show that this "destruction of the tissues" is not wholly the result of an ulcerative process, or that the exception is not contradicted by analogy, and by the absolute opposition which exists between the forces of life and of physics? Dr. C. indeed, makes all other lesions, even "softening," "induration," and "dry gangrene," to depend upon inflammatory action. (3)

So, too, Dr. Abercrombie: "In the former stage of tubercle, it is probable that the part remains susceptible of active inflammation, and healthy suppuration. In the latter stages, it appears to pass only into that peculiar state of softening, which arises from the decomposition of the tubercular matter." (4) This is a common doctrine in respect to the softening of tubercle, the fallacy of which we shall endeavour to indicate in another place.

Dr. Carswell thinks that pus may be sometimes the result of vital action, and again of a spontaneous change of "extra-vascular blood."

But, he has "never met with an anomalous formation of pus, without finding, at the same time, inflammation and suppuration to a greater or less extent in some remote organ." (5)

Professor Alison supposes that the "transformation of blood into pus may go on either in the interior of vessels, or in the exudations that have taken place from them." But we have "no satisfactory information," he adds, "how it comes to pass, that, in some cases, it begins almost from the beginning of the inflammation, and in other cases it hardly takes place in any stage of it, however protracted." (6)

Here the vitalist can supply the necessary light. De Haen, (7) Andral, (8) and others, believe, also, that pus may form itself altogether in the blood. The latter philosopher, and other physiologists, suppose that pus may be generated in the interior of a clot of blood, or in polypiform concretions. This phenomenon

(1) Guy's Hospital Reports, April, 1839.
(2) Prac. of Physic, p. 400.
(3) Ibid. p. 396 — 402.
(4) Edin. Med. Chir. Trans. vol. i. p. 686.
(5) Illustrations of the Elementary Forms of Disease, fas. 8, pp. 5, 8.
(6) Outlines of Pathology, &c. p. 439.
(7) Method. Medendi. De Gen. Puris, t. i. p. 60.
(8) Clinique Méd. t. iv. p. 683; and Patholog. Anat.

has been well attested by De Haen, Rostan, Reynaud, Andral, Laennec, Hache, Flandrin, Gendrin, Legroux, Bright, and others. We have therefore endeavoured to show, in our Venous Congestion, Sec. 9, and to sustain our views of life in other aspects, that all these substances have been organized through the medium of their attachment to the parietes of vessels, &c.; and that when they had been found isolated, they have been detached by different causes. (1) Andral goes so far with these new views in physiology, as to suppose "that if pus exists in other parts of the body, it has been transferred to coagula by absorption"! (2)

Sir E. Home believes that pus is so highly endowed with vitality, that "it becomes vascular like coagulated blood, and forms the granulations." (3)

Dr. Young agrees with Mr. Earle and others, that "there can be little doubt, that the globules found in pus are the identical

(1) We suppose the concretions to be either generated by inflammatory action in the vascular parietes, or that a recent clot of blood, by exciting that action, becomes attached in consequence. In the venous system, there is great reason to think that they are the result of the former process; and the transformations, which take place in those instances, reflect some light, by analogy, upon the suppurating concretions, and the whole, conjointly, contribute no little force in establishing an universal law in respect to the generation of pus. In all the cases of suppurating aneurismal concretions by M. Gendrin, he found the walls of the sac the seat of inflammation. (*a*) Corvisart found a concretion through a considerable extent of an enlarged subclavian artery and its branches, which was perfectly consolidated with the coats of the inflamed vessels. (*b*) It is stated also by Gendrin, that whenever suppuration is artificially produced in sanguineous concretions, it only takes place when the coagulum is recent, and a high degree of inflammation has been previously established in the vessel or cellular tissue which are the seat of the experiment. Can any mysterious influence be exerted by the *inflammation*, per se? The problem is easily resolved upon two principles; 1st, there may be an union between the inflamed parietes and the clot; or 2d, the parietes suppurating, the clot is dissolved by the pus, which is capable of this action.

The reasoning which identifies the vital and physical changes in relation to several products, is partly this. It is assumed that coagula of blood undergo, spontaneously, a conversion into pus. Nothing is easier, afterwards, than to apply the law of analogy, which we admit to be of great power in the science of life. Thence we read, as a conclusion of the argument, that "there is, then, the closest analogy in the formation of phlebolites, and that of pus and scrofulous matter within coagula, and also the change of coagula in veins into various *morbid* structures." (*c*) But, how does analogy operate when we come to the same formations as depending upon the vital actions of originally organized parts?

(2) Dr. Hughes, ut cit.

(3) Philos. Trans. 1818, p. 194.

(*a*) Hist. Anatom. &c. t. ii. p. 26.

(*b*) Journ. de Méd. t. xx. p. 210.

(*c*) Crouneian Lectures for 1835, lec. 2.

globules of the blood."(1) And thus, Mr. Bird: "Pus very clearly resembles blood; like that fluid, containing large quantities of globules of albuminous matter in diffusion, as well as in containing a serum, or aqueous solution of albumen." "It has also a large quantity of iron."(2) He thinks it nearly identical with fibrin.(3)

From what we have now seen, it is not remarkable that Dr. Hake should say, "it is not known whether pus is a degradation of the blood globule or a new formation." But allowing that it is a "degradation," still it is a new formation, since it is no longer blood, and the alteration has been effected through vital actions. (See Vol. I. pp. 56, 675 — 677, *notes*, and Essay on *Digestion*.) Dr. Hake's observations, which have been "extensive," "do not afford sufficient data on which to decide the method of its formation."(4) The secret may be found in a former remark of this observer, that "in the depths of physiology subsists an invisible world."(5) And yet Dr. Hake uses the microscope, and in deliberating upon the question whether "pus is formed from the blood," he remarks that "there is this difficulty, that they (the globules) are greater in diameter than *many* of the vessels composing the plexus before described." There are *some* vessels, then, whose diameter the microscope excepts in favour of the globules. He says, also, that the "globules of pus are from one-third to twice the size of the blood globule." We have, also, from Dr. Hake what we had anticipated, "An Account of a New Form of the Pus Globule." (See Vol. I. p. 699, &c.) But, what is most essential, Dr. H. finds that they are generated in the vessels, or before exposure to the air. This is important as showing organic agency in their formation, and some consistency

(1) Introduction to Med. Literature, p. 574.

(2) Guy's Hospital Reports, No. 6, 1838, p. 45.

(3) As to the spontaneous change of fibrin into pus, there appear to be different claimants for this new law in physiology. Thus, Dr. Hughes: —

"Justice, however, demands that I should state, that Dr. Addison has for some years strongly insisted upon this softening of fibrin giving rise to the puriform fluid found in the veins of persons affected with phlegmasia dolens; and that Dr. G. Barrows, in his Croonian (Crouncian) Lectures, published in the 15th vol. of the Med. Gazette, mentions some experiments which he had made, and which he had adduced in support of his opinions." (a)

(4) Treatise on Varicose Capillaries, &c. With an Account of a New Form of the Pus Globule, p. 11, 1839.

(5) Hake on the Organic Corpuscle.

(a) On Fibrinous Concretions in the Heart, in Guy's Hospital Reports, April, 1839.

in nature, as well as the fallacy of the prevailing doctrine that the capillaries have no orifices. (1) (Vol. I. pp. 685 — 689, 703.) Nevertheless, without feeling disposed to be more skeptical than facts may appear to warrant, we may be permitted to have some doubt about these purulent globules. Observers equally good, with good glasses and better eyes, only discover them after the secreted fluid has been exposed to the air; others find them before such exposure can have happened, generated within the vessels, or out of decaying tissues, whilst the latest observer comes forward with "an account of a new form of the pus globule."(2) Added to this, we are justified in referring to the extensive deception which grew out of the promulgation of Monro's universal doctrine of "convoluted fibres." (Vol. I. p. 700.)

Nothing can be more incompatible than these conflicting views with the common analogies of nature, or supply a stronger evidence that pus is a substance *sui generis*, and the propriety of looking to the vital forces for an explanation of its origin. If "the laws of physiology begin where those of the physical sciences end," it cannot be that pus is sometimes the result of a vital process, sometimes formed by the degeneration of solid parts, or again a putrescent or other chemical change of extravasated or intravascular blood. It seems to us that pus must be regarded as a uniform result of vital action, or that we must consent to annihilate all distinction between living and dead matter. (Vol. I. p. 627.)

"Since healthy physiology," says Bichat, "has been studied with method, a love of truth, and a desire only to collect facts, we have no longer been presented with those extraordinary cases in which nature seems to depart from the laws which she has imposed upon herself." (3)

But, how is it with the laws themselves? Or, has there not been some departure from that "methodical study of healthy physiology, and that love of truth," which Bichat so eminently illustrated. (Vol. I. p. 626, *note*.)

Since, however, chemistry has so greatly invaded the science of physiology, let us appeal for a moment to the light which has been reflected upon the composition of pus; in which we may find a farther proof of its specific nature. The best observers, as we have seen of Mr. Bird, find it closely allied to the blood; and as far as chemistry goes, its composition appears to be nearly uniform under all circumstances.

(1) Treatise, &c. ut supra. (2) See, also, vol. i. p. 686, *note* 3.
(3) General Anat. &c. vol. i. p. 341.

"It differs," says Mr Bird, "but slightly in its characters when obtained from secreting surfaces in different parts of the body; that, for example, expectorated in the last stages of phthisical *disorganization*, or during the natural cure of an empyema, differs from that obtained from an abscess only in the occasional presence of bronchial mucus." (1)

"There is certainly no obvious difference," says M. Gendrin, "between the pus of chancres, and that of simple wounds."

Taking, therefore, the authority of chemistry, as to the uniformity of the composition of pus, we need not say that this identity of character refers its production exclusively either to vital or physical agencies.

But, it appears that certain reagents establish an unknown difference between pus and other animal products. The globules of pus are distinguished from all other animal fluids, with the exception of homogeneous blood, in being changed by sal ammoniac into a tenacious jelly. But this substance dissolves the globules of blood, whilst it has no such action on the globules of pus. (2) Ammonia, according to Rayer, converts pus into a peculiar, ropy, slimy matter. When pus is combined with blood, coagulation takes place in the usual manner; but the coagulum soon after becomes fluid. (3) If a heated glass rod, according to Mauld, be plunged into pure blood, the rod will be covered by a red elastic membrane of fibrin; but, on the contrary, if a very small quantity of pus be mixed with the blood, no such formation will take place. If a little more pus be added, the blood will not form into a clot. (4) The globules of pus are said, also, to be twice the size of those of the blood.

Pus precipitates in cold water, and, according to Gendrin and others, the globules are not affected. (5) Many regard it as similar to lymph; but lymph coagulates spontaneously, and pus does not, whilst lymph is dissolved by sal ammoniac, besides other distinctions. Others consider it more like serum, or mucus. Meckel says it is "so like mucus that we cannot distinguish them by our reagents." (6) But pus is peculiarly affected by ammonia and sal ammoniac, and mucus, unlike pus, according to Gendrin and Bird, is coagulated by nitric and acetic acids;

(1) Ut cit. p. 44. (2) M. Donné in Archiv. Gén. Août. 1836.
(3) Ibid. According to Sir E. Home, and others, pus retards the putrefaction of meat. (4) Revûe Médicale, March, 1837.
(5) Hist. Anatom. &c. t. ii. §1472.
(6) Gen. and Patholog. Anat. vol. i. p. 334.

whilst, as stated by Bird, (1) and others, pus is dissolved by nitric, and hydro-chloric acids. Pus also evaporates to dryness without coagulating; and if dissolved with mucus or serum in sulphuric acid, the pus is precipitated by the addition of water. The latter, according to Beaumes, forms a red solution in sulphuric acid, which soon becomes black, and a little water precipitates it in the form of a grayish white substance; (2) whilst, according to Bird, (3) a solution of mucus in sulphuric acid becomes "quite transparent by the addition of water." Pus is not coagulated like mucus by oxymuriate of mercury, and it is precipitated by water from a solution in potass; though in potass we have a substance which is capable, in some degree, of denoting the modifications which pus undergoes, since it sometimes changes it to a viscid substance, which Thomson compares to the mucus secreted by the bladder. (4) When pus has subsided in water, if the vessel be shaken, the mixture presents a uniform turbid whiteness.

The effect of reagents, therefore, shows us that pus, although variable, is a substance *sui generis*, whilst its peculiar constitution defies all chemical research. The reason is obvious; physical powers have had no agency in its formation. But, being derived from the blood, and all parts alike capable of its generation under a *common modification of action*, when we come to its direct analysis, one has identified it with the red globules, another with fibrin, another with serum, and another with mucus; just as chemistry identifies starch, gum arabic, liquorice, sugar, and vinegar; or the various preparations of "artificial" and natural chyme; and, according to Davy, (5) even the putrefaction of muscle, or the product of certain cancers, and the albumen of eggs. Here we must admit that the elements and constituents are combined in peculiar ways by the agency of the vital powers, though we know nothing of the manner. (P. 122.) The farther, indeed, the chemist pushes his investigations, the more he multiplies proofs that the whole subject of organic products belongs, essentially, to another department of philosophy. (See Vol. I. pp. 56, 57, 62, 67, &c. and *Digestion*.)

Although purulent matter possesses a generic character, it has been always observed to present modifications according to vary-

(1) In Guy's Hospital Reports, No. 6, p. 45.
(2) Trait. de la Pthisie Pulmon. t. i. (3) Ut cit.
(4) On Inflam. (5) On Organic Chemistry, p. 417.

ing conditions of inflammatory action: and this coincidence we hold to be sufficiently indicative of its dependence on vital agencies. Hippocrates says of it, "pus vera optimum est album, æquale et læve, et quam minimum gravcolens; huic autem contrarium pessimum." Aretæus says there is no end to the varieties of pus. He states fourteen distinctions, and says that it is absurd to test such a varied secretion experimentally.(1) The most enlightened moderns, even such as defend the chemical and humoral doctrines, admit that "there are differences in pus which elude our means of investigation." This is the opinion of Gendrin, (2) and Louis; (3) and Andral has drawn a forcible picture of its varieties as determined by numerous modifications of the vital forces.

Chemistry professes, however, to have done something towards illustrating the differences in purulent matter. Thus, the proportion of albumen, according to Carswell, is much greater when the product of the serous, than of the mucous, membranes; and Gendrin found that in caries, it abounds with phosphate and muriate of lime; in scrofula, with a large proportion of soda and muriate of soda; and in gout, with an excess of carbonate and phosphate, and a little cerate, of lime. (4) Every other distinct tissue has been said to determine some analogous modification; all of which goes with our other proof of the vital origin of pus. Again, one kind of pus turns litmus paper red, whilst another will turn it back again.

The moment we recur to the vital relations of pus, we might go on to encumber our pages with their manifestations. We might say that one kind of pus is innoxious, and always so when the result of what is called *healthy* inflammation, (Vol. I. p. 523,) another produces smallpox, another cow-pox, another chancre, another gonorrhœa, &c. and each of these results will always follow respectively, when generated by either form of

(1) "Ut species puris prope infinitæ sunt. Quicunque vero aut igne, aut aqua humiditates explorant ac notant, hi haud ita multum phthoen mihi dignoscere videntur." — *De Diut. Affect. l.* 1, *c.* 8.

(2) Cependant il présente d'autres différences, qu'il n'est pas donné de reconnoitre par nos moyens physiques d'investigation." — *Hist. Anatom. des Inflam. t. ii.* §1473.

Again, "On conçoit qu'il peut y avoir des modifications nombreuses dans la facilité avec laquelle il est produit, puisqu'il tire évidemment son origine du sang qui peut être altéré de bien des manières, et qu'il est subordonné à l'existence d'une phlegmasie dans la partie où il se forme." — (§1463) He states, also, many facts that lead to this conclusion, as seen in caries, scrofula, gout, &c.

(3) On Phthisis, sec. 225 — 230. (4) Op. cit. §1471.

disease. The pus of smallpox injected into the veins of an animal proves rapidly fatal, whilst "laudable pus," is inoffensive. (Vol. I. p. 523.)

It may be said with as much propriety that the never-varying matter of smallpox and cow-pox is a mere exudation from the blood, a breaking down of the tissues, or the result of some slight chemical change, as to make the affirmation of common pus, or other morbid productions. And, although it be hypothetically urged that the blood in smallpox may communicate the disease, this is certainly not true of cow-pox, or of chancre. We have stated a remarkable illustration of the peculiar characteristics of the smallpox and cow-pox virus in our first volume, (p. 535, *note*,) where, also, we have shown the fallacy of the humoral rationale. The late experiments in Britain, showing the affinity of both, through near approximations produced by vital actions, are a striking commentary upon the whole physical doctrine.

Is it not, then, the same with pus as with the almost infinite variety of other specific products that distinguish the animal and vegetable kingdoms?

Having now seen that pus is a product of vital action, and that all the results of inflammation are opposed to the hypothesis of vascular debility, stagnation of blood, and chemical agencies, the conclusion seems to us unavoidable, that every stage of the disease is conducted by the powers of life. That these, and the consequent actions, are more or less exalted, and otherwise modified, appears not only from the various facts already stated, but from the analogy which is supplied by all healthy and morbid secretions. In no other way can we explain the simple fact that pus and serum are often generated with great rapidity, and in vast abundance; whilst the plainest physiological results indicate the whole philosophy. If, for instance, the glandular organs be stimulated, and the action of their capillary system increased, the bile, saliva, urine, &c. are poured out in redundant quantities; and again the same results will follow in morbid irritations of those organs, and even when the subjects of inflammation.

So far as chemistry has been connected with the physical theories of inflammation, its indications are universally opposed not only to the mechanical, but to the chemical doctrines. This is as true in respect to the analyses of serum, lymph, and mucus,

as of pus.(') These are either essentially specific products, or specific modifications of the natural secretions; whilst every change may be said at all times to correspond with certain vital

(1) Thus, of serum: the products of the peritoneum, pleura, the lining of the pericardium, the arachnoid, synovial bursæ and capsules, serous cysts, &c. offer varieties peculiar to each. (*a*) It is upon these distinctions that Hodgkin founds his truly philosophical doctrine of analogies betwixt the serous and mucous structures.

Again, these varieties are always modified by disease, the alterations corresponding with the vital signs. (*b*) Bichat speaks of them especially in relation to peritonitis. Baudelocque says, that in puerp. peritonitis, "the serous effusion is so irritating, that when we make several successive dissections, the hands will become covered with large pimples, which inflame and suppurate. It is very difficult to get rid of them." (*c*) The physical theorists assumed that the change was extravascular; but it is too uniform and peculiar to the foregoing affection. And how is it with the excoriating nasal mucus in some varieties of catarrh? "There is reason to believe," says Dr. Craigie, "that the fluid discharged from the transparent membranes during inflammation, or as an effect of this process, is *always specifically* and *chemically different* from that which is found in a healthy state." (*d*) Still our able author goes for the mechanical theory. There is a common assent as to the peculiar nature of the hydrocephalic fluid. We have seen, too, that serum passes by insensible degrees into pus.

As to secreted lymph, there is a general concurrence of opinion, that it partakes of the nature of the tissue from which it is effused; or as Hunter has it, "the blood changes into this or that kind of substance according to the stimulus of the surrounding parts." (*e*) A striking illustration of this principle occurs in the reproduction of the crystalline lens, as described by Soemering in the human subject, and determined experimentally on animals by Cottreau, Leroy d'Etioles, and Mayer. (*f*)

Great varieties are observed in mucus, especially in inflammation, and according with the degree or other modification of that disease, and the situation of the membrane. Analyses afford different results, and the sensible differences are more remarkable. In the latter instance, the coincidence with the modified action is very obvious. It is seen in the varied secretion from the schniderian membrane. "In the first, or most ordinary form of bronchitis," says Dr. Stokes, "we have a mucus, and afterwards a *muco-purulent* secretion; in the second, a secretion bearing the character of *lymph*, as in some of the forms of croup; in the third, the secretion is principally *serous*, as in the different forms of humid catarrh and asthma; whilst in the fourth, there is little or no secretion, a disease which has received the name of the dry catarrh." (*g*) And so distinguished humoralists, when studying the phenomena of life,—as thus Andral: "A diseased mucous membrane presents as many varieties in the composition of its secretion as there are *different degrees or modes of irritation* in the membrane that furnishes it." (*h*) And so Müller. (*i*) Here, too,

(*a*) See Marcet in Med. Chir. Trans. Lon. vol. ii. p. 373: A tabular view in Cyclopædia of Prac. Med. art. Dropsy; Carswell on the Fluid Products of Inflamed Tissues, in Illustrations, &c. Fas. 12: Babington's Analysis of the Hydrocephalic Fluid in Conquest's Cases, in Lon. Med. Gaz. March, 1838. Gendrin Hist. Anatom. des Inflam. t. i. p. 213, and t. ii. p. 492—523; Bichat's Patholog. Anat. vol. ii. p. 88: Davies on Diseases of the Lungs and Heart, p. 326: Laennec, t. ii: Hodgkin's Lectures on the Morb. Anat. of Serous and Mucous Membranes, *passim*.

(*b*) See citations, ut supra. (*c*) Puerperal Peritonitis, p. 210. (*d*) Practice of Physic, p. 293.

(*e*) On the Blood, &c. p. 86. (*f*) Archiv. Générales, Jan. 1833. (*g*) Diseases of the Chest, p. 45.

(*h*) Patholog. Anat. vol. i. p. 297. (*i*) Physiology, vol. i. p. 472.

phenomena, not only as it respects the immediate seat of the effusion, but often in a constitutional sense.

If the changes depend on chemical principles, other products should be found; inflammatory action should not be necessary to a particular result, nor should there be a uniformity in the particular modification which attends a secretion from a particular organ, as in inflammations of the mucous membranes, and at a particular stage of inflammation, yet varying, though still uniform, at other stages; nor should there always be a distinctive character to the products of the several modes of inflammation; nor should each organ manifest, in these respects, its own peculiarities, as evinced by the secretions, and the coincident nature, in each part, of its own granulations. Chemistry can do no more than fulfil its legitimate office. The secretions being derived from the blood, should present the elements, and, perhaps, also, though variously combined and modified, the constituents of that fluid. But even the variable proportions of the natural constituents, as they may be determined by disease, can only be explained by the vital laws.

The liquor sanguinis only separates into its component parts, after the laws of vitality cease to operate. (1) From which and the foregoing facts, and many others which appear in our several Essays, we infer that when lymph, serum, and all other products from the homogeneous, living blood, take place, it can only be through the agency of the vital powers. If, also, we allow vitality to the blood, it is but reasonable to conclude, that its va-

Andral agrees with Bichat, that "all these varieties are especially owing to sympathetic influences." (*a*)

The expectoration in simple mucous inflammation of the lungs is very different from that of peripneumonia; and the latter is so unique that Laennec says "it may by itself enable us to recognise the disease." (*b*) Dr. Forbes regards it as an important diagnostic; (*c*) as do, also, Dr. Stokes, (*d*) Williams (*e*) Andral, and others. Mr. Brett, who made an elaborate analysis of different kinds of mucus, found that pneumonic expectoration differed from most other kinds of expectorated matter. Phthisical expectoration was very various, but depended upon the stage of the disease, and the manner in which the actions became modified. (*f*)

Finally, in their natural state, "the mucous fluids are not the same in any two places. They vary in the principles that constitute them, as the membranes which furnish them vary in their structure." (*g*) The mucus of some parts is dissolved by potass, whilst it is rendered gelatinous from others.

(1) Med. Chirurg. Trans. vol. xvi. p. 293.

(*a*) Bichat's Gen. Anat. vol iii. p 282. (*b*) Diseases of the Chest, p 220. (*c*) Ibid. (*d*) Ut cit. p. 321. (*e*) Lectures on Diseases of the Chest, lec. 10. (*f*) Trans. of British Association, 1837. (*g*) Bichat's General Anatomy, vol. iii. pp. 89, 115.

rious component, as well as elementary parts, will be held together by the powers of vital affinity till that power ceases to operate; or, till it is overcome by the greater and united powers of the secerning vessels. And although the spontaneous separation of the component parts of blood, imply a decline or an extinction of the vital forces, yet no such result may appertain to coagulable lymph when eliminated by the living solids; but the process being a vital one, the vessels of secretion may even endow the lymph with greater vitality.

It is an inference, also, not easily resisted, that since the various secretions of the body, in their natural state, depend upon the vital powers, they are equally the product of those powers, when redundant in quantity, or when modified in quality. If physical laws were essentially concerned in the process, many of the facts which we have mentioned could have no existence. If chemical forces mainly operate, then should the products be widely different from the constituents of the blood. These forces are not concerned in diversifying the proportions of those constituents; but in separating their elements and forming new compounds. (1) Such apparently sometimes happens, as seen in the presence of the hydrocyanic acid and saccharine matter in the urine. These form stumbling-blocks in our way, from being rare and almost solitary evidences of the possible independent operation of chemical forces in the animal system; its most extraordinary characteristic consisting in its abstraction from the laws of chemistry. Yet have we endeavoured to show, in our Humoral Pathology, that what is thus apparent has no real existence, but goes to our general conclusions as to the philosophy of life.

The natural philosopher relies upon uniformity and universality in the fundamental operations of nature, and toils at apparent exceptions till they are reconciled to her general laws. (Pp. 128, 133, Vol. I. p. 626, *note.*) But not so with the mechanical inquirer when the phenomena of organized beings are the subject of investigation. "We find writers," says Dr. Abercrombie, "talking of general rules in medicine with exceptions to

(1) So, also, "the great difference between the affinity of chemistry, and the force of vitality is this; that, whereas the former in all the changes that it produces, acts by substituting one element for another, the latter impresses changes without replacing the elements which may be abstracted by its operation." (a) This was Hunter's doctrine; (p. 122.)

(a) Prof. Draper in Amer. Journ. of Med. Sciences, vol. xxi. p. 124.

these rules."(1) Do we not find them "talking" of fundamental exceptions to great laws in physiology? Do they not ascribe the same phenomena, and the same specific results equally to vital, chemical, and mechanical agencies, — to the whole collectively, or individually? Or, if vitalism have the ascendency, do not eminent philosophers, like Hall, and Louis, ascribe all the phenomena, and the lesions of inflammation to totally "opposite modes of action?" But, however the effort be made to show that nature is not always consistent, the devious course, like the pursuit of alchymy, will ultimately conduct us to the truth.

The doctrine of exceptions is the great bane of all philosophy. It supposes nature to operate by partial, not by universal laws, defeats the generalization of facts, and the practical advantages naturally arising from them. After the Hippocratic philosophy was lost, and before its restoration by Bacon, the ratiocination of physical and medical philosophers was alike. A multiplicity of causes was considered necessary to explain any of the phenomena of nature.

The elements of inorganic, and especially of dead organic, matter, are constantly combining spontaneously into specific compounds, again separating, and again uniting to form other combinations. Every part of the animal or the vegetable kingdom is alike composed of those elements, though, as is said, not in definite proportions. And when the principle of vital affinity gives way, the being that had for years resisted the action of chemical powers may be speedily broken up, and the elements may form a multitude of new combinations; but they require for their reconstruction the agency of organized matter. For this purpose, nature has contrived a most elaborate system, in which she has introduced forces having an ascendency over the inorganic, as if to magnify the grandeur of her operations. Nor is it an improbable conclusion, that "in the planets we distinguish, and in those that we cannot, different forms and laws of life prevail." (2) We may trace the plan with increasing conviction, without a solitary hope for the materialist, from the infusoria up to man. And yet, with all the complex organization of animals, and all its superior endowments with the vital forces, and we

(1) On Diseases of the Abdominal Viscera, Preface.

(2) Med. Chir. Rev. Lon. vol. xv. p. 114. — See Voltaire's very graphic speculations upon this subject, in Œuvres, t. xiv. p. 99 — 101. 1771.

may add, with all the agency of the chemical powers, the elements of what was once organized matter cannot yet be combined till they have felt the creative power of the vegetable world.

These laws are the foundation of an elegant distinction betwixt the vegetable and animal kingdoms; the former subsisting upon inorganic, whilst the latter live upon organic matter; (1) thus manifesting a general final cause of vegetable life, in its supply of nourishment to the higher kingdom; whilst a more specific is seen in the ultimate tendency of the whole process of vegetation to result in means for perpetuating its own species. If the combinations which compose organic matter are apparently few, and of a specific nature, it only shows the absence of the chemical forces. It is the license of these forces, however precise they may be, to operate in the most unrestrained manner, and with only a fortuitous result. But in all living organized matter, the perfection of design is so manifest in its structure, in its functions and products, that we irresistibly admit that it must be withdrawn from the capricious influence of chemical or physical powers.

Had it been consistent with proper brevity, we could have wished to have illustrated the whole subject by analogies drawn from animals that are destitute of a heart, (2) and from the vegetable kingdom. Their healthy and morbid processes, their healthy and morbid products; the infinite varieties that occur in these respects in the latter kingdom, according to the organization and endowments of each species of which it is composed, and yet these varieties always the same; the very limited number of elements that make up the endless variety of the specific products, as well as the organized parts, and many other corresponding facts, reflect a flood of light upon our subject. Here circulation is carried on by the vessels alone, and by them are all the products determined. Nor is there anything in physics or chemistry that will explain a single result, in its proper connections. Like animals, all plants are subject to disease, and wounds heal in the latter as in the former. We engraft one plant upon another, just as we transplant one part of the most

(1) See Mirbel, Trait. d'Anat. et de Physiol. Vegetables, t. i. p. 19; and Smith's Introduction to Physiolog. Botany, p. 24.

(2) See, particularly, Haller's Element. Physiol. t. iv. s. 4, § 29 — 51, where this subject is examined.

perfect animal upon another. None can doubt the analogy of the processes, of the laws and actions upon which these common results depend. The vegetable kingdom is full of these parallels; and so are all animals whose circulation is only vascular. The next step brings us to the human fœtus which grows to maturity without a heart. It is in vegetables that we see the greatest simplification of the laws and functions of organic life. They are divested of those secondary influences which so constantly embarrass our inquiries in more complex organization; and whilst they are, therefore, better subjects for experimental research, the facts they supply, which are full of analogies, may be safely carried up as analogies to the most complex beings. Here, too, we have an endless variety in the component parts of organization, yet invariably the same in every species. As in animals, growth, reparation and secretion, are the results of a secreting process, depending on various modifications of action.(1) And, as root, wood, bark, &c. with all their established varieties, are secreted from the sap in a fluid state, so are bone, muscles, tendon, nails, hair, &c. with all their determinate modifications, derived in a fluid state from the blood. (Vol. I. pp. 68, 594, 626.) Thus, every part is endowed with a secreting apparatus, and, in its normal state, with precise and unvarying modifications of common powers. In proportion, too, as the organization of parts approximate each other, so will the nature of their secretions; and hence we find greater analogies among the products of the serous, mucous, and cellular tissues, than in more complex organization. Do you speak of abnormal urine, abnormal milk, bile, &c. we refer you to what we have said on this subject in our Humoral Pathology. There is nothing analogous to the foregoing in the whole range of inanimate matter; nor can these considerations fail of their natural connection with the subject we have been considering. Our present knowledge is a fulfilment of the prophecy, —

"Neque ideo Naturæ divitias exhauserimus, cum in fabrica intima plurima

(1) It is the doctrine of many philosophers, M. Andral for instance, that "in general, the elements are separated from the blood in certain organs, the peculiar structure of which favours that separation; and that after the separation, they are so united and combined in their respective organs, as to form the different secretions." And yet our author, and numerous followers, maintain, at other times, the simple mechanical exudation of some of the very products which are alleged to depend in ordinary cases upon a process of decomposition and recombination under the control of vital powers.

latere queant, quæ æternum nos sint fugitura." (1) "Nec plures, sed unus est modus procedendi, et differentia quæ intercidit inter corpora oritur ex diversis illorum formis, propter quas dissimilia sunt corpora in operationibus, *quamvis unica sit omnium causa*;" et "certum siquidem est, quod in operibus suis analogice Natura procedit." (2)

Powerful attempts are now making to introduce a new philosophy into medicine and physiology, or to revive an exploded one. Whilst the unique process of inflammation and its results are to be expounded on mechanical principles, a coagulum of blood, long extravasated and long dead, is to be revived and organized, and carry on an isolated life by its own independent resources; and this, too, in the midst of venous blood. By this it is to be nourished, whilst it is known to be fatal when circulating in the nutritive system of other parts. The highest degree of vitality, a mysteriously vivifying influence, far exceeding the *spiritus archæus*, is thus given to the blood, when it is yet disputed whether it possess any vitality at all. Coagula of blood are, also, supposed to undergo every variety of spontaneous change that attend the actions of living organized matter; and which have hitherto been referred to the agency of the vital forces. They are said to undergo spontaneous conversion into pus, and by those who believe that pus is at other times a secretion; to change into calcareous matter arranged in concentric layers, although there may have been originally no lime in the clot; into carcinomatous, melanotic, lardaceous, hæmatodic, mammary, and scrofulous degenerations, &c. The glandular structures are to be mere strainers of the blood, and the secretions, the matters strained. And to finish the climax, the humoral pathology is to simplify disease in such a manner, that according to a truly eminent physician,

"Passing by this organ and that, and this function and that secretion, we penetrate the spring and source of all, even to the blood itself, and there find the seminal principle of disease; and that this, in hitherto most fatal of all fevers, may be remediable by the simplest means, which are always at hand;" (3) this simple means being the muriate of soda.

In what we have now said of the results of chemical tests, we have justly employed the means with which chemistry has supplied us for its own defeat; nor do we find that it has in any re-

(1) Haller, Element. Physiol. t. iii. p. 423.
(2) Baglivi, de Naturæ Analogismo.
(3) Dr. Latham on "Subjects connected with Clinical Medicine," p. 53.

spect invalidated the conclusions of the soundest vitalist which were expressed when organic chemistry began its career. We have often demonstrated this fact; and in relation to our present inquiry we may quote Mr. Hunter, who has much upon the subject:

"To ascertain," he says, "the properties of pus, or to distinguish it from mucus, it has, with mucus, been put to the test of chemistry." "Whatever my opinion might be, yet *bold assertions, the result of described experiments*, made me avoid falling into the *same error of describing what I had never seen.* I made, therefore, some experiments on this subject; and, in consequence of having previously formed the above-mentioned opinion, I was more general in my experiments. I made them on organic animal matter, as well as inorganic, and the result was the same in all. I took muscle, tendon, cartilage, gland, viz. liver, and brain. Also pus, and the white of an egg. I dissolved each in sulphuric acid, and then precipitated the solution with vegetable alkali. Each precipitation I examined with such *magnifiers* as plainly showed the forms of the precipitate; all of which appeared to be flaky substances. The precipitate by the volatile alkali appeared to be exactly the same." And so of solutions in "vegetable caustic alkali precipitated by muriatic acid, and examined by the *microscope*, the appearance was the same," and so on. (') (P. 114 — 122.)

We do not, however, require the aid of chemistry to contradistinguish purulent from all other formations, and to establish its exclusive dependence upon vital actions. Upon this question no chemico-mechanico-physiologist will controvert the expositions which we have quoted from M. Andral in our first volume, p. 627.

It was our purpose to have stated, at page 120, in connection with Fourcroy's, and Carpenter's views of the prospective career of organic chemistry, the coincident expectations of a leading European Review, that nothing might be suppressed on our part which may be adverse to our own conclusions:

"Our attention has been chiefly turned, our acuteness principally directed, to the *consequences* and the *symbols* of disease, of the *most obvious* character, to symptoms and to structural changes which the *unassisted* sense of vision could perceive. How gross those must be, how general, how removed from the simpler and more elementary features of morbid action, need not, we conceive, be pointed out. The *microscope* is to be applied to the solids and fluids in disease, and to distinguish, as no doubt it will, the primary deviations from normal mechanical constitution, which disease must of necessity exhibit, and in which it may often, perchance, consist. And *chemistry* is to come with her wonderful analysis, *outrunning* the microscope far more than the microscope *outruns the eye.* When we contemplate the field of research before us, we cannot think ourselves sanguine in anticipating a fertile crop of discovery in

(1) On the Blood, &c. pp. 430, 431.

the future." (1) (See Vol. I. pp. 529, 676, 699, &c. and Essay on *Digestion.*)

We cannot but think, however, as we have variously endeavoured to show, that the habits of observation which are denoted in the first of the sentences which we have taken the liberty of marking in the foregoing quotation, must continue to form our sole dependence.

From what we have now stated, it results that there can be no such condition as *passive* inflammation, as usually contradistinguished from *active*. Those who have made this distinction, have founded it mainly upon the effects of opposite remedies, and, it appears to us, without a due consideration of the principles which determine their effects. The difference consists only in different modifications of the vital forces, the extremes of which, in relation to remedial agents, are connected together by various intermediate gradations. The remedies must therefore be adapted to the precise changes in the vital properties, which constitute the true pathology of disease. We shall resume this subject in our *Venous Congestion*, Sec. 15.

Having introduced the subject of homœopathy in a note, at page 179, we would say that we had great hesitation whether we should bestow upon it any farther comment than we had transiently made in our first volume, at page 673. But this work being especially designed as a guide to students in their search after truth, it has occurred to us that we neglected speaking sufficiently of the fundamental error of that doctrine, and of the distinguishing principles which direct the medical philosopher; since these are the first and most important that are concerned in the treatment of every individual case of disease.

The original import of "*similia* similibus *curantur*," were there any meaning in the expression, was that similar diseases are cured by similar remedies. The only intelligible phraseology is *similes morbi similibus remediis curantur*. The homœopathist, however, having his peculiar latinity, we shall no longer differ with him upon the structure of language, but proceed to state the absence of all pathological considerations in his application of the dogma as he understands it, and the important practical errors which must constantly ensue. He prescribes only

(1) Lon. Medico-Chirurg. Review, Oct. 1839, p. 349.

for a particular symptom. Let us illustrate the fact by the example which we quoted from Hippocrates. *Vomiting* is the *disease* in the philosophy of homœopathy, and the dogma is the cure. Tartar emetic is therefore to be given (1) under all circumstances of the remote and pathological causes. If cerebral symptoms spring up, these must be equally met, each one, by some agent that will produce the symptoms, respectively, in health. And yet the latest advocates of homœopathy claim John Hunter as a disciple, (2) just as we have seen of the humoralists; (Vol. I. p. 634.) Whatever, therefore, may be the profession of homœopathy as to its respect for pathology, in a practical sense it must reject it, *in toto;* whilst its ostensible regard for the science, (which is the only basis of practical medicine,) is equally in conflict with its peculiar dogma. The moment it admits the absolute condition of disease as the ground of practice, it abandons *the symptom*, and identifies itself with "allopathy."

There is a branch of the homœopathic school which strenuously defends the dogma, yet allows that many diseases must be treated "allopathically." But, we may say, that there can be no such thing as homœopathy contradistinguished from "allopathy," and yet cöoperating together. The construction necessarily involves an absurdity; since "allopathy" embraces as much the peculiar doctrine of homœopathy as the opposite principle.

The following are the precepts which have prevailed in all medical works of consideration since the day they were promulgated by Hippocrates: "*Contraria morborum contrariorum sunt medicamina.*" These are *a* cure, not *the* cure, for contrary diseases. Again, continuing the language of Hippocrates, (p. 179, *note,*) "nam si quis homini vomenti, aquam multam bibendam dare velit, eluentur ea, propter quæ vomit una cum vomitu. *Et sic quidem per vomitum vomitus sedatur.*" In the same paragraph, therefore, Hippocrates, after saying that vomiting may be relieved by direct sedation, or upon the principle of "contraria," &c. draws the important practical conclusion, "*et ambobus contrariis modis homo sanis fit;*" each being wholly determined by the nature of the causes, and not by the mere act of vomiting.

However, therefore, the man of science is constantly concerned

(1) Ticknor's "Letter, &c. for Believing the Fundamental Principles of Homœopathy," p. 8. 1840. Hahnemann, and others.

(2) Ticknor's "Letter," p. 30.—Channing's Discourse on the Reformation of Medical Science, &c. p. 42. 1839.

about the foregoing principles, he can never know which may be applicable to any individual case of vomiting, or of other disease, till the exact pathology of the case is understood ; and whoever prescribes upon any other principle does it at random. Each of these principles, indeed, which are held by the homœopath to be contradictory, must be often applied in simultaneous connection, many examples of which occur, especially in our first volume, and where Mr. Hunter is introduced in his true aspects. (Pp. 222, 242—245, *Humoral Pathology*, *&c.*)

Finally, the great doctrine *similes morbi similibus remediis curantur*, is of universal application in medical science, however it be liable to various modifications according to the varieties which may be determined in similar diseases by their remote causes, &c.

We conclude, therefore, that the professions of homœopathy as to the "inductive philosophy of Bacon" have no foundation, but that, like the "numerical system," it excludes all principles and reposes alone on the contingency of the greatest amount of particular results, as they may fluctuate one day after another, and compounded of the "experience" of such as "blow in Chili," up to the philosopher who selects his facts and exalts them to a science. (Vol. I. p. 400.)

APPENDIX TO ESSAY ON THE THEORIES OF INFLAMMATION.

STATE OF THE CIRCULATION IN FEVER.

(See pp. 145 *note*, 181.)

It is only our purpose in this Appendix to consider the doctrine of "stagnation of blood" in its relation to idiopathic fever. Of course, we shall not go over any part of the ground we have just travelled, but simply regard the special foundation of the foregoing hypothesis.

Dr. Craigie remarks in his valuable work on the Practice of Physic, that,

"The blood in ague and fever generally moves more slowly than natural through the capillaries. I am aware that this may appear paradoxical to those who have been accustomed to hear and to speak of the *accelerated circulation*, and *increased impulse* of blood in fevers. These expressions, however, I am prepared to show, are adopted with fallacious views of the circulation."

Our author also states, in various places, that there is a "partial stagnation" of blood in the capillaries in remittent and continued fevers, and "in all forms of fever" during the stage of excitement, — though we must say, not upon the authority of any facts, nor is it shown by any reasoning. In the plague, "there is a sudden, almost immediate retardation of the motion of the blood in the capillaries of all the organs, in all cases, and in the most intense and virulent, stagnation more or less complete of the blood." It is affirmation, we mean it respectfully, without proof.

Exactly the same objection is made by our author to what is called "increased local determination of blood;" and, as in the foregoing case of fever, all have hitherto "agreed in representing the blood to be propelled with increased force and velocity," and all have been alike mistaken. It is said that there is here, as in the hot stage of fever, the same capillary stagnation of blood. All the various proofs to the contrary that have been offered are "fallacious." (1) We shall, therefore, take an entirely new ground of demonstration; as we have, also in our attempt to exhibit the error of the corresponding doctrine in regard to inflammation. We may also premise, that in noticing this new hypothesis, we have far more in view the great objects which we have contemplated throughout our work, than any disposition to quarrel with the hypothesis itself. It will be seen, too, that it has a special bearing upon our next Essay. The question is, also, of great magnitude, in a practical sense, and philosophy, as well as humanity, is deeply interested in its right construction. Our author

(1) Practice of Physic, vol. i. pp. 95, 96, 246, 277, 278, 279, 281, 287, 329, 364, 355.— 1836.

is justly one of the leading guides of the day; and we would not assume that he is wrong on a great physiological point, without endeavouring to prove him so.

The foregoing doctrine, however, is not wholly original with Dr. Craigie. We find something like it laid down by Marshal, in his work on Hydrophobia. (1) But our author considers it opposed to universal belief, and that "it may appear paradoxical." &c. We frankly admit that we are placed in the supposed dilemma.

This is not a matter of speculative philosophy. In the stage of reaction, then, we find the heart beating with preternatural force, as admitted by our author. 2nd. If the temporal artery be divided, the blood is projected with an increased momentum. 3d. If we puncture, or apply leeches to the skin, the blood flows with a profusion unknown in health, as is also admitted by our author; and this it often continues to do with unyielding obstinacy. The blood, therefore, which ultimately escapes, is not the blood which was originally contained in the capillaries; but blood that is constantly circulating in them, and, of course, according to our facts, with increased velocity. 4th. If we look at the veins, we find in their turgid condition a proof that the blood is not only freely transmitted from the capillaries, but with an accelerated motion. This is shown by the projection of a stream on opening a vein.

But, says our author, less and less blood will be expelled by the heart in a given time, in proportion to the frequency of its pulsations; that "much more blood passes through the heart when it contracts 60 times in a minute, than when it contracts 70 times, and more when it contracts 70 than when it contracts 90 times, or 100, or 120 times. It must also follow, that the blood moves more slowly when the heart contracts with unnatural, than when it contracts with normal frequency." (2)

Here the important fact is overlooked, that the dilatation of the cavities of the heart, and the force with which they contract, are constantly variable, under the same conditions of frequency. Thus, in one case, as in the sinking paroxysms of phthisis, or organic affections of the heart, this organ shall beat 120 pulsations, and the circulation may appear almost at a stand. The skin becomes cold and bloodless. Place your ear on the region of the heart, and you may scarcely hear its action. But, on the contrary, in the hot stages of fever, when you have the same frequency of pulse, there is a florid, ardent state of the skin, and you may see the bed shaken by the commotion of the heart. Is not the force of the heart greatly augmented by violent exercise, a paroxysm of anger, &c. whilst the frequency of the pulsations is as greatly increased? And who will deny, that under these circumstances, the velocity of the circulation is increased, and that the blood performs its entire circuit with increased rapidity? Or can it be entertained that there is a stagnation of blood? But the cases are clearly parallel, as they concern the circulation, with the reacting stage of fever. Again, on the other hand, a paroxysm of fear, like that of anger, shall accelerate the action of the heart, whilst the vigour of the organ shall be depressed as in the cold stage of fever. The philosophy of these different physiological conditions we have endeavoured to expound in our Essay on Bloodletting. It appears to us that they have no connection whatever with mechanical principles, excepting that in the latter case, the action of

(1) P. 106—122, &c. (2) Practice of Physic, vol. i. p 95.

the heart is, in part, embarrassed by the central accumulation of blood, and by the vital constriction of the capillaries. It is apparent, therefore, that nothing can be predicated of the force with which the heart may dilate or contract, or of the rapidity of the blood in its circuit, by an abstract regard to the frequency of the pulsations. It is all relative; and our conclusions must be founded on the facts in each individual case. Nor is there any thing opposed to these considerations by the philosophy of physics or mathematics. It appears to us an error upon any principle, to suppose that "more blood passes through the heart when it contracts 60 times, than when it contracts 120 times." More may be admitted to the heart at each dilatation in the former than in the latter case; but if the velocity of the circulation be twice as great in the latter instance as in the former, the same quantity of blood may have "passed through the heart" in a given time in both cases. But as we have seen, and may be shown upon the mechanical principles of our author, less blood actually "passes through the heart," in a given time, during the cold stage of fever, when the pulsations may be only 60, than in the hot stage, when they may amount to 120. In the former case, the heart is clogged and embarrassed by the accumulation about its cavities; and the capillaries being also contracted, the quantity of blood which passes through the heart is greatly lessened; a large proportion remaining almost at a stand in the great internal vessels. But in the reacting stage of fever, these obstacles are entirely overcome; and if the pulsations now amount to 120, it appears to be manifest that the heart having less blood to move at each pulsation, and with augmented power, will drive it forward with an increased force. Besides this mechanical view of the subject, the change in the state of the vital forces adds greatly to the momentum of the blood. Irritability and contractility are everywhere increased, not only in the heart, but throughout the vascular system; and the capillaries, so far from retarding the motion of the blood as in the case where there were but 60 pulsations, now coöperate with the heart in advancing the motion of the blood.

But our author, in weaving his hypothesis, has borrowed the web of another, which we, for one, are in no respect disposed to admit. We allude to the doctrine, long since nearly settled against him, that capillary circulation depends upon the vis a tergo.

Returning to the respective stages of fever, we find the pulsations generally less in the cold than in the hot stage. According, therefore, to our author's hypothesis, more blood should be sent out by the heart, and the circulation should be more active in the former, or cold stage, than in the latter. If this view of the subject be correct, we must have a modification of the existing theory of fever, as it has been universally received. Taking the premises of our author, we think we have sufficiently demonstrated, that, having too much reliance upon "facts," we have been deceived as to the absolute nature of the different stages of fever; and that the *cold* stage should hereafter be considered the *hot*, whilst we correct our prejudice in a corresponding manner as to the hot stage. It is clear that "these expressions have been adopted from fallacious views of the circulation," however "paradoxical it may seem to those who have been accustomed to hear and to speak of increased action" as the hot stage. The prevailing mistake is also farther shown by the superior effica-

cy, as it is said, of bleeding in what we have hitherto considered the cold stage of fever. Few have doubted, till of late, that this remedy is best adapted to the stage of excitement, or "increased action." And since first impressions are said to be generally the best, it follows, from the greater success of bloodletting in the cold stage, that this must be the true stage of "increased impulse" or "increased action." (Vol. I. p. 205 — 210.)

It is due to our author to say, that his argument is not wholly speculative; but that he places a part of the burthen upon that capital resource for demonstrating the *actions of life*, — morbid anatomy. After saying, as we think rather mechanically, that "the weight and heaviness of the head, the oppressive languor of all the muscles, and particularly in the back, the inability to retain the body or limbs in the erect position, and the natural desire for the horizontal posture, depend, I conceive, *entirely* on *the load of blood* with which the capillaries of the *whole body*, and *all its organs*, are oppressed," we have the following contribution from the debris of the various organs : —

"The foregoing," says our author, "cannot be directly demonstrated by anatomical inspection, because it *almost never* happens that individuals are cut off on the *first* day or *days* of fever; (!) and when the fatal event takes place, we recognise only the *effects* of the morbid action, and not the *exact* nature of the *process itself*. But it admits of as satisfactory evidence as any question of this kind, by comparing the appearances found in different patients cut off at different stages, and, *throwing aside those phenomena* which are manifestly to be regarded as *mere effects*, selecting those only, which, by their constancy, appear to be entitled to the character of *causes*." (1)

We are certainly not disposed to deny, that particular organs will appear more or less plethoric from inflammations that generally supervene in the progress of idiopathic fever; but even in these cases, we think that we have shown that the circulation could not have been retarded.

This, however, is not the greatest concession that we have to make in behalf of morbid anatomy; for we have no sort of doubt, that in fevers, and, we are willing to say, in all other diseases, the blood is more or less apt to stagnate in all parts of the vascular system when death is taking place, and especially so after that event has come. It is also not to be questioned, that, for an uncertain time antecedently to death, the skin may be hot, whilst the action of the heart shall be languid, and beating with augmented or diminished frequency; that, also, the capillaries, from the declining powers of life, notwithstanding the increased temperature of certain parts, shall have partially lost their power over their contents; and that, therefore, the blood will move in them with diminished velocity, and may undergo an accumulation. Examples of this nature were common in the cholera asphyxia; where the skin at the umbilical region stood at a temperature of 106, or more degrees, after all pulsation had ceased for some time in the radial artery. (P. 29 — 35.)

Before leaving our subject, we cannot forbear expressing, more distinctly, what we have already implied, the nature of the causes which so variously affect the movements of the circulatory organs, and especially the capillary blood-vessels, not only in fevers, but in all other diseases. These, it appears to us, are mainly the different modifications of the vital forces; and according as

(1) Practice of Physic, vol. i. p. 97.

they may be exalted or depressed, or otherwise changed, so will be the forcible dilatation and contraction of the heart, without regard to their frequency, and so will be the action of the capillaries. In fever, without looking at the remote causes, or regarding any hypothesis, we see that action is universally languid during the cold stage, and it is equally evident that it is exalted in the hot. However the vital forces may be affected in other respects, there is abundant proof that these powers are depressed in the former, and preternaturally excited in the hot stage, whilst there are as evidently, and necessarily, corresponding degrees of action.

In other affections, however, the remote causes shall have modified the vital forces in a different way, and according to these modifications will be the state of the circulation in the capillary blood-vessels, without the least connection with the *frequency*, and perhaps but little with the *force*, of the heart's action; though in the latter case there is certainly, in most instances, a coincidence betwixt vigorous action of the heart and that of the capillaries. This we have just seen to be true in the natural state of the organs, as in a paroxysm of anger, or when the circulation is excited and accelerated by violent exercise. This correspondence betwixt the action of the heart and that of the capillaries, betwixt the quantity of blood projected and its velocity in the capillaries, appears to be almost fundamental in the animal economy. The final cause of this harmonious concert is abundantly obvious. Still it cannot be regarded as an absolute law. For in the first place it does not depend upon the intrinsic nature of the vital forces; and, again, it is liable to those disturbances from the influence of remote causes which more greatly embarrass the relations betwixt parts whose mutual dependence is less perfect. Nevertheless, the harmony is better preserved than among other parts; and whether the impression of modifying agents be directly upon the heart or the capillaries, the influence of sympathy is extended to the other, by which a concerted action is maintained. (1) This we have seen to be remarkably manifest in bloodletting, which is a simple standard by which we may approximate the philosophy.

In connection with the foregoing subject, and as having a relation to our inquiry into the philosophy of life, we cannot avoid noticing, that our author has founded upon the mechanical doctrine of capillary "stagnation" of blood in the reacting stage of fever what appears to us a mistaken conclusion in regard to the causes which stimulate the heart to greater and more frequent action. "The stagnation," he says, "affords a mechanical obstacle to the farther transmission of blood from the heart. This is the cause of the heavy labouring action of the ventricles in fever." "The chambers of the heart are never completely emptied, and consequently, before the last stimulus has ceased to act, a new one is applied; or, rather, to speak more to the matter of fact, the stimulus of the blood never ceases to operate. This is the cause of the frequent ventricular contractions." (2)

If such, then, be the real cause, should not the same phenomena predominate

(1) There are some instances, especially where the nervous system is unusually concerned in the morbid process, in which the action of the heart greatly exceeds the apparent action of the arteries; (vol. i. p. 356, &c.) and this is a proof to us, when considered in relation to our foregoing remarks, that the capillary vessels have an independent action. We think that this conclusion results from our several essays, respectively.

(2) Ibid. p. 276.

most in the cold stage of fever, since here there is a real obstruction in the contracted capillaries, and the accumulation of blood within the cavities of the heart is then undoubtedly greater? But is it ascertained that "the cavities of the heart are never completely emptied," in the hot stage of fever, or as much so, at least, as when excited by exercise, anger, &c.?

It appears to us that a variety of facts concur in showing that the increased labour and frequency of the heart's pulsations in the hot stage of fever are mainly owing to vital changes, whilst the foregoing considerations show that they are not connected with the supposed mechanical condition of the capillaries, or any peculiar relation of the blood to the heart. In the cold stage, however, these causes have really an important effect upon the action of the heart, and it is sometimes a primary object to remove the constriction of the vessels and relieve the heart of its load. But even here, much of the difficulty attending the heart consists in the alteration of the vital powers, which, however, are destined to undergo an important change in the subsequent stage of the paroxysm.

Again, when the heart is prostrated in simple venous congestion, not only from a morbid impression upon its vital forces, but from a preternatural determination of blood upon its cavities, if we abstract blood freely, and thus modify the vires vitæ, and take off the contraction of the capillaries by which the mechanical obstacle was constituted, we shall at once increase the vigour and perhaps the frequency of the heart's action. (Vol. I. pp. 129 — 134, 197 — 209, 224 — 232.) But, will it be contended that the result is now owing to the cause assigned by our author in an analogous case? Carry the bloodletting still farther, and take half the blood from the body, we may thus possibly still go on increasing the force and frequency of action. We do it, however, by increasing the irritability, directly and indirectly, of the heart, — not by accumulating any blood in its cavities. (Vol. I. pp. 239 — 248, 261, 267.)

We have been thus candid with our author, since he unequivocally affirms that we are all in the wrong upon the foregoing subjects, without producing a fact to show us so. And since we have undertaken to defend a ground that has not been assailed by any weapons, we cannot forego the disposition to say, that in all our author's discussions of the great questions relating to fever, &c. we hear little or nothing of the vital forces, but everything is construed upon mechanical principles, — everything referred to the fulness and obstruction in the capillaries, just as we have seen of the prevailing theories of inflammation.

Dr. Craigie's work, however, is full of rich erudition, and, as it appears to us, of practical remarks that contradict his hypothesis of fever, and which can only be founded on a close observation of nature. It is a learned, and valuable contribution to medicine, especially in its relation to the remote causes, the symptoms, and the treatment of diseases. But, whilst minds like our author's may disregard, (as we have seen of the most illustrious of the humoral school,) their own hypothetical views in pathology, there are others who may take his word and carry his suppositious philosophy into the chambers of the sick. — See APPENDIX I. of *Venous Congestion.*

PHILOSOPHY OF VENOUS CONGESTION.

"Res obscuræ et sensibus immanifestæ ad possibile redigi possint. — ARISTOTLE *Metaphys. c.* 4.

"Physiology begins where the physical sciences end." — MAGENDIE.

"In organic bodies, the spirit of the theories should be wholly different from the spirit of the theories applied to the physical sciences." — BICHAT'S *General Anatomy applied to Physiology and Medicine, vol. i. art.* 1, *s.* 9.

"It must be allowed just to join abstract reasonings with the observation of facts; and to argue from such facts as are known to others like them." — BUTLER'S *Analogy of Religion, &c. Introduction.*

"Analogical reasoning, founded upon the uniformity of nature, is frequently employed in the investigation of facts; and we infer, that facts of which we are uncertain, must resemble those of the same kind that are known. The bulk of the reasonings in natural philosophy are of this kind." — KAIMS, *on the Progress of the Sciences, b.* 3.

"Induction and analogy are the guides by which a man of penetrative genius is conducted into the hidden walks of Nature, and much farther than the *senses* will enable him to do. The whole art of physic depends on this mode of reasoning." — ZIMMERMANN, *on Experience in Physic, vol. ii. p.* 41.

"In investiganda alicujus morbi natura, causa, aut curatione, per analogiam ratiocinari, ac procedere possimus ab aliquo morbo manifesto, apparente cum quo *occultus* similitudinem aliquam haberet. — BAGLIVI, *de Naturæ Analogismo.*

"There is no more mischievous or mistaken spirit in philosophy, than that which is always seeking, on the appearance of new phenomena, for new principles to explain them, rather than attempting to refer them to others before established, and which are known to give rise to more or less similar effects." — ARMSTRONG'S *Lectures on Acute and Chronic Diseases, vol. i. p.* 100.

"The same disease yields diversity of symptoms; which, howsoever they be diverse, intricate, and hard to be confined, I will adventure yet, in such a vast confusion and generality, to bring them into some order." — BURTON'S *Anatomy of Melancholy.*

[The substance of this article, embracing all its leading facts and arguments, was communicated simultaneously with that on Bloodletting, to Dr. James Johnson as announced in the Medico-Chirurgical Review of July, 1837. (1) We are not sensible that any variation of our opinions, or any new modes of inves-

(1) The following is the notice to which we refer. "33. In MS. to be presented to the Royal Medico-Chirurgical Society.

"On the Philosophy of Bloodletting and Venous Congestion, by Martyn Paine, M. D.

"The Society had just closed its sittings for the Session, when the above MS.

tigating our subjects, occur in either. This statement is due to ourselves for the purpose of showing that we have borrowed no opinions from any of the able observers to whom we shall refer, as entertaining certain coincident views, and whose writings have appeared since the foregoing period.]

SECTION I.

We have reserved for this place the important subject of Venous Congestion, that we might have all the advantage of any light that may be reflected by the examination which we have bestowed upon the natural and modified powers and actions of life in our preceding investigations.

We have hitherto seen that it is the prevailing disposition of the age to refer the phenomena of organized beings, whether in health or disease, to the agencies of chemical or mechanical powers; but most of all to the former. This error, as we have endeavoured to show, has its deep foundation in confounding the vital with the properties of dead matter, and in yielding the senses to the illusions of the microscope, or to mistaken analogies in chemical science. Mechanics has been largely introduced "to circulate the blood," and in some diseases, as in inflammation and its consequences, it is the principal fulcrum. But we now approach an affection in which we lose sight of chemistry, and all vital agencies, and surrender the science of life to an unadulterated mechanical philosophy. Here we have "nothing but stagnation," (p. 145, *note*,) and the modus operandi of all our remedies is construed upon physical principles.

Whilst every other tissue of the body has been, from the earliest records, the acknowledged seat of frequent inflammation, the remarkable fact exists, that it is only within a few years that the veins have been suspected of being liable to this mode of disease. (1)

"Nearly a century and a half had elapsed," says Dr. Lee, "from the time

was received. It will be presented in time for the ensuing Session." (p. 296.)

We know nothing farther of the disposition of the manuscript; but can well suppose that it was mislaid in the midst of Dr. Johnson's multifarious engagements.

(1) Venous inflammation was wholly neglected till described by Mr. Hunter, as a consequence of injuries, in the Trans. of a Society for the Improvement of Med. and Chir. Knowledge, vol. i. 1793. About the same time it was noticed by Schmuck, in *Diss. de Vasor. Sang. Inflam.* 1793; and by Sasse, *in Diss. de Vasor. Sang. Inflam.* 1797.

when phlegmasia dolens was first clearly pointed out by Moriceau, until I ascertained by dissection the true nature of the complaint." (1)

Meckel, however, in 1797, described the symptoms and morbid alterations in uterine phlebitis, and attributed the fatality of the disease to the prostrating influence of venous inflammation. (2) Other observers have remarked, about the same time, a frequent dependence of puerperal fever upon inflamnation of the womb. Lieutaud describes a bad case of uterine phlebitis. The milky fluid, observed about the intestines and in the lungs, was evidently pus. (3) Kirkland says,

"It is evident that an inflammation of the uterus, and a consequent absorption of putrid matter from this part, will bring on what is now called puerperal fever; and that the inflammation of the abdomen, &c. is frequently the consequence of the fever thus brought on." "Whence it is evident, that we ought to make a *distinction* between the *fever* and the *disease*." (4) (Vol. I. p. 540.)

In 1816, M. Ribes made some investigation of the nature of uterine phlebitis; and ascertained, also, the existence of venous inflammation in erysipelas, and in some other instances. (5)

The inquiry has since been diligently pursued, but mainly in reference to uterine and traumatic phlebitis; from all which,

"Il en résulte que la phlébite regardée autrefois comme très rare, elle est aujourd'hui donnée comme assez fréquente." (6)

"*Necessitas medicinam invenit, experientia perfecit.*"

"If we permit ourselves," says Armstrong, "impartially to consider the vast importance of the whole venous system, we shall probably be led to conclude that its morbid states have by no means received sufficient attention." "Probably the anatomist may find in the peculiar structure of the venous apparatus of the head and the liver the cause why these organs should more often suffer in congestive fever than the rest." (7)

In looking into authors for their opinions upon Venous Congestion, we have remarked a very general silence as to its existence. Some, who have noticed it, confound it with the results of common inflammation; (8) whilst some one or two regard it as a *post-mortem* occurrence. (APPENDIX I.)

(1) Researches on Pathol. and Treat. of some of the most Important Diseases of Women, p. 149. 1833.

(2) Sasse, de Vasor. Sang. &c. (3) Anat. Med. t. i. p. 318.

(4) On Child-Bed Fevers, pp. 66, 49, 47. 1774.

(5) Mémoires de la Société Méd. D'Emulation, t. viii. p. 624.

(6) Dubois (D'Amiens) Pathologie Gén. t. ii. p. 322. (7) On Typhus Fever.

(8) Whenever we speak of *inflammation*, or *common inflammation*, it refers to that which affects other tissues than the venous. Whenever the term *congestion* may occur, we shall employ it as expressing a morbid fulness of the veins, arising from disease of their parietes; having no reference to the state of the arteries, and being wholly different from that fulness of the veins which is consequent upon a preternat-

Even "the congestive form of fever," says Dr. Armstrong, "is entirely unnoticed by all our systematic writers on medicine, for this obvious reason, that they follow Dr. Cullen to the letter." (1)

We have already seen, however, in our Essay on Bloodletting, that Dr. A. was mistaken; which may be attributed to the disinclination of this very original man to avail himself of the knowledge of others. We may also add, that the subject of venous congestion, in its simple conditions, and as exerting constitutional influences, was more generally investigated, and better understood by the physicians of the last century, than since morbid anatomy has had its unresisted empire. The three Kæmpfs, (2) Stahl, (3) Hoffmann, (4) Schmidt, (5) Koch, (6) Faber, (7) Elvert, (8) Juncker, (9) A. E. Buckner, (10) E. A. Nicolaus, (11) Bertholdi, (12) treated of venous congestion, and ascribed to it important consequences. They generally consider it the result of obstruction to the circulation. It is evident, too, that Hippocrates, (13) Aretæus, (14) and Alexander of Tralles, (15) were as well acquainted with the affection as the moderns. The two last treated the worst forms of it by bloodletting.

The condition of the veins, which we are considering, being

ural quantity of blood thrown upon them by the arteries under various influences. Nor do we recognise, in the least, as appertaining to this affection, that turgescence of the veins which is produced by obstructing causes. — (See vol. i. pp. 81, 204, *notes.*)

We shall employ the terms *acute* and *sub-acute*, in a relative sense, as it respects the degree of disease; and *active* will be synonymous with *acute*. We do not recognise *passive* conditions till certain degrees of disorganization have taken place, or where some mechanical cause may obstruct the circulation, and then only so far as it respects the *direct* effect of such causes. If we speak specifically of *chronic* inflammation or congestion, we shall prefix the epithet. *Sub-acute* inflammation, or *sub-inflammation*, may mean inflammation of short or long duration. *Congestion* always means *venous* congestion. *Inflammation*, per se, always inflammation of other tissues than the venous. *Phlebitis* is intended to imply what is recognised as venous inflammation.

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 187.

(2) De Infarctu Vasorum Ventriculi, 1751; and De Morbis ex Atrophia, 1756.— (3) Vena Portæ, Porta Malarum hyperchon. 1705. — (4) Op. Om. Physico-Med. t. i. s. 1, c. 8, de Sang. Circ. &c.; and De Rat. Therep. Med. s. 1, c. 4, § 40, &c. c. 5, § 38, 1748. — (5) De Concrement. Uteri, 1753. — (6) De Infarctibus Vasorum in Infimo Ventre, 1752.— (7) Ulterior Expositio novæ Method. Kæmpfianæ, 1755.— (8) De Infarctibus Venarum Abdominalium, 1754. — (9) De Congestionibus in Genere, 1718; and Diss. de Venæ Portæ, Porta Salutis, 1742. — (10) Fundamenta Patholog. Gen. ex Anatom. et physico Mechanicis Principiis, 1746. — (11) Bemuhungen in dem. Theoret. und Pract. &c. 1749. — (12) Diss. primæ Liniæ Morborum Venæ Portæ, 1777. — (13) L. de Flat. ver. 107 et seq. and in many places.— (14) De Acut. Morb. l. 2, c. 8; and de Cur. Acut. l. 2, c. 3 and 7. — (15) L. 12, c. 3.

almost universally recognised, and too palpable to be doubted, we shall reserve for our first Appendix an examination of the objections to its existence; and shall now endeavour to show, as briefly as possible, that its pathology is not, as generally supposed, of a mechanical nature; but that it is strictly vital, exerts extensive influences upon the whole system, and that the dilatation of the veins is often of an active kind. We may say, indeed, that the pathology of venous congestion has not been investigated, as we are aware of, by any writers; and forming a subject of the deepest interest, it has appeared to us to peculiarly invite discussion. If we have not misapprehended its nature, and founded our opinions on imaginary phenomena, or the results of fortuitous practice, we must regard this disease as the most prevalent, most insidious, and most fatal to the human race. Indeed there can be no doubt that "phlebitis," in its acknowledged form, "is infinitely more common than is frequently suspected." (1) Medical observers have agreed that there is a mysterious something attending congestive fevers, and many are convinced that the unknown cause of their malignancy resides especially in the venous system, under the disguise of "an obstruction to the circulation." Tissot says they "are more dangerous than they seem; like a dog who bites without barking;" (2) or as Cleghorn calls them, "fraudulent, deceitful fevers," with "insidious intervals," and "treacherous remissions." (3) M. Bailly in his Italian Fevers, notices very particularly the deceitful character of these fevers, and states a fact which has been observed of the yellow fever in America, that the patient may be walking about just previously to the accession of a paroxysm which is fatal in a few hours. M. Maillot speaks of the same insidious remissions in his African Fevers. In Texas, during a late epidemic of this character, "by common consent the malady was called the *cold plague*." (4) (See Sec. 12.) In our southern and western states where congestive fevers are rife, and their fatality great, the disease is known under a variety of expressive epithets, such as "*the battle-axe of death*."

Venous congestion, whether in its local forms, or connected with idiopathic fever, appears to be greatly owing to miasmatic

(1) Jackson's Principles of Medicine, and Animal Organism, p. 25. 1832.
(2) Avis au Peuple sur la Santé, c. 17, § 242.
(3) On the Epidemical Diseases of Minorca, c. 3, p. 103, and c. 6, p. 164 — 168.
(4) Boston Med. and Surg. Journ. Jan. 22, 1840, p. 390.

poison; and in these instances it manifests its worst modifications. It therefore prevails most in particular latitudes, and in certain places. In America, north of the latitude of New-Haven, (Connecticut,) it is comparatively unknown, except when occurring in connection with typhus fever; the true typhus being also limited, on this Continent, to the regions north of that line. Remittent and intermittent fevers begin in that latitude, (with the exception of a few localities,)(1) and are found over the whole continent south of it. (Sec. 12.) Local forms of venous congestion are generally rife in proportion to the prevalence of the latter fevers. Again, in some places where those fevers do not often *originate*, as in that part of the city of New-York which is covered with houses, local venous congestions sometimes occur epidemically, especially before adult age, and sporadic cases are to be seen at all times. And again, in places where intermittents and remittents prevail in autumn, local venous congestions may occur epidemically, and independently of the fever, in winter. It often happens, however, when the local conditions of disease are neglected, or badly treated, that an explosion of the constitutional disease will sooner or later take place. (Vol. I. pp. 471 — 474, 541 — 545.)

If venous congestion be complicated with idiopathic fever, it is commonly apparent after the general explosion takes place; though absence of pain, and perhaps of other striking symptoms, is apt to betray the unwary into a false security, or to beguile him into the fatal belief that debility is the worst attendant. This is especially true, if there have been, antecedently, a gradual access of the local affection, which often precedes, even for many weeks, the invasion of fever. This preliminary condition, or when the disease maintains, throughout, a local character, is often difficult of detection, in its early stages, to all but the man of sagacity and careful experience. We once said to an intelligent physician of the western prairies, who had shut his eyes against congestive disease, regarding it as a fanciful hypothesis,— "What did you, and others, denominate the local form of the complaint when death so often unexpectedly made his invasion?"

(1) See, particularly, Holmes' Prize Diss. on Intermittent Fever in New-England, 1836.

Dr. Craigie's statement that, "intermittents are abundant along the whole tract occupied by the States, as far north as the St. Lawrence," (a) is only applicable west of New-York.

(a) Practice of Physic, vol. i. p. 73.

"Oh," he replied, "we then called it *being suddenly struck with death;* or, the vital organs were *suddenly overwhelmed;* or, the circulation had *lost its balance.* At other times, we remarked a very altered state of the intestinal discharges; and this being the most remarkable symptom, we regarded it as the disease itself, and we then called it *a vitiated state of the secretions,* which only indicated cathartics for its removal, and tonics to sustain the strength?" "But what, Sir," we continued, "did you consider the disease, when inflammations, or other sympathetic derangements took place?" "Then," said he, "we had always some more tangible ground, and called it by the name of the last in the series of developments; and here we seemed to be sustained by morbid anatomy. Until I began to investigate the real condition of the congested organs, to note the often insidious local symptoms, and connect them with the equally obscure ones of a general nature, at the early stages, my practice was wholly fortuitous, and without a solitary principle, or a sound experimental fact, to direct it. The secretions were vitiated, and, like most others, I gave cathartics to get rid of the offending matter, which, as I have said, we also considered the disease itself. But it generally happened that the secretions grew worse in proportion to the extent and activity of our cathartics, till a general febrile action set in. By this time, prostration of strength had also become another prominent symptom, or perhaps earlier, and our efforts were now directed to the fever and debility. We changed our practice to the spirit of Mindererus in the evening, when the exacerbation was greatest, and to tonics and wine in the morning, when the excitement had abated. This often brought about a more violent excitement, and the more violent it was, the better for our patients, for then we would sometimes bleed and thus drive the enemy from his ambush." "And what, Sir, did you consider those cases, which were originally a 'vitiated state of the secretions,' when they reached their fatal termination?" "When death was not an early result, and constitutional sympathies had supervened, we pronounced them a remittent, or a bilious fever, but more generally considered them cases of typhus."

That this is in no respect a suppositious case will not be doubted; nor need we scarcely add that this able man now looks back with those sensibilities which are the surest evidence of conviction and a love of truth.

"Are these things, then, necessities?
"Then let us meet them like necessities."

Great discouragements are certainly opposed to our inquiry, and not the least, the manner in which the discoveries relating to phlebitis have been received by many of the profession.

"It would appear," says a reviewer, "from what we can gather in conversation, that many gentlemen regard the investigation of phlebitis and of secondary inflammation, as rather amusing than practical. They affect to smile at those who pursue it, as persons of a theoretical turn, who are diverted from studies of matters of daily interest, to the pursuit of novel and fanciful pathological researches. There cannot be a more palpable blunder than this; one which stamps those who commit it as ignorant and routine men, who are totally unacquainted with the nature of many cases they daily have occasion to treat." (1)

May not this neglect of the fundamental ground of therapeutics, account, in a manner, for the fatality of phlebitis, as well as of venous congestion? And if, as implied, by our philosophical reviewer, the pathology of phlebitis, which is a comparatively rare disease, be a subject of momentous importance, venous congestion is equally recommended by its dangerous tendencies, and has the advantage, in our favour, of being "a matter of daily interest."

In the prosecution of our subject, we shall again find ourselves obliged to become the reluctant critic on the opinions of others, and, adopting the advice of Cicero, we shall still endeavour "to draw forth truth from the collision of different views; or, at least, to approach to it by these means." But we need not say, that we disclaim every sentiment but that of respect for their genius and learning. We are sure, indeed, that it is a study of their writings which has led us to many conclusions which we have hitherto expressed, and which it is our present purpose to continue.

We shall endeavour to arrive at the results which we have proposed, by the scanty facts in morbid anatomy, by the phenomena of the disease, by its remote causes, by the effects of remedies, and by induction from collateral facts. We shall examine the subject analytically and synthetically, ascending from effects to their causes, and returning by way of causes to their effects. We shall lay a broad foundation of analogies, which, indeed, are the great basis of medical science; and, in this respect, we hope to exceed in our approximation to truth, the conclusions which would be justified by the lowest degrees of inductive philosophy,

(1) Lon. Med. Chir. Rev. vol. xxix.

and which we may as well express in the language of Butler in relation to a holier subject: —

"In questions of difficulty," he says, "or such as are thought to be so, where more satisfactory evidence cannot be had, or is not seen; if the result of examination be that there appears upon the whole, any the lowest presumption on one side, and none on the other, or a greater presumption on one side, though in the lowest degree greater, *this determines the question*, even in matters of speculation; and in matters of *practice*, will lay us under an *absolute and formal obligation*, in point of prudence and interest, to act upon the presumption or low probability, though it be *so low as to leave the mind in very great doubt which is the truth*." (1) (See APPENDIX II. on *Analogy*.)

The amount of information which may be obtained according to our method, surpasses any knowledge that morbid anatomy can supply, in a general sense, as to the pathology of disease. The facts which we investigate, depend equally upon determinate laws, are very various, and offer a wide field for connected inductions which terminate in one conclusion. It is very far, however, from our intention to carry out —

"The sublime hypothesis of the old philosophy, that by the circuit of deduction all truth out of truth may be deduced;" although "when we consider the wonderful connection and inter-dependence of all knowledge, made more and more manifest by every day's advance in science, it would seem almost to prove the hypothesis by an accumulation of particular examples." (2)

It will be, also, our early purpose to show that none of the existing theories are compatible with the facts which relate to venous congestion, or varix, and that their imputed causes have no real existence. The laws which govern venous circulation will also become a subject of consideration; so that, in our wide range of inquiry we shall embrace many distinct and independent topics.

We may also say, that by the term, venous congestion, we mean a strictly local disease, (p. 217 *note*,) upon which, however, constitutional sympathies, simulating fever, may attend; as well described by Hippocrates, who concludes:

"Hoc ergo modo, ut dixi, febres fiunt, et cum febribus dolores, et aliæ quædam ægritudines." (3)

We regard true fever as an independent disease, and in the sense in which it is considered by Fordyce, (4) Pinel, (5) R. Jackson, (6) Clarke, (7) Bichat, (8) Southwood Smith, (9) Morgan, (10)

(1) Butler's Analogy of Religion, &c. *Introduction.*

(2) Verplanck's Oration on the Advantages and Dangers of the American Scholar, p. 19. 1836.

(3) L. de Flatibus, v. 132, &c.

(4) Dissertations on Fever.

(5) Nosolog. Philosoph. t. i.

(6) On Febrile Diseases.

(7) On Fever.

(8) Gen. Anat. vol. iii. p. 234.

(9) On Fever.

(10) On Prin. Surg. p. 84.

Armstrong,(1) Craigie,(2) Senac,(3) Rush,(4) Sydenham, Hunter, &c.

The able Senac, after discoursing of the "malignant intermittent fever," and stating that "at the termination of a paroxysm, the terrible symptoms abate, and oftentimes *entirely disappear*," concludes from this and other circumstances, that

"A rule of no small moment may be here deduced in acute diseases. It appears, for instance, that there may be great disorder in the functions of the body without real inflammation or any fixed disease in the solid parts." "Yet those parts, which have experienced such deep and distressing affections may in a short time be entirely relieved."

Should the patient die at the invasion of the first paroxysm of fever, we may look in vain for any physical evidence of disease.

The terms fever, constitutional fever, and idiopathic fever, we use in a common sense.

We shall pursue our usual method of first stating the opinions of distinguished writers in their own language, and this we do the more so at present, to show that venous congestion is regarded as one of the common maladies of our race by the ablest philosophers.

"The venous, or arterial system," says Dr. Copland, "having lost the principal part of their tone or vital tension, react imperfectly upon the mass of blood injected into them by the heart's action, and become distended and congested." (5)

Dr. Barlow states, that "simple congestion evinces over distension of the vessels, and clearly implies debility of their coats." (6) Here we suppose the doctrine is intended to apply indiscriminately to venous and arterial plethora.

Dr. Hope thinks that "congestion of blood may result either from a passive accumulation connected with a sluggish vascular action, or from mechanical obstruction into the right side of the heart." (7)

Dr. Bright probably refers to both systems of blood-vessels, when he says: "Inflammation is a state of excessive action, while congestion is a state in which the vessels being unable to free themselves from the blood which they have received, become gorged and overloaded;" "they become distended beyond their power of contraction." (8)

"'Behold,' says Magendie, 'the character, and principal and general fact of the blue cholera. The ventricles of the heart being debilitated, there results

(1) On Typhus; and Lectures on Acute and Chronic Diseases.
(2) Practice of Physic. (3) De Recondita Feb. Natura et Curat.
(4) Medical Inquiries, &c. (5) Dic. Prac. Med. Art. *Congestion of Blood.*
(6) Cyclopædia Prac. Med. Art. ibid.
(7) Principles and Illustrations in Morbid Anatomy, p. 133.
(8) Medical Reports, vol. ii. pp. 652, 654.

cold, discolouration of the face; and as the feebleness of the contraction proceeds incessantly, the result is the very remarkable fact of the stagnation of the blood in the veins, and the blue colour of the skin.' — *Leçons sur le Cholera Morbus, p.* 13. In support of this hypothesis, M. M. brings forward the evidence of a similar colour produced by an experiment, where, by a mechanical impediment to the arterial circulation in the leg of a dog, he has found the stagnation of blood to occur in the veins." (1) (Vol. I. pp. 397, 510 — 519, 526, 539, 567, 650, 697 — 698.)

"Congestion," says Dr. Horner, "mest frequently is the result of mechanical impediment to the venous circulation." (2)

Dr. Baillie remarks that "the most *common diseased appearance* of the pia mater is that of *its veins being turgid with blood.* This depends upon some impediment to the free return of the blood from the head towards the heart, which may arise from a variety of causes, and is very different from an inflamed state of the pia mater. The smaller branches of its arteries, *filled with a florid blood, are not more numerous in this state than is natural,* but its *veins* are *much more distended* with dark blood." (3) *And so of all others.*

"The phenomenon next to be considered," says Professor Naumann, "is the morbid distension of the veins, which we observe at the seat of congestion. In these vessels circulation is not entirely suspended, though it becomes more and more inert from the arteries conveying to the organs an excess of blood, in relation to its depressed vitality. Since, then, the blood arrives in a very slow course through the intermediate capilliaries into the veins, these latter participate less and less in any impulse from the heart. The blood thus accumulates and distends those torpid, unresisting channels, through which its progression can only be affected by a mechanical repletion, or a species of overflowing so to term it. (4)

Dr. Macartney observes that the most remarkable circumstance with respect to venous congestion, and the one which has not hitherto been described, is, that arteries found in a congested part are *smaller* than their natural size, (5) and it is the frequent vacuity of the arteries in congested parts after death, as in other cases where common inflammation has not existed, that led Dr. Clutterbuck to the belief that venous congestion arises from that contraction of the communicating arteries which takes place,

(1) Dr. Horner, in American Journ. of Med. Sciences, August, 1835, p. 282. Dr. H. objects to the philosophy.

(2) Horner's Patholog. Anat. p. 135. (3) Morbid Anatomy, vol. ii. p. 379.

(4) On the Theory of Congestion, in British and Foreign Med. Rev. No. 5, p. 21.

(5) Lond. Med Chir. Rev. Jan. 1839. We should say, however, that this observation appears to have been founded upon the results of ligatures to the jugular veins; which is not an example of what we consider venous congestion, or of any natural process. Nor, indeed, can we see any analogy between an experiment upon a rabbit, where the knife has been freely concerned, and the venous circulation completely obstructed by ligatures, and that spontaneous congestion of the human body, which arises from miasmatic poisons, "the suppression or diminution of natural secretions," "dejection of mind, sedentary habits," &c. The fact, therefore, as to the state of the arteries in the foregoing instance, is only offered for consideration. But whilst con-

more or less, after death. (1) But will this explain the vast accumulation of blood which often takes place in particular organs, and the fact of its limitation to particular parts, even though admitted to be the seat of common inflammation; when, in the most obvious cases of the latter disease no such venous injection takes place?

Dr. Stokes observes that "the simplest idea you can get of the condition of the brain in the congestive form, is to consider what its state is in persons who have been hanged. These persons have the vessels of the brain loaded with blood from the violent interruption of the venous circulation." Then follows the axiom in regard to morbid plethora of the blood-vessels, — "now, this increase in the quantity of blood circulating in the brain, may arise from two causes; one depending on the interruption of the venous circulation, the other produced by an increased action of the arterial system."

We have adverted, in the preceding note, to the absence of analogy in the foregoing cases; and we shall endeavour to show hereafter, that there is no greater resemblance than between the *ligature* and the *vital forces*. But, we are here reminded by Dr. Stokes, that "nothing shocks one so much as that which has a tendency to overturn and expose a favourite dogma." (2)

"What is commonly termed congestion of the brain," says an able writer, "I have endeavoured to show is simply a deterioration of the blood caused by an imperfect aëration, a prominent example of which occurs in the disease termed congestive typhus." (3)

Portal supposes that hepatic congestion "depends upon the large supply of blood through the hepatic artery and vena portarum, whilst its veins are proportionally smaller than in other organs." (4) But will this hypothesis answer for any other organ than the liver, and shall we have a distinct one for each organ?

"Whatever takes off, or diminishes, the mobility of the nervous power," says Cullen, "may very much retard the motion of the blood in the vessels of the brain; and that, perhaps, even to the degree of occasioning rupture." "I suppose that the disease (apoplexy) had diminished the action of the vessels of the brain, and thereby given occasion to a stagnation, which produced the appear-

stant dissections show us that the arteries are often void, when the corresponding veins are turgid with blood; on the other hand, we shall endeavour to show that there is a great tendency in acute forms of venous congestion to pass into common inflammation, and may become more or less complicated with it. Whenever this happens, the capillary arteries, as well as the veins, will be more or less preternaturally full.

(1) Cyclopædia of Prac. Med. Art. Apoplexy.

(2) Lectures on the Theory and Prac. of Med. pp. 249, 357.

(3) Dr. Hooker, Essay on the Relation between the Respiratory and Circulating Functions. Boston, 1838.

This leaves unexplained the cause of venous turgescence.

(4) Obs. sur la Nat. et le Trait. des Malad. du Foie.

ance mentioned." (1) What occasions "stagnation" in cerebral congestions where there is no apoplexy, and where, too, there is a manifest increase of the "mobility of the nervous power"?

Professor Alison mentions two of "the most remarkable examples of the tendency to local congestions of blood, dependent on pernicious diseases." First, "the stagnation of blood in the vena cava desendens, and appearance of various symptoms indicating compression of the brain, or sometimes bloody effusion on the brain, consequent on obstruction to the flow of blood through the heart, and, therefore, frequently succeeding to violent palpitations, however caused." Secondly; "a similar stagnation in the jugular veins and congestion in the head, leading to various diseases in the brain, in cases of disease where there is obstruction to the free flow of blood through the lungs, although the heart be sound; especially if there be frequent exertions of coughing, as in bronchitis, hooping cough, or asthma." (2)

Although we shall endeavour to show, that affections of the lungs never impede the return of blood from the head, we may now say that venous congestion of the brain is a very rare consequence of either of the foregoing diseases.

Hamilton, (3) and Malcolmson, (4) say that the venous congestions of the liver, head, kidneys, etc. in berberi, are owing to obstruction. Dr. Mackintosh, in speaking of the cold stage of intermittents, remarks that, —

"The term congestion implies that the balance between the arterial and venous systems is lost for the time, the latter being overloaded or congested; and not that the circulation in any organ, or set of organs, is entirely obstructed, which, notwithstanding, does actually happen in those extreme cases in which reaction does not take place, and the individuals die in the cold stage." (5)

"In congestion," says Dr. Wardrop, "the vascular system, more particularly the veins, of the affected part, are preternaturally distended with blood, whilst in an organ which is inflamed, there is a change in the condition of the arterial capillaries." He then proceeds to illustrate the former condition, by saying that "congestion of blood can be artificially produced in the arm by tying a ligature around it, and thus distending the veins." (6) And again; "when from any disturbance of the circulation in the right heart, the veins of the brain become preternaturally distended, symptoms supervene," &c. (7)

The Medico-Chirurgical Review, (8) in its notice of Dr. Macartney's Treatise on Inflammation, remarks, that "congestion, as Dr. M. rightly observes, *belongs*

(1) First Lines, &c. s. 1119. Cullen evidently saw that the accumulation of blood in the brain cannot be explained on mechanical principles. But whatever be meant by a "diminution of the mobility of the nervous power," it seems to be evident that its imputed influence should be co-extensive with the venous system.

(2) Outlines of Pathology, &c. p. 378.

(3) Edin. Med. Journ. vol. ii. p. 22, &c. (4) On Beriberi, pp. 132, 133, &c.

(5) Elements of Pathology and Practice of Physic, vol. i. p. 58.

(6) On Bloodletting, p. 64.

(7) Wardrop on Diseases of the Heart, part 1, p. 87.

(8) January, 1839, p. 135.

o the venous system. It is caused by any mechanical impediment to the free motion of the blood in the veins; such as obstruction to the circulation of the blood in the liver, the lungs, or other important organs, or by pressure of the trunk of any vein. It is also brought on by the suppression or dimunition of natural secretions, and by supplying the body with more nutriment than is expended in growth or secretion. (Vol. I. p. 582, 611—625.) It is sometimes, indeed, and always formed by dejection of mind and sedentary habits, which serve to accumulate the blood in the venous system, and to embarrass the circulation. Every impediment to the passage of the blood in the small veins renders its passage slow. But the blood appears to be also very fluid; at least it transudes in many cases through the coats of the veins into the neighbouring cellular membrane." (P. 225, *note.*)

So far as there may be any increased fluidity of the blood, it appears to be opposed to the mechanical hypothesis in all its aspects; but, in point of fact, we think it is as often the other way. In respect to the escape of blood from the veins, we regard it as important in illustrating the pathology of venous congestion, and shall therefore consider its bearing in a subsequent section. See, also, Vol. I. p. 180—183.

No writers have described more accurately the symptoms of this affection when connected with idiopathic fever, or laid down a better plan of treatment, than Jackson and Rush; but we do not find any distinct expression of their opinion as to its pathology. The former, however, often speaks of "stagnation of blood" in the veins. We do not meet with the expression in any of Rush's works.

"We do not find," says an erudite reviewer, "in this part of his Essay, any sufficient reason to change the opinions which we expressed in a former number, that disordered circulation within the cranium, arising from increased impetus of arterial blood, or obstructed flow of venous blood, is frequently, of itself, the immediate cause of the symptoms both of irritation and of torpor." (1)

The doctrine which we shall offer will not interfere with the probability that cerebral symptoms are often owing, in part, to pressure constituted by congestion of the veins.

Dr. Abercrombie entertains the common mechanical doctrine so far as venous congestion concerns all parts but the brain, and mainly so in this organ. Having adopted Monro's views as to the departure of nature from her unity of design in having provided the circulation of the brain with distinct laws, he yields partially to this anomaly in expounding venous congestions of that organ. (Vol. I. p. 161.) Thus: —

"There is, however, another source of serous effusion entirely distinct from this, viz. interruption of the circulation in the veins in any part of the body. In

(1) British and Foreign Med. Rev. vol. iv. p. 437.

this manner, we see a tightly bandaged limb become œdematous below the seat of the pressure, and we find anasarca of the whole or part of the limb produced by the pressure of tumours, and ascites arising from induration of the liver. Whenever such interruption occurs in the circulation of a vein, it appears that increased effusion takes place from the exhalant branches of those arteries with which the vein is more immediately connected, depending probably upon a state of congestion in these parts, which in its effects is nearly analogous to inflammation. *Such a state of impeded circulation* evidently takes place *in the brain* from a variety of causes; such as the pressure of tumours, chronic disease of the sinuses, tumours on the neck, certain diseases of the lungs and of the heart, and probably from that very remarkable condition of the brain to which I have proposed to give the name of simple apoplexy. From serous effusion produced by such causes as these, probably arise those affections which have been called *chronic hydrocephalus*, and *serous apoplexy*." (1)

M. Andral, at the head of the anatomical school, considers venous congestion as a

"Mechanical" condition, and as "resulting from an obstacle to the venous circulation."

"*Pure* blood," he says, may escape from the *over-distended* vessels, just as water *transudes* through the permeable sides of a vessel in which it suffers *compression*. To *this cause* are to be referred several hemorrhages and dropsies produced by a simple transudation in a tissue mechanically congested; and, although these effusions have really *nothing active* in their nature, yet they are considerably diminished, and sometimes *altogether removed by bloodletting*, which, in such cases, acts in a *purely mechanical* manner, by removing *from the vessels* the fluid *by which* their parietes were kept in a state of over-distension. These *pathological* observations are quite exemplified in the majority of those cases of hæmoptysis, hæmatamesis, ascites, and other affections, which are connected with organic disease of the heart." (2) (Vol. I. p. 180 — 183.)

Mr. Morgan says of congestion, that, —

"In its most common acceptation, we understand the blood to be *collected* in the venous trunks, or in those branches of the capillaries which form the incipient veins, and in this sense, only, we can comprehend its meaning." He also thinks that, "its importance and the frequency of its occurrence have been also grossly exaggerated, — that it seems to be *ill understood*, and, consequently, much absurd reasoning has been adduced respecting it; and it has given rise to great confusion in practical medicine."(3) So it is said of phlebitis, (P. 222.)

Finally, the whole doctrine as to stagnation of blood, and the supposed dependence of the most formidable symptoms of venous congestion upon that mechanical condition had its origin

(1) Patholog. and Prac. Researches on Diseases of the Brain, &c. p. 41. See, also, p. 313 — 326.

(2) Patholog. Anat. vol. i. pp. 10, 42.

(3) Principles of Surgery, p. 45.

when less was known of the circulation, and the laws which govern organized beings. For thus, Hippocrates:

"Conversæ vero, et valde siccæ fientes venæ *intenduntur*, et inflammatæ afflu-entia attrahunt. Unde corruptio sanguine, et spiritibus non potentibus natu-rales in ipso vias permeare, perfrigerationes fiunt *ex statione*, et vertigines, et vocis interceptio, et capitis gravitas, et convulsiones. Hinc morbi comitiales fiunt, aut apoplexia, si in ambientes locos fluxiones inciderunt. (1)

We know of no writer, ancient or modern, when speaking of the cause of venous congestion, who has not regarded it as a "stagnation of blood;" and it is, in nearly all cases, referred to obstruction. Bichat condemned the latter doctrine.

We may conclude this section by adverting to the remarkable fact, that the researches of the present age in morbid anatomy should not have resulted in exploding the mechanical doctrine of venous congestion, and should have shed so little light upon its pathology. It is plain to every observer that this state of the veins had been connected with the diseases of which their sub-jects had been the victims. But, with rare exceptions, a phe-nomenon so striking has been permitted to pass with the simple record that the veins of this or of that organ "were injected with blood," or, "in a state of distension." This was not so, till the period of which we are speaking. From Hippocrates down, this condition of the veins had been supposed to have an impor-tant agency in the disease, however their views of it may have been as mechanical as our own. We therefore hold up this fact as one among the many others which we shall state in another place, that it has been the prevailing tendency of morbid anato-my since the exit of Bichat to divert us from the phenomena of nature.

SECTION II.

With the exception of Dr. Armstrong, we have now cited the most philosophical observers who have fully expressed an opinion, as far as known to us, as to the pathology of venous congestion.

Dr. Armstrong has investigated our subject more extensively than any other writer in relation to symptoms and treatment;

(1) L. de Viet. in Morb. Acut. s. 4, ver. 48.

but, like all others, he considers the state of the congested veins in a mechanical sense.

"His theory of congestive typhus presumes that the functions or structure of some important organ are deranged by an almost *stagnant* accumulation of blood in some part of the venous system." (1)

Nevertheless, we think it has been justly said by Dr. Boot, in speaking of "congestive fever," that, —

"I claim for my deceased friend the merit of having elucidated this important subject more distinctly than had been done before. He has enlarged, and at the same time simplified our views of fever, and has more satisfactorily explained its nature: and though he has laid no claim to the prize of originality as to the discovery of facts, he has the merit of having reasoned upon those previously known, more sagaciously than his predecessors had done, and of more clearly developing and arranging, by his masterly power of analysis and generalization, the intricacies of an involved and difficult subject." (2)

It is obvious, however, that Armstrong's mechanical doctrines of the pathology and treatment of venous congestion exclude the whole subject from the philosophy of life, and recognise no other principles than such as prevail in the world of dead matter. This leads him to confound what is really mechanical with what is, in the most important sense, of a vital nature. He therefore "simply places the loss of balance either in a morbid fulness of the veins or arteries locally, or in a general change of capacity, relatively, between the venous and arterial systems." (3) No essential distinction is made between the cause and condition of the local and general affection.

Dr. Boot thus represents Armstrong's views of the manner in which venous congestion arises in the brain:

"When the lungs and mucous membranes of the bronchiæ are affected," "the heart and brain may become consecutively affected from this condition of the lungs, which presents an *impediment to the ingress* of blood propelled towards them by the heart. It consequently accumulates in the right auricle and

(1) Cyclopædia of Prac. Med. Art. Fever.

(2) Boot's Memoirs of the Life and Medical Writings of Dr. Armstrong, vol. i. p. 133.

Dr. Boot pays a just and high tribute to the "Sketches of Epidemic Diseases in Vermont, 1815," by our distinguished countryman, Dr. Gallup. "The 1st section of his 5th chapter will show that his views of congestion were precise, and his treatment admirable." — P. 131. In the latter opinion we have already expressed our full concurrence; (vol. i. p. 224,) and we will add that we consider the section, to which Dr. Boot refers, as an example of the best medical philosophy extant. Gallup, however, leaned to the mechanical theory; but it is apparent in this section that his eyes were fully upon nature, and not upon hypothesis. The whole work is an honour to the age.

(3) On Typhus Fever, p. 115.

ventricle, and *is impeded in its descent from the brain*, which *thus* becomes *in its turn* congested." An effect is also ascribed to the unoxygenated blood. (1)

Again, "Under the term congestion," Dr. Armstrong says, "there are ten circumstances combined. 1st. The reduction of animal heat on the surface of the body. 2d. A recoil of blood from the surface to the centre of the body. 3d. A consequent excess on the venous side of the circulation. 4th. A correspondent deficiency on the arterial side. 5th. The heart has sustained a shock then. 6th. There is, in consequence of this, an impediment to the free return of venous blood, and that leads to the following as its necessary effect, namely; 7th. An over accumulation of blood in some weak internal part. This can be demonstrated by an examination after death. 8th. The respiration is weakened, &c. 9th. The constitution of the blood is thereby changed. 10th. A diminution of the nervous and muscular power." (2)

Nor can we regard the foregoing as descriptive of venous congestion. It is mainly an account of the physiological changes which occur during the cold stage of fever, or such as are apparently induced by local venous congestion, or by acute phlebitis, or by common inflammation when about terminating in suppuration. This condition, by the universal contraction of the capillary system of blood-vessels, evidently forms an universal obstruction to the circulation. But the obstruction exists on the side of the arteries, and the accumulation of venous blood must therefore be about the right cavities of the heart and venæ cavæ; and since the engorgement, which constitutes the disease under consideration, is often remote from the centre of the circulation, it appears to be evident that it cannot be determined by the central accumulation of blood, even could we understand the physiological condition which is implied by the term *weakness*, as employed in the 7th proposition. (P. 153.) This is apparent, also, from the frequent independence of local congestions, as seen particularly in most cases of apoplexy, of any febrile paroxysm; whilst, at other times, as we have said, they become the cause of a paroxysm analogous to the cold stage of idiopathic fever. That local congestions supervene on the invasion of fever is obvious enough; but we shall endeavour to show that they then depend upon causes as truly of a vital nature as the febrile paroxysm itself.

Our author also supposes, in common with others as we have seen, that venous congestion results from a simply diminished energy of the heart and arteries, and that it must necessarily follow, when their energy is much impaired, that they cannot

(1) Boot's Memoirs, &c. p. 130.

(2) Lectures on Acute and Chronic Diseases, vol. i. p. 167.

maintain the natural current of arterial blood, and that a proportionate accumulation takes place in the veins. (1) But here, as in the former case, the accumulation must be about the right cavities of the heart, not in the venous system of remote organs; and, as it respects the local effect of the obstructed capillaries, in either case there should be, upon mechanical principles, a diminished volume of blood in the corresponding veins, since its transit is partially arrested. Our author, also, well states that "when sufficient evacuations have been made," for the purpose of restoring the general equilibrium, "certain degrees of venous congestion may remain, partly from want of power in the heart and arteries;" (2) this being the mechanical aspect of the curative process. These remaining "degrees" appear to us to constitute the real congestion, which bloodletting has failed to subdue, and which may have been the essential cause of that depression of the circulation on which itself is supposed to depend. This disturbance of the general equilibrium of the circulation, as constituted by the cold stage of fever, or as induced by local congestions, we have considered at large in our Essay on Bloodletting, and have endeavoured to indicate the principles on which that remedy operates in its removal.

Our eminent author has a remark, sustained by common experience, which shows the little probability that venous congestion, in our acceptation, has any direct dependence on a prostrated state of the arterial circulation, or that "the degrees which remain after sufficient evacuations, are owing, in part, to a want of power in the heart and arteries." "In the last stages of typhus fever," he says, "there are certain degrees of venous engorgement about the viscera, resulting from the universally increased action of the arteries throughout the second stage." (3) Both facts are certainly as stated; but can it be admitted, that the congestion remains in one case because of the deficient arterial action, whilst it depends in the other upon augmented action? Here we must disconnect all idea of common inflammation from the congested organs, since their arterial system is often found in a natural state. The general circulation, too, is now universally vigorous. There is no accumulation of blood about the right cavities of the heart, no obstruction in the capillaries. Again, —

(1) On Typhus Fever, p. 159. (2) Ibid. p. 119. (3) Ibid. p. 53.

"I use the term congestion," says our author, "to denote the state which exists from the influence of depressants in which excitement does not take place." If there be no congestion or inflammation of organs, and no excitement, "then we have a perfect example of what I call congestive fever."

This, however, is nothing more than what happens in syncope, and in many other analogous affections. These, and indeed all fevers when introduced by a cold stage, should be, upon the general principle, equally congestive fevers, whether reaction take place or not.

As "a perfect example of congestive fever," Dr. A. speaks of "a gentleman who was travelling, and after eating some cold veal pie, fell on the floor in a state of extreme congestion, of which he was relieved by an emetic." (1)

Here, the illustration consisting in a central determination of blood, produced by a general contraction of the capillary blood-vessels, is only parallel with what we shall have seen affirmed of hanging, and ligatures to the veins. No distinction is made between those conditions and that in which venous turgescence of certain organs is productive of peculiar and remarkable phenomena. The various examples of congestive fever, which are adduced by Dr. Armstrong, and others, appear to us to consist in very different pathological states; agreeing only, in certain cases, in the general determination of blood from the circumference, which is made the type of venous congestion. In some of the cases, as in that of an overloaded stomach, the shock of a fall, or of a surgical operation, of fever, &c. the central determination of blood arises from the prostrating influence of the brain upon the heart, in which a morbid condition of the capillary blood-vessels has no participation. When a person dies of a fright, there is only the mechanical influx of blood. Yet, "this," says Dr. Armstrong, "is what I call congestive fever." (2) In these cases, however, the shock may establish a predisposition to venous congestions, or local inflammations, and may be ultimately followed by symptoms analogous to those of fever, arising either sympathetically from the local affections, or from a general violence done to the whole vascular apparatus. But we do not consider either of these conditions fever; though they serve to enlighten us as to its philosophy, and the rationale of the attendant venous congestions. (Vol. I. pp. 205, 209.) (See APPENDIX II.) The cases are analogous to that where the body becomes severely chilled by cold, and a state of reaction follows. And, to continue the parallel, if reaction do not soon follow, the

(1) Lectures, &c. vol. i. pp. 171, 178, 179. (2) Ibid. p. 149.

impression of the cold may be so great as to develope local congestions, and inflammations; and this, where the subject dies without reaction. Cases of this nature will be examined hereafter. (See APPENDIX III. on Effects of *Cold*.)

In fever, which proceeds from truly morbific causes, the determination begins at the circumference, and is owing to the pathological condition of the extreme vessels. (See Vol. I. p. 204.) And again, in the same fever, so far as any local venous congestion may influence the phenomenon, it depends on the prostrating effect of venous inflammation, or some analogous condition, as we shall endeavour to show, both upon the heart and blood-vessels, but especially the former; and, therefore, in these instances, at the invasion of collapse, the determination of blood about the heart may proceed from causes acting conjointly upon the centre and circumference.

It is well stated by Professor A. H. Stevens, in relation to certain affections where the nervous system is especially concerned, that,

"The same effects result from apparently opposite, but, in reality, similar conditions. Thus we have retching and vomiting in apoplexy from congestion or extravasation of blood in the head; and the same symptoms in depressed skull, the same in crushed limb, the same after excessive bleeding, or an extensive burn." (1) (See *Motto*, Burton, p. 215.)

The primary constitutional symptoms may be alike in all these instances, for the reason that there is nothing specifically morbific in any of the remote causes, and from a common physiological change in the nervous centre upon which the phenomena mainly depend. We have already seen, in our Essay on Bloodletting, that the immediate result of its impression upon the brain is the transmission of the nervous influence to the heart and stomach, a reaction of those organs upon the brain, and other complex sympathies. In all these cases, too, the nerves which preside over organic life have sustained a violent shock of a common nature. The heart, becoming prostrated in its powers, transmits blood imperfectly to the brain, and all other parts; and this is another cause of the phenomena. The blood, also, accumulates about the right cavities of the heart; but, as yet there is no venous congestion, or other local disease. Subsequently, however, either in cases of excessive bloodletting, of accidents, burns, cold, surgical operations, &c. a state of venous

(1) Lecture at the New-York Hospital, 1839.

turgescence, or common inflammation, springs up, most probably in the brain. This may take place, in some of the cases, during the general prostration of the vital forces, but more commonly after some reaction has begun. If the cerebral veins suffer, this is venous congestion, and grows out of the direct shock that had been sustained by the brain. It will probably be greater, too, in proportion to the duration of the stage of collapse, especially should much reaction follow. The reason is obvious. The longer the brain is affected in its general circulation by the prostrated state of the heart, the greater has been the direct injury of its powers, whilst they will suffer farther from the prolonged interruption of the circulation, and from the corresponding influence of sympathies. Violent reaction following, would lead, of course, upon our principles, to aggravate the venous congestion, and be very likely to transmute a state of venous congestion into common inflammation. This disposition of the former condition to pass into the latter we shall consider in our 14th Section, when we shall probably find in its philosophy a proof of our pathology of venous congestion.

Again, the local developments will arise in other parts, especially in cases of shock from falls. This may depend upon contusion or direct shock of some internal organs, or may grow out of the shock of the nervous centre, as frequently witnessed in congestions of the liver, after general concussions, or a blow upon the head.

At other times, when the predisposing causes have been specific, as they are called, as in miasmatic affections, the nervous system is specifically impressed, as well as the vital powers of the other tissues, (Vol. I. pp. 474—478, 568—575,) and venous congestions are more pervading, and their pathology, as we shall endeavour to show, more or less specifically modified. There being, also, no sudden and violent shock of the nervous centre, as in the former cases, or as happens in poisoning by the gas of charcoal, alcohol, opium, prussic acid, &c. (Sec. 12,) the vascular system of the brain may suffer no derangement, and venous congestions be as apt, or more so, to be determined in the liver, stomach, &c. It is also evident that when miasmatic causes operate, they alter more extensively and profoundly the vital powers, and make general impressions on this or that organ which contribute largely to the venous congestions. This last, and analogous causes, are those, also, which induce the true

congestive fever; complicating venous congestions with a profound constitutional affection, but which are confounded with the mechanical determinations of blood about the heart, as in cases of accidents, &c. or the local congestions, or the reaction, that may spring up in consequence. These different cases, as was said of some other analogous varieties of disease, "may not be improperly compared to certain species of plants, by no means uncommon, which are liable to be confounded with others by an inattentive observer."(1) There is often, also, a strong resemblance between the phenomena which attend the depressing effects of injuries, those of venous congestion at its stage of prostration, and those which distinguish the first period of constitutional fever. Our diagnosis may be, therefore, in the last instances sometimes difficult, and in cases rapidly fatal perhaps impossible.

Again, according to Dr. Armstrong, simple congestive fever "consists exclusively of venous congestion, unattended subsequently by excitement, and is a deviation from the usual law of nature." We should therefore look for some extraordinary vital modifications to establish such an apparent "deviation from the usual law of nature;" or should, at least, regard the affection in a strictly local sense. This want of the secondary stage, in the cases supposed, evinces the absence of that important element of disease on which it depends in constitutional fever; and should reaction, or other sympathies ultimately supervene, the discriminating eye will readily separate these results from the phenomena of idiopathic fever. The congestion which is induced by specific causes is considered "the most formidable of all the varieties of fever;" whilst the nine others which we have seen enumerated by our author are such as develope local rather than constitutional disease, and are the predisposing as well as the exciting causes. So far, therefore, as the resulting congestions are truly morbid, we include those causes amongst our evidences that the vascular turgescence is a consequence of some morbid state of the venous tissue, and upon which the phenomena mainly depend. D. A.'s account of the symptoms generally relates to what we regard as the second stage of congestion, or the stage of constitutional sympathies.(2)

We need not speak of the momentous practical errors which

(1) Abernethy's Preface to his Works. (2) See, also, Boot's Memoir of the Life and Medical Opinions of Dr. Armstrong, vol. i. p. 128.

have evidently grown out of this confusion of different pathological conditions, however the treatment of either may have been empyrical. Bloodletting has been found, for unknown reasons, since it was first indicated by Aretæus, (Vol. I. p. 323,) to be indispensable in congestive fevers attended by great sinking and prostration; and for this, and analogous reasons, the same practice has been largely and fatally applied at the invasion of those concussions which are produced by falls, and other mechanical injuries. Morbid anatomy, too, finding that venous congestions have ultimately supervened, has contributed its indiscriminating part. And we may here advert to another, though less important error which is made by our author in common with most other writers, in confounding typhus fever with the affection which arises from dissection wounds, &c. (1) In the latter case, either direct, pernicious inflammation of the venous or the analogous absorbent tissue is produced, or equally pernicious impressions are made upon the nervous system, &c. as we have already endeavoured to explain. (Vol. I. pp. 397, 474 — 498, 515 — 522, 539 — 549, 563 — 575.)

In local congestions which evidently grow out of specific causes, and in many of a chronic nature where the causes may have been complex and perhaps inscrutable, we constantly find that there have been numerous evidences of the existence of the congestion, for a longer or shorter period, preceding the development of those symptoms which are said to distinguish the first stage. These phenomena denote a radical difference between true congestion and mechanical accumulations of blood. They may consist, according to the seats of the disease, of languor, headache, flushed countenance, soreness of the eye-balls, tightness of the forehead, pain in the right hypochondrium, or in other parts, a morbid state of the secretions, &c. At this primary stage of the disease, the pulse is commonly excited, sensibly hard, and if blood be drawn with proper precautions, it is often buffed and cupped. Sometimes the pulse is now preternaturally slow, especially in hepatic congestions, and it is then apt to be intermittent or irregular, and this not unfrequently continues after convalescence has begun.

After an uncertain duration of the primary symptoms, there commonly takes place a sudden change in respect to the state of the general circulation. The symptoms of excitement disap-

(1) Lectures on Acute and Chronic Diseases, vol. ii. pp. 127, 147.

pear; the pulse becomes smaller, but still its hardness may be generally distinguished; it is sometimes more frequent, and again is slower; if previously irregular, this irregularity often increases, or it may now be developed for the first time. The sanguineous capillaries become contracted, the surface more or less pale and cool; prostration of strength, depression of mind, &c. are suddenly augmented; the blood darker, and for the first time trickles from the divided vein. It is at this stage, that the attendance of the physician is commonly required; and it is, therefore, not so remarkable, perhaps, that these cases should be confounded with the collapse which is induced by falls, blows, wounds, &c.

At other times, however, the phenomena which distinguish the primary stage of congestion go on increasing, till the disease passes into common inflammation, when the seat of plethora is more or less transferred from the veins to their communicating arteries. In these cases, of which luminous examples are given by Annesley in his Diseases of India, the only important change of symptoms which existed during the congestive stage, is that of their progressive increase; being often, however, but little more severe after the congestion had passed into inflammation.

If venous congestion precede an attack of idiopathic fever, the same symptoms of excitement generally forerun the stage of depression, though the local affection may ultimately contribute with the constitutional predisposition in producing that first stage of the fever. In these cases, the stage of depression may be prolonged by the ultimately depressing influence of the venous affection. This, however, generally depends more upon the specific nature of the predisposing causes of the constitutional disease; or, when the congestive affection operates, much upon the nature of its own remote causes.

We have seen that it is the opinion of Dr. Armstrong, and of all others within our knowledge, that,

"When the lungs and mucous membrane of the bronchia are affected, the heart and brain may become consecutively affected from this condition of the lungs, which presents an *impediment* to the ingress of blood propelled towards them by the heart. It consequently accumulates in the right auricle, and ventricle, and is *impeded in its descent from the brain*, which *thus becomes* in its turn congested."(1)

And again, it is said by Dr. Armstrong, that, "in examining the bodies of some patients, who had died of the most concentrated attacks of congestive fe-

(1) Dr. Boot's Memoir of the Medical Opinions of Dr. Armstrong, vol. i. p. 130.

ver, I have found the right side of the heart loaded with dark blood; and in reflecting upon the phenomena of all, am now inclined to believe, that their pathology is intimately concerned with the functions of the right ventricle. For when the action of the right ventricle is diminished, and when it is overloaded by too great an accumulation of venous blood, it must, by consequence, occasion a REMORA of venous blood in *distant organs*. (2) (P. 232.)

We have thus brought together the whole ground of the mechanical doctrine of venous congestion. If it be supposed, in other instances, to be constituted by venous obstructions in particular organs, the principle is the same; and, indeed, the type of the whole pathology is commonly borrowed from the experiment of a ligature around a vein. But, taking the foregoing rationale as something which prevails in natural congestions, especially of the brain, the mind is at once struck with the assumption of conditions which never exist, and with the absence of all those great physiological considerations which should form the fundamental ground of every pathological view of disease, and of every remedy that may be applied for its relief. (Vol. I. p. 290, &c.)

In the first place, we may say, that in cases of fever, there are only two conditions under which an accumulation of blood takes place in the right cavities of the heart; one of which is the stage of collapse, the other that decline of the functions which precedes death. Congestion, and even greater trouble, of the lungs themselves, as we shall show, do not constitute an obstruction to the general circulation. It is also true, that in a majority of cases, no accumulation of blood is found about the right side of the heart; and when it is so, the "lungs" are generally free from accumulation of blood. This will be found to be true in reference to specific facts. It cannot, therefore, be assumed that "the pathology of congestive fevers is intimately con-

(2) On Typhus Fever, sec. 3, p. 57. Our author has, also, another pathological view of fever, to which we cannot subscribe. "If I had to fix upon any part of the body," he says, "as more immediately involved in the production and continuance of fever, it should be the heart." — (*Ibid.*) We apprehend that the essential change which constitutes the pathology has been originally exerted by the morbific causes directly upon the vital forces of the capillary vessels; (vol. i. pp. 474 — 480, 568 — 575,) that the special disturbances which ultimately ensue in the heart are mainly of a sympathetic nature; and, however they may tend to maintain the morbid action at large, it is in a comparatively unimportant degree, and that when these disturbances of the organ subside, it is in consequence of the decline of that disease in which they originated. We shall have something to say on these reciprocal influences, whether in health or disease, in our section on the powers which circulate the blood.

cerned with the functions of the right ventricle," either in the foregoing, or in any other sense.

Again, as to the local congestions, they occur in distinct parts, and often remote from the centre. Were this distant plethora, as in the brain for instance, occasioned by the accumulation about the heart, then should it exist, upon the soundest principles in hydraulics, in all parts, as far as the remora may extend. But our author, in another place, states the facts which leave the subject on its true grounds.

"Though the engorgements," he says, "may exist in different organs in the congestive typhus, yet the *large* vessels about the right side of the heart, the *veins of the brain*, and of the liver, are *most frequently*, and *seriously* affected, and *next* to them, those of the spleen and *lungs*." (1) (Also, p. 217.)

Here, then, the mechanical hypothesis must abide by its own facts and principles. There exists an accumulation of blood about the right side of the heart; but the local congestions must be independent of all "remora," since the *brain* is *most* frequently, and may be alone, the seat of congestion, whilst the *lungs*, so intimately connected with the circulation of the right ventricle, are *least* frequently so. These are important facts to be borne in consideration. They, individually and unitedly, settle the question against the mechanical doctrine which relates to the dependence of congestion upon central determinations, or remora, of blood. We see, also, in the comparative exemption of the lungs, which will not be denied, that the common reference of the *primary* obstruction to these organs is without foundation. So far, however, as the remora may pervade the veins of an organ in the vicinity of the heart, the stimulus of distension, suddenly and violently inflicted upon organs predisposed to disease, may contribute to that pathological state which constitutes the essential part of venous congestion. But it is manifest that this is only one of the numerous *exciting* causes, and performs only a subordinate part. The predisposition was already formed by other causes; else should the lungs, which are least frequently congested, be always thus affected, and the brain most rarely; whilst the latter is the most common seat of the attendant congestion. Indeed, the blood is as well determined *from the head* towards the centre, as from the other extremities. And, then, we have congestions springing up at the onset of febrile paroxysms, where there had been no antecedent cold stage; but, most of all, do we

(1) On Typhus Fever, sec. 3, p. 54.

find an objection to the imputed mechanical cause in the frequency with which those congestions manifest themselves before there is any constitutional disturbance. That such conditions exist, we are authorized to infer from the attendant phenomena; and it is especially demonstrated in apoplectic affections, and when other congestions prove suddenly fatal.

Again, we have more from Dr. Armstrong, to the same effect; nor shall we fail of observing from the natural comparison which is instituted between inflammation and venous congestion, as well as from what we shall see more directly of the analogous phenomena of each, that one is as much a vital condition as the other, and that both are allied in their phenomena. Thus:

"Like inflammatory fever, which is known under a great variety of names from the mere locality of the inflammation, — the congestive form, from the modification of symptoms to which its local effects give rise, includes various affections having different abstract denominations. Yet each class of diseases depends upon one primitive condition: for while fever, combined with inflammation, makes up the simple pathological state of all the varieties of inflammation, so *venous congestion is the simple condition which forms the essential characteristic of different diseases, scattered arbitrarily, and disconnected, in the works on systematic nosology.* If we attend to the predisposing and exciting causes of congestion; to the general or partial effects to which it gives rise; to the symptoms by which these are indicated, — we can understand and explain the nature of one form of apoplexy and of hydrocephalus, of some catarrhs, especially in old persons, of one of the varieties of diarrhœa, of the worst form of cholera, and of several chronic diseases; and can *account for those effusions* which are observed in different cavities after death, when no symptoms had indicated their existence during life. The term Congestive Fever, however, is not strictly applicable to all these cases, because *local congestions may occur without being followed by the consequences of a more general condition.* Yet in *all its forms* it is naturally allied." (1)

The foregoing statement, which is clearly founded on an observation of facts, and sustained by the analogous experience of Robert Jackson, and some other faithful observers of nature, embraces many important considerations which severally terminate in the conclusion, that there is nothing of the imputed mechanical nature in venous congestions. The several points, as well as others hitherto introduced, will be more extendedly examined in their appropriate places; whilst various specific facts will arise in our progress, by way of illustration. But, before parting, for the present, with our most able author, we must carry out the duty we had prescribed to ourselves of noticing what we regard

(1) Boot's Memoir, ut. cit. p. 132.

as another fundamental error in doctrine. To this we have already adverted; and it clearly arises as a hypothesis out of the mechanical doctrine of "stagnation" and "obstruction." Thus:

"And what constitutes the predisposition to congestive fever? It is, in one word, DEBILITY." This debility may be "general," or "it may be *local*, while the *general system is apparently strong*." (1)

This is the "bark and wine" doctrine, whose results not only contradict the assumption, but prove it an unintelligible subtlety. It was laid down by Armstrong, as we have said, as a necessary *sequitur* of the doctrine of "stagnation," and as naturally suggested by its coincident relation to the phenomena of dead matter. Nevertheless, since our author brings the effects of remedies to sustain his views of venous congestion, and these consisting especially of bloodletting, and other depressing agents, it is remarkable that he should not have seen the error, as well as the pernicious tendency of the hypothesis. (Pp. 153, 232, and sec. 15; and Vol. I. 279 — 280, 420, *note*.)

It is said by Dr. Kellie, that, —

"In obstructions of the auriculo-ventricular valves, an obstacle exists to the free return of the venous blood from the head."

But that this is hypothetical appears to be shown by our author, when he immediately adds, that,

"Of the several cases of enlargement, and of other structural diseases of the heart, which have come under my own observation, *not one* of the patients had any lethargic or apoplectic symptoms." (2)

Considering the local character of venous congestions, that they are consequent upon a great variety of remote causes, as well as of sympathetic influences and moral emotions, and that the brain is especially liable to be the seat of the affection, where the descent of blood is promoted by gravity, it seems to be obvious from these facts alone that obstruction, and passive relaxation, are as little entitled to respect as the doctrine of remora. The hypotheses have been devised without a proper reference to facts; and they must yield even if we shall not have substituted a better. Is there any case extant which will resolve the fact, that in many epidemic fevers the veins of the brain are found almost universally in a state of engorgement; attended, also, by many of the usual phenomena of inflammation, whilst there is an absence of arterial plethora? In respect even to the hypothesis by Cullen, (p. 226,) the only one of a vital nature which has been offered,

(1) Armstrong on Acute and Chronic Diseases, vol. i. p. 157.
(2) Med. Chir. Trans. Edin. 1824. p. 141.

it is manifest that it would be inapplicable to the brain, for which it was especially designed, on account of the influence of gravitation; whilst there are no facts to render it intelligible in the principles it involves.

Dr. Stokes, we perceive, carries out upon this subject, the principles inferred from the mere effect of hanging. (P. 226.) Perhaps we have already said more than is proper in opposition to this mode of illustrating the causes and pathology of disease. The venous engorgements consequent upon febrile attacks, which Dr. S. so well describes, are supposed sometimes to remain, but more commonly "to disappear when the hot stage comes on." These venous congestions are, also, well supposed to give rise to common inflammation. But what interests us at present, is the cause of the persistence of venous engorgements in some cases after reaction, and not in others. Here the illustration of hanging should be carried out. But as soon as the rope is removed, the accumulated blood, as we shall have occasion to show, escapes from the brain, notwithstanding the supposed peculiarities in the circulation of the organ. Here, however, in cases of fever, it invariably remains after the subsidence of the cold stage has been followed by that of reaction; and this, too, however its escape may be promoted by the force of gravity.

Regarding venous congestion in its most simple form, and not complicated with idiopathic fever, we must look for the cause of that prostration of the general circulatory powers, which often ensues, and other remarkable phenomena, not in the accumulation of blood, but in some morbid condition of the venous tissue. The former, as we shall show experimentally, is wholly inadequate to the results. Or, returning again to constitutional fever, or still regarding the disease in its abstract sense, if it be said that some modified state of the veins of the affected part favours the accumulation of blood, as admitted by Dr. Armstrong, in a vague sense, (pp. 232, 243,) we shall then have obtained a starting point, for the investigation of the true pathology. Indeed, this shrewd observer states in one place, that "in the regular congestive fever, the *topical* accumulations of blood are so great as to overpower the natural energy of the heart;" (1) &c. And if this be often true of the failure of the heart's action in congestive fever, why not more generally so in simple congestions? The

(1) On Congestive Typhus.

proof, that it is so, is to be found in the history of their progress. (Sec. 6.) If the topical congestions be complicated with constitutional disease, the latter may be alone concerned, by its early operation, in the depression of the circulatory powers. But, as observed by Dr. Armstrong, it often happens in these complications that the action of the heart is overpowered mainly by the topical congestion. When the constitutional cause operates, the prostration occurs in paroxysms, more or less regular, and is followed by a state of reaction. In the other case, the paroxysms are more irregular, and the cold, or the hot stage, is apt to be prolonged, especially the former, at the explosion of the disease. Both conditions, however, often contribute more or less equally; producing great varieties in the phenomena, which are farther diversified should common inflammation, and derangements of structure, supervene.

In all other diseases we find that the extreme capillary vessels are the instruments of morbid action. Analogy would prompt us to look for some such principle in venous congestion. But, as a morbid condition of the capillary veins would not alone explain the expansion of the larger series, where shall we be so likely to find the true seat of the difficulty as in the coats of the veins themselves? This principle, we shall ultimately see, will resolve the difficulty of accounting for the prostrated state of the heart and arteries, when it arises from congestion of a single organ, and will explain other remarkable phenomena, and the dangerous tendencies of this affection; which it appears to us in vain to interpret by any mechanical hypothesis. We believe there is no affection more strictly vital in all its aspects; until, by its powerful sympathetic impression on the whole sanguiferous and nervous systems, that prostration is produced which determines an accumulation of blood about the right cavities of the heart, when the phenomena become influenced by this mechanical result.

"That congestion of blood in the great veins," says Dr. Alison "is not, *per se*, adequate to account for the phenomena of any form of fever, appears distinctly from the fact, that no form of fever follows the great congestion there in cases of suspended animation, in syncope, or from extreme cold, or submersion in water." (1) (See APPENDIX III. *on Cold.*)

But, whilst no constitutional derangements or local congestions may follow the great central determinations of blood as

(1) Outlines, &c. ut cit. p. 518.

expressed in the foregoing quotation, the most violent symptoms are produced by "congestion of blood in the great veins" of every important organ. There is, therefore, a broad distinction in these different pathological conditions. It is manifest that there must be causes operating in one, which have no existence in the other. One is indeed mechanical congestion, as it respects the accumulation of blood, and may produce no morbid phenomena; (1) the other is vital and prolific of vital results. One is determined by a general contraction of the capillary system; "sanguis presentem horrorem metuens, ad partes maxime *calidas* concurrit."(2) (p.234.) The moment that reaction takes place, (and in syncope it may be in a moment,) the equilibrium is restored. In the other case the affection is limited to the venous

(1) It is unimportant to our purposes, whether this mechanical accumulation of blood about the centre of the circulation be the result of a primary impression on the heart, or upon the capillary vessels. In our remarks upon the philosophy of bloodletting, we have endeavoured to show that either cause may operate under different circumstances.

(2) Hippocrates, de Flatibus. — The view which we have now taken appears to account in part for that paleness of the countenance, and for the absence of the blood from the surface generally, in many cases of apoplexy. (Vol i. p. 342, &c.) Dr. Philip, so conversant with the physiology of the brain, supposes that "its vessels suffer themselves to be morbidly distended by the force of the circulation, and thus to receive a greater than usual share of the blood sent to the head; the external vessels, consequently, receiving a small quantity, and the countenance becoming pale in consequence." This appears to us to be only a part of the prevailing mechanical theory of life. The cerebral engorgement requiring only a small additional supply of blood, and the external branches of the carotids being in no respect obstructed, and those being often tied with impunity, we are compelled to look to very different, and in a practical sense important, principles in explaining both the engorgement of the brain, and the pallor of the countenance. Besides, in this modification of apoplexy, dissections supply abundant evidence that the engorgement is seated in the veins; and, indeed, it is so considered by Dr. Philip. "The distension," he says, "is not in the capillaries; but a preternatural distension of the larger vessels is conspicuous, while the brain itself is often nearly or altogether of the natural colour." (*a*)

In our remarks on bloodletting, (sec. 2, 3, 5, 7, 9, 11,) we stated that it is the tendency of the nervous influence, in determinations of blood to the brain, to maintain the heart and capillary vessels in a state of excitement, whether the affection be inflammatory or congestive. This, however, may be true only of the early stages of venous congestion; though more so of the brain than of other organs. The precursory symptoms of the apoplexies under consideration are generally those of some degree of excitement; and it is not till the congestion begins to exert a depressing influence on the nervous power and organic properties, which is doubtless powerfully aided by its own pressure or pressure from extravasation, that the action of the heart is prostrated, and the blood deserts the surface. Then it seems to be, that the depressing effect of venous congestion, which is more or less its tendency at certain stages,

(*a*) Philip on the Means of Preserving Health, &c, p. 94; Essay on Indigestion, p. 149, &c.; Experimental Inquiry, &c., p. 279.

system of particular organs, is independent of any contraction of the capillary vessels, and is liable to a long duration. In the cold stage of an intermittent, bloodletting may almost instantly restore the general equilibrium of the circulation, (1) (Vol I. p. 205–208,) whilst numerous repetitions of the remedy may be necessary to subdue any attendant local congestion; and whether the fever will be at once subverted may depend greatly upon the presence or absence of local venous congestions. In typhus fever, the constitutional influence is still more obvious; and the vital nature of the congestive affection is farther indicated by the frequent salutary effect of quinine in one variety of fever, and its injury in another; the pathological state of the veins being modified by the remote causes in a manner corresponding more or less with the modified form of the constitutional affections. (Sec. 12.) It is also worthy of remark, that when venous congestions exist, the blood is not diminished in the congested veins, as it is in all other organs, during the stage of collapse.

Whether the central determination of blood be owing to venous congestion of some organ, or to idiopathic fever, or to both conjointly, this mechanical oppression of the heart embarrasses not only the whole circulatory system, but of course, also, the functions of the whole body. This, however, is a superadded cause, and constitutes no part of that simple state of local disease which it is our object to investigate. But, it too often happens that the primary and real congestion is overlooked, and remedies delayed till a depressing influence is extended over the whole arterial system, and the blood is accumulated in consequence about the centre of the circulation. It may be, then, a leading object to overcome this general interruption of the circulation, on which the phenomena are supposed to depend. (Vol. I. pp. 145, 200, 205, 209, 218.) But, if the disease be one of simple congestion, it will be seen from an attentive observation of the effect of remedies, and other consecutive symptoms, that

co-operates with the nervous influence in consummating the paroxysm. (Vol i. p. 342—261.) When this effect is exerted, the blood recedes, in the cases supposed, from all parts towards the centre, excepting the veins of the brain, which remain injected in obedience to the laws of disease.

(1) When a patient dies in the cold stage of fever, at its first invasion, the lungs may be found gorged with blood. But this may be only the result of the mechanical cause, from the immediate connection of the lungs with the heart. But this engorgement of the lungs from central determinations of blood is much less common, as will be seen, than generally supposed.

the central determination of blood has been only one of the numerous consequences of a local affection.

Again, congestions may spring up in rapid succession in remote parts, from the sympathies which prevail in the venous system, or, they may take place simultaneously, or, as we have said, in conjunction with constitutional fever; and then may follow, in the ratio of this general predisposition to congestion, those influences by which the general volume of blood is more or less determined upon the heart. The most formidable examples, being also often complicated with common inflammation, are the plague, yellow fever, the malignant intermittents of Italy and Africa, the "congestive fever" of our southern States, (p. 219,) scarlatina maligna, bad forms of purpura hemorrhagica, &c. In the foregoing cases of fever, the earliest form of local disease is generally the congestive, which may soon run into common inflammation, or the latter may sooner or later spring up independently. The venous congestions, however, are apt to prevail throughout the progress of the disease, and impart to either, as we believe, not only certain symptoms which they have in common, but their greatest obstinacy and danger.

The congestions, as we have said, often begin before the invasion of the constitutional malady, and seem not unfrequently to be an exciting cause of it. Again they are consecutive, or simultaneous, and appear to depend in part upon the general affection; according to the predisposition of particular parts to venous inflammation. Again the morbific agent may fail of producing its full constitutional impression, whilst it may determine perfectly the local disease. This may depend on the previous state of the system, or of particular organs, arising from habits, age, constitution, &c. It is for this reason, that in epidemic congestive fevers we often meet with congestions in their simple form, or complicated with every gradation of the constitutional disease. (See Sec. 12.)

In respect to the supposed interruption of the descent of venous blood from the head by congestions of the lungs, we have already stated the great infrequency of the latter affection compared with cerebral congestion. But the opinion appears to be also contradicted by direct facts; and since this subject has acquired an importance in the prevailing doctrine of cerebral and other congestions, and from its intrinsic interest to the physiologi-

cal doctrine of life, we shall give to it a particular consideration.

Goodwin, (1) Bichat, (2) Andral, (3) Louis, (4) Kellie, (5) Broussais, and others, have clearly shown either by experiment, or by anatomical facts, that no pulmonary or thoracic affection can constitute an obstruction to the return of blood from the head. Broussais says, "having closely investigated the subject, he has remarked that the difficulty of the disgorgement of the superior vena cava into the right auricle of itself seldom produces cerebral symptoms." (6) How constantly do we witness the greatest embarrassment of respiration from emphysema, congestion, and disorganization of the lungs, and accumulations of various fluids within the thorax, without symptoms of cerebral congestion. In all the cases of pulmonary emphysema by M. Louis, there was an absence of cerebral congestion. This corresponds with the more recent observations of others, so far as our knowledge extends. Dr. Philip relates a remarkable case of dyspnœa, in the course of which he says, —

"It was one of the peculiarities of this extraordinary case, that even when the fits of dyspnœa were worst, the patient never had any swelling of the jugular veins, unusual beating of the carotids, or temporal arteries, or other symptoms of impeded return of blood from the head." (7)

And who has not witnessed paroxysms of dyspnœa not less remarkable in the cholera asphyxia, and this, too, attended by the highest degree of venous engorgement of the lungs, but where

(1) Connection of Life with Respiration.

(2) Sur la Vie et la Mort, part 2, art. 6, s. 1.

(3) The translator of Andral's Medical Clinic on the brain complains that "he has taken no notice of the different pathological states of the lungs, which have a tendency to bring about cerebral congestions." — (*p.* 119, *note.*) And thus Mr. Ley, on laryngismus stridulus, whom we neglected in our first section:

"The cerebral disturbance, again, may be the consequence of the frequent attacks of breathlessness, almost amounting to asphyxia, which, by impeding the flow of blood through the lungs, causes accumulations within the cavities of the heart, and, subsequently, venous congestion within the cranium." — *p.* 81.

Dr. Abercrombie derives an argument in favour of his doctrine of deranged equilibrium in the circulation of the brain, from apoplectic attacks having rarely occurred during disordered respiration. In his own cases, however, there was not the slightest evidence, on dissection, of any change in the blood-vessels of the brain. They appear to have been instances of sympathetic, or what is called nervous apoplexy; the cerebral powers being either extinguished before any sensible changes take place in the vessels, or recovering without leaving any remarkable lesion.

(4) On Pulmonary Emphysema. (5) Edin. Med. Chir. Trans. 1824, p. 152.

(6) Principles of Physiological Medicine, &c. p. 405.

(7) Lon. Med. Chir. Trans. vol. iii. p. 290.

the brain has preserved the integrity of its functions to the last, and where, on dissection, its vascular system was perfectly natural? Here also, and in other diseases, cases are often occurring, where the heart, lungs, and other neighbouring organs, are gorged with blood, whilst the vessels of the brain may be preternaturally empty.

In respect to disorganization of the lungs, take the work of M. Louis on Phthisis, and we have an amount of proof sufficiently ample. Or the strongest possible example from another work by the same minute observer.

Thus, "the left lung was almost wholly converted into excavations, and the gray and opaque, or semi-transparent substance; on the right side, there were barely two-fifths of the lung permeable to the air, — so that the respiratory function was almost entirely confined to this diminished surface." (1)

In the brain, however, there was only a moderate fulness of the cerebral vessels, and this appears to have been in the capillaries. It is stated by this author, that "the cerebral functions are generally undisturbed in phthisis to the last moment of existence;" nor are lesions of the brain more common than after other chronic affections. (2)

If, in dyspnœa, the face be sometimes injected, or the jugulars swell, it arises from compression of the superficial veins by the respiratory muscles, or, in the former case, a determination of blood upon the arterial system. (3)

It should be stated, however, that observations were formerly made which were supposed to sustain the opinion that the return of blood from the brain is impeded by the act of *expiration*. (4) But, an attention to the nature of the observations must satisfy the inquirer of their deficiency; especially since they are contradicted by such a mass of proof as is daily presented by thoracic affections. The impression, however, is still very general, that, even a voluntary suspension of respiration impedes the circulation in

(1) Researches on Phthisis, obs. 29, p. 333. (2) Ibid. sec. 196, 206, 363.

(3) Different authors advert to this fact as a proof of the accumulation of blood in the head during expiration. It will be found as we have stated in the text. It shows us how one hypothesis may be brought in support of another.

(4) See Edin. Med. Essays, vol ii.; Blumenbach's Institutiones Physiologiæ; Med. Chir. Trans. vol. viii.; Ravina in Mém. de Turin, 1811; Magendie's Notes to Bichat sur la Vie, &c. pp. 310, 354, and in Journ. de. Phys. Expér. t. i. p. 200; Bostock's Elem. of Physiol. vol. ii.; Hunter on the blood, &c. p. 187; Experiments by Lorry, Schilchtling, and Lamure in Mémoires de l'Academ. des Sciences, t. i. and iii. pp. 113, 277; and of Haller, in Elemt. Phys. t. iii. l. x. p. 171 — 180, where numerous old authorities on this subject are quoted; and Exp. in Op. Phys. t. i. p. 231.

the lungs. Thus, Dr. Elliotson: — "The mere suspension of respiration impedes the circulation through the heart, by causing *obstruction* in the lungs." (1)

"Physiologists," says Bichat, "have all of them admitted that a mechanical impediment is put to the circulation of the blood by a collapse of the lungs. This idea they have *borrowed from the phenomenon of hydraulics*, and it is their reason for the death which ensues, in consequence of a too long continued expiration." He then proceeds to show experimentally that, "the very folds produced in the vessels by a state of collapse in the lungs," after exhausting them by a syringe, and at the same time laying open the cavities of the chest, "are *no real impediment* to the circulation of the blood." And, "along with these observations let us place the facility with which the pulmonary circulation continues to be made, when collections of water, pus, or blood, are lying within the pleura, or pericardium." (2)

What is true of complete expiration, and analogous causes, is equally so of inspiration. Inflate the lungs as powerfully as may be, according to Bichat and Magendie, and the circulation remains as free as ever. (3) Emmert has also confirmed the foregoing results, and Müller remarks, "the disturbance of the circulation, after the interruption of the respiration in the higher animals, is certainly not produced by the collapse of the lungs." (4)

When cerebral congestion actually supervenes as a consequence of pulmonary congestion, or other derangement of the lungs, it springs from the principle of sympathy. (5) And here

(1) Human Physiology, p. 210.

(2) Researches on Life and Death, p. 211—217. tr. (3) Ibid. p. 218.

(4) Elements of Physiol. vol. i. p. 190.

This inquiry is in no respect affected by the experiments of Alison, and Edwards, or the opinions of Williams, Kay, and others, as to an arrest of the circulation in the lungs in asphyxia; much less by the hypotheses which have been devised to account for this phenomenon.

(5) It is in no respect a mechanical phenomenon. M. Dumas, in his excellent chapter on the sympathies which prevail amongst organs, remarks that "we can no longer doubt that there are species of apoplexy, epilepsy, delirium, &c., produced sympathetically by diseases or lesions of the lungs; as in nervous asthma, suffocative catarrh, &c. (*a*) Bonet relates the case of an aged female who had been severely afflicted with difficult respiration for fifteen years, the embarrassment daily increasing. During all this time, she was exempt from cerebral symptoms; but, at length was attacked with hemiplegia, followed by apoplexy. The lungs were greatly disorganized, and the heart ulcerated. (*b*) Morgagni relates analogous cases; the cerebral attacks supervening suddenly, and only after a long duration of pulmonary disease. Armstrong, (*c*) notices the sympathetic influence of chronic affections of the lungs in inducing apoplexy. But, it is even far more so with affections of the abdominal viscera, which will form a subject for consideration.

"The tendency to local congestions of blood," says Dr. Alison, "is very much

(*a*) Principes de Physiologie, t. 2, p. 264. (*b*) Sepul. Anat. t. 1, l. 1, s. 2, ob. 9.

(*c*) Lectures, &c. vol. ii. p. 273.

we may notice the important fact that the heart is frequently empty when the lungs are gorged in their venous system; whilst, also, the vessels of the brain are in a natural state. Nor can we attempt an explanation, as will be farther considered hereafter, of the cerebral congestions which follow the destructive influence of narcotic poisons, carbonic acid gas, certain epidemic causes, &c. upon any of the prevailing doctrines. These agents exert their effect especially upon the tissues of the nervous centre, and it is here that they establish accumulations of venous blood, and other anatomical characters that arise from true congestion of the veins. It is also worthy of remark, that in these cases, the lungs and the heart are generally in a natural state. (Vol. I. pp. 574—512, 568—575.)

In obliterations of the jugular veins, or of the vena cava descendens, there may take place preternatural accumulations of blood in the cerebral veins. But we do not find that they have ever been attended by any of the phenomena of congestion, or of extravasations of blood or serum, or by any interruption of the cerebral functions. In obliterations of the venæ cavæ, a vast extent of the venous system, often of the great internal viscera, becomes involved in the circuitous circulation; but there is everywhere an absence of the characteristic symptoms of congestion. This, we apprehend, is owing to the absence of that disease in the venous parietes which we consider the essential pathology of congestion, and a principal source of the phenomena.

dependent, in many cases, on previous diseases," of which he states the following as an example: "A similar *stagnation* in the jugular veins and congestion in the head, leading to various diseases of the brain, in cases of disease where there is *obstruction* to the free flow of blood through the lungs, although the heart be sound, especially if there be frequent exertions of coughing, as in bronchitis, hooping cough, or asthma." (*a*) The rarity of cerebral congestions, in the worst of the foregoing affections, is sufficiently conclusive against the mechanical doctrine; but, as we shall endeavour to show farther, as it has no foundation in nature, we must look to the vital causes stated at the beginning of the foregoing quotation, which can be only supposod to operate through the power of sympathy, and thus to establish an alteration of the vital properties of the venous tissue of the brain. The same principles are concerned in the pulmonic cases, as when congestive apoplexy follows morbid conditions of the stomach, the intense operation of solar heat, "repelled eruptions," suppressed evacuations, mental emotions, and other instances where it would be absurd to apply the doctrine of mechanical obstruction.

We may also say here, that an undue importance appears to be often ascribed to temporary excitements of the heart in determining local plethora, both of the veins and arteries. (Vol. i. p. 380.)

(*a*) Outlines of Pathology, p. 378.

When that pathological state exists, it is probable that pressure contributes more or less to the results in cerebral congestions.

In a case by Dr. Wm. Hunter, the circulation in the superior vena cava, and the trunk of the left subclavian and jugular veins, was nearly obliterated by the pressure of an aneurismal tumour; but among all the painful symptoms there does not appear to have been any connected with the head. (1) Kellie says of this case, that "the consequences to the circulation within the head must have been equivalent to the obstruction of both jugular veins." (2) Baillie, (3) Hodgson, (4) Cline, (5) Raymond, (6) and others, have reported similar cases, but unattended by any remarkable cerebral symptoms. In a case of obstruction of the vena cava, by Mr. Wilson, the sinuses of the dura mater, the veins which entered them, and others of the theca vertebralis, were in a state of great enlargement. But the cerebral functions were unaffected. (7)

Lander, (8) Young, (9) and others, relate cases where the internal jugular veins and carotid arteries became involved in a morbid growth of the surrounding parts, and where one, at least, of the jugular veins was obliterated, without affecting the brain. Kellie concludes,—

"That the pressure of thoracic and abdominal enlargements, or other impediments to the free passage of the blood to or from the heart, as has been commonly considered to be their effect, do not have any tendency to force more than the usual quantity of blood upon the brain." (10) (*See our p.* 243.)

Sometimes, however, the stimulus of distension will develope in *certain* veins concerned in the circuitous circulation, as we shall farther show, a varicose affection, attended by pain and the organic changes to which varix is liable.

Nor was the exemption from cerebral symptoms, in the foregoing cases, owing to the effect of habit, incident upon a slow

(1) Med. Obs. and Inquiries, vol. i. p. 323.

(2) Trans. Med. Chir. Socy. Edin. vol. i.

(3) Morbid Anatomy, vol. i. p. 34; and Trans. of a Society for the Improvement of Med. and Chir. Knowledge, vol. iv. p. 127.

(4) On Diseases of Arteries and Veins.

(5) Trans. of a Society for the Improvement of Med. and Chir. Knowledge.

(6) Journ. Hebdom. de Méd. t. 11, p. 110. — The sides of the vena cava descendens were united near the auricle. See, also, "two cases of tumours pressing upon the superior vena cava," in Lon. Lancet, Jan. 27, 1838.

(7) Trans. of a Society, &c. ut supra, vol. iii. p. 65.

(8) Edin. Med. and Surg. Jour. vol. vii. p. 407.

(9) Hodgson on Diseases of the Arteries and Veins.

(10) Edin. Med. Chir. Trans. 1824, p. 152.

accumulation of blood; since, as may be abundantly shown, the experiment of tying the internal jugular veins is not followed by the slightest inconvenience. This observation, it is well known, is as old as Galen, and has been often repeated by others. Emettus tied the carotids and both internal jugulars of a dog. The animal was watched for many weeks, but there was no alteration in his appearance. The result has been often confirmed by others. Dr. Kellie

"Tied the common jugular veins of a dog as low in the neck as they could well be reached." The animal, having suffered no inconvenience, was killed on the seventh day. "The vessels of the pia mater and brain were moderately filled, and the sinuses were distended with blood. There was *no serous effusion*, and the brain had a healthy and natural appearance." (1)

Repetitions of the foregoing experiment gave the same result. Allowing something for Dr. Kellie's object, which was not favourable to our conclusions, and that the veins were even more turgid than represented, if we contrast the exemption from all symptoms, in these, and similar experiments by others, with those morbid phenomena and products which attend cerebral congestion, we must conclude that the pathology, in the latter instance, is wholly different from that which arises from mechanical obstruction, and that the morbid phenomena, which apparently arise from the plethoric state of the veins, in true venous congestion, depend, in reality, upon a pathological condition of these vessels, of which the turgescence is only a consequence.

It might be doubtful how far the foregoing observations are applicable to man, had not the same experiments been made upon him. Mr. Simmons, in a surgical operation, tied the internal jugular without inducing any cerebral derangement. (2) Simpson, (3) Stevens, Gibson, (4) and Morgan, (5) have done the same, with equal success. In Morgan's case of attempted suicide, the carotid artery was not divided, and the man returned to his employment on the 20th day.

In the cases of hanging which have been brought forward to illustrate cerebral congestion, we now see the inapplicability of the principle, and how probable it is that true congestion depends on

(1) Edin. Med. Chir. Trans. 1824, p. 163. See, also, analogous experiments by Sir A. Cooper, in Guy's Hospital Reports, vol. i. p. 473.

(2) Medical Facts and Observations, vol. viii.

(3) Edin. Med. Essays. vol. v. p. 337.

(4) Amer. Jour. of Med. Sciences, vol. xiii. p. 305.

(5) Ibid. vol. xviii. p. 330.

causes that are entirely foreign to mechanical obstructions. It is also well known that in cases of hanging, the accumulated blood escapes from the larger veins as soon as the ligature is loosened;(1) but this will not happen in *cerebral* congestions, even after the removal of the scull-cap, unless, perhaps, after an interval of some hours. Failing to bring about the morbid symptoms and products of venous congestion by obstructing the smaller trunks of veins, experiments like that of Lower, of tying the vena cava, are now and then made to exemplify the natural process of disease. They have been seriously taken as the ground of conclusion not only as to the mechanical philosophy of venous congestion, but to establish the mechanical dependencies of dropsy. (Sec. 16, and Vol. I. pp. 180, 514.) If the facts which we shall state in this essay, which have a direct bearing upon venous congestion, be admitted, the whole doctrine as to the mechanical pathology of dropsy, with the qualified exceptions we shall mention in our last section, must necessarily fail, and the phenomenon, when connected with venous congestion, be placed upon the same vital grounds as when arising from common inflammations.

Finally, it may be said that, the calibre of the veins being enlarged, the momentum of blood is lessened in consequence. But, in the first place, this objection is only raised after the enlargement has taken place. The question at issue is, what produces the enlargement? Whatever may be the diminished velocity of the blood, it is exactly the same as happens where the veins dilate under the influence of heat, exercise, &c. (P. 156.)

(1) Boerhaave states that he had been long familiar with the appearance of the brain after death from strangulation, both in man and animals. When the cord has remained tight upon the neck, the veins of the brain have been occasionally gorged, and many vessels have been brought into view which had never been seen under any other circumstances. We must suppose the same to have happened in the experiments of tying the jugular veins, as already related. But it is added by Boerhaave, that as soon as the rope was loosened, the blood escaped from the brain, "sed dum laqueus remittitur, *omnia evanescunt*, quia sanguis per venas jugulares effluit." (*a*) The same fact is stated by Morgagni, (*b*) Piorry, (*c*) and others. Lancisi, (*d*) Déjean, (*e*) Winslow, (*f*) Cook, (*g*) De Haën, (*h*) Coleman, (*i*) Kellie, (*k*) Wepfer, (*l*) Kite (*m*) Alberti, Littræus, Bruchier, &c. testify to the effect of the rope in producing great engorgements of the brain in the human subject. Still it is generally admitted that the blood escapes from the large veins whenever the ligature is loosened. Piorry,

(*a*) De Morbis Nervorum, p. 671. — (*b*) Ep. 19, a. 11, 15, 36. — (*c*) Collection de Mémoires, pp. 291, 302. — (*d*) De Subitan. Mort. Ob. 4. "Puncta sanguinea spisso agmine candidam medullæ substantiam variegare." — (*e*) Mém. de l'Acad. des Sciences, 1782. — (*f*) Ibid. 1741. — (*g*) On Nervous Diseases, p. 109. — (*h*) Rat. Medcn. t. i. — (*i*) On Suspended Respiration. — (*k*) Op. Cit. p. 137. — (*l*) Obs. Anat. de Apoplexia. — (*m*) Essays on Submersion; and Mem. Lond. Med. Soc. vol. iii. p. 215. How does all this affect the atmospheric doctrine of the cerebral circulation? (Vol. i. p. 161, *note.*)

SECTION III.

SUPPOSED OBSTRUCTIONS, ETC.

"Oculis magis credere oportet, quam opinionibus." — HIPPOCRATES, *de Diæta*, l. i. v. 85.

We have now seen that venous congestion is in no respect mechanically determined by a remora of blood during depressed states of the general circulation; whilst the affection is constantly presenting itself where no such depression has existed; and even during high states of febrile excitement. We have seen, too, that neither affections of the heart or of the lungs have ever any tendency to prevent a due return of blood from the head, where venous congestions predominate; and we have also shown, that when blood is greatly accumulated by mechanical causes in the veins of the brain, it produces no symptoms of disease. We might also ask why it happens that the lower extremities are so exempt from venous congestions, since they are constantly subject to ligatures; and why are the most complex organs the common seat of the affection, where no obstacle to the return of venous blood can be shown to exist? Nay more, how, on the principle of obstruction, or relaxation, or remora, will you explain the venous congestions which are equally and uniformly produced by the excessive application of prussic acid, alcohol, opium, and all the narcotic poisons, carbonic acid gas, a surfeit, certain shell fish, animal poisons, natural and morbid, from a blow on the head or over the liver, constitutional predisposition as in asthma, the peculiarities of age; and still in relation to particular organs, as the brain in infancy, &c.? And why does it happen that the blood retreats from the veins of the brain when the hangman's ligature is removed, but remains obstinately in morbid congestions? This query we shall answer by saying, that, in the former case the elasticity of the veins is not impaired by disease, and, consequently, the vessels contract as soon as the obstructing cause is removed; whilst in the latter case, the elasticity being

who made numerous experiments upon dogs, by hanging, drowning, and suffocating, says, that when the head was afterwards elevated, the blood would escape from the cerebral veins. (a)

(a) Collection de Mémoires, pp. 291, 302.

actually impaired, the veins refuse to contract, and the blood remains. We have shown the philosophy by an experiment upon the arteries at pages 175, 176. This, however, is truer of the brain than of the liver, since the congested veins pervade the latter organ, which enlarges in its congestions, and its whole substance may contract, by gravitation, after vital action is extinguished.

If, however, obstruction be really the cause, as is generally imagined, we must look for the supposed obstacle at some point in the largest veins of the congested organ. If the whole organ be involved in the venous plethora, it must then be supposed that the obstructing cause is seated in the principal venous trunk. But this is utterly contradicted by all experience in morbid anatomy. Bichat remarks that,

"Physicians who have opened but few bodies (!) employ the vague and insignificant word *obstruction*," &c. (1) (P. 251.)

When, however, an organ is congested only in certain parts, the obstruction must be formed in numerous veins at the largest areas. This is less consonant with probability than the former assumption, whilst it is equally contradicted by the anatomist. In respect to the liver, so constantly the seat of congestion, Bichat observes, without any special reference to this inquiry, that,

"Having a solid and granulated substance in which no extraordinary motion can take place, except those of the general locomotion of the organ, it is evidently incapable of frequently interrupting the course of the black blood which the abdominal system sends there." (2)

Finally, it is only in the capillary system that obstructions to the circulation of an organ, of the nature we are considering, can be constituted; and here the mechanical doctrine of inflammation may be defended on the ground of this possibility.

Again, it has been surmised by some that the essential pathology of venous congestion consists in debility of the veins, and their consequent relaxation. What is relaxation as applied in this instance? The result, certainly, of some modification of the vital forces of the venous parietes. This condition constitutes disease, and places the pathology of venous congestion upon purely vital grounds. And, if we now consider for a moment that this supposed relaxation is but a softening of the tissue, which certainly occurs in advanced stages of the disease, and

(1) General Anatomy, &c. vol. iii. p. 237. (2) Ibid. vol. i. p. 438.

look at the analogies supplied by inflammation, it appears that the question might be decided by this test alone; but more especially when connected with what we shall say as to blood-letting and other remedies for venous congestion. We are sensible, however, that the term relaxation has been borrowed, in this instance, from the physical sciences, and is intended to imply a simple mechanical diminution of the power of cohesion.(1) We believe the application to be opposed by all sound philosophy; but having sufficiently argued the distinction between the laws of life and of physics in other places, we shall only now ask, whether there be any other evidence of the imputed relaxation than the accumulation of blood? Will it in the least explain the phenomena of venous congestion, or account for the operation of its appropriate remedies? Would such uniformity of dilatation be likely to pervade the entire venous system of an organ? Would this condition spring up, as it were, in an instant, with all its appalling phenomena, if simple relaxation were the proximate cause? Why should this condition be so prolific of evil when the jugular veins may be tied without a cerebral symptom?

"Inflammation," says Mr. Annesley, "does not arise and advance to its acme at once, or even in a few hours; but, venous congestion may supervene in a very short time, and after but a brief duration, terminate in the inflammatory state." (2)

In the cholera asphyxia congestions spring up with the most amazing rapidity; and, perhaps, in no other condition of the body is the probable fallacy of the doctrines of relaxation, passive accumulation, and mechanical distension, better shown. Here the system is greatly bereft of blood. The vessels are generally in a state of great contraction, partly from the influence of disease, but more especially from the want of their ordinary

(1) Assuming at present, what we shall endeavour to show, that the pathological cause of venous congestion is an inflammatory, or some analogous state of the venous parietes, the enlargement of the veins could be readily explained by the ordinary results of inflammation; since the separation of the molecules, the softening of the texture, and an interstitial deposit of fluid, are exactly what follow that disease. But although these causes be adequate, and can only stand for what can be possibly meant by debility and relaxation, we do not, nevertheless, believe that they are the true ones. And supposing a loss of elasticity, we see not how this may happen without an antecedent affection of the vital forces; since the physical qualities depend upon the composition and texture of the veins, and these are produced by the forces and actions of life. Those forces always operating in organic bodies, maintain a constant control over the physical properties.

(2) On the Diseases of India, vol. i. p. 348.

contents. How improbable, then, that the entire venous system of particular organs should be gorged with blood, when its contraction, if not morbidly affected, should so readily follow the diminished volume of the circulating mass. Here, too, different organs may be, in different instances, the seats of the accumulation. We open the body, and find perhaps the *heart*, the *lungs*, the liver, and every other organ, except the brain, in a bloodless state. But the veins of the brain may be crowded beyond any other example. Again, with the same general phenomena, the lungs, or the liver, may be the seat of the engorgement, and the whole vascular system of the brain shall be nearly exempt.(1) Why is this so? Can it be from any sudden local debility of the vessels? Is there a remote analogy to justify the conclusion? Do we find the slightest appearance of obstruction? And general plethora is out of the question. What mysterious cause suspends the operation of gravity, and detains the blood in the cerebral veins with an appearance, as it were, almost to bursting? Are the veins of the dense parenchyma of the liver more susceptible of relaxation, or more liable to obstruction, than those of the external surface? Or, if the phenomenon depend on a general contraction of the capillary system, why is not the liver, and the region of the heart, its unvarying seat? These are questions for the understanding. We have seen, also, that these congestions, in undoubted idiopathic fever, do not in the least depend on remora, where, as in cholera, the right cavities of the heart are often bloodless.

In the cholera we have abundant evidence of a predisposition to inflammation in the very uniform phenomena which attend the alimentary canal, and we think we have formerly shown, without this construction in view, that the veins of these organs are more or less concerned in the morbid process. (2)

Were relaxation the cause, it is highly probable that some portions of the veins would be more relaxed than others. But no apparent inequalities take place; and for reasons which will

(1) This diversity in the morbid appearances is, in itself, conclusive that the accumulations in the brain do not depend upon any peculiarity in the laws which govern the circulation of that organ, as supposed by Monro, Kellie, Abercrombie, &c. (p. 255.)

(2) See our Letters on the Cholera Asphyxia of New-York, p. 130, 1832.

All the foregoing remarks in relation to cholera were embraced in our manuscript as stated in the introduction of this essay, and, of course, before any information had been disclosed as to the structural lesions which the veins sustain in this affection. We shall have occasion to advert to this fact again in our 7th section.

soon be stated, the current of blood is free, and no unusual pressure is exerted on the parietes of the vessels. In the true varix, such inequalities are often presented from disorganizing inflammation, and are vaguely assumed as an evidence of relaxation, and a consequent yielding of the parietes to the column of fluid. But, even in varix there is manifestly no retardation of blood, since on rubbing it out of the vein, the current follows the finger with the same velocity as in other veins. It is therefore a fair conclusion that the current of blood is equally undisturbed in venous congestions.

It should be also considered that the athletic and vigorous are most frequently the subjects of sudden and violent invasions of venous congestions; such as are most obnoxious to common inflammation, and where the apparent debility is an illusion. "The finest peasantry on the earth are the victims that typhus rejoices in;" (1) in which all observers agree. Stimulants, too, are one of the most prolific causes of venous congestions. And, where the causes induce universal disease, as in malarious fevers, the whole venous system, not one part alone, should be more or less relaxed, or obstructed, and congested. If it be assumed that one organ may be more susceptible than others, then the whole mechanical philosophy is abandoned, and the vital doctrine admitted. Physical agencies, attended by the imputed physical results, do not operate in the partial manner supposed. If miasmata obstruct or relax one part of the venous system, the effect must be coextensive with the whole, just as wax injected into the vena cava would *obstruct* the whole venous circulation, or as a dry substance on being immersed in water would become *relaxed* in all its parts. And, is there no analogy seen between these local developments of venous congestions and the coincident determination of inflammations, upon particular parts, by the same predisposing causes? That there is no real debility or relaxation in the case, is also manifest from the general effect of bloodletting in rëestablishing the strength, and in removing the supposed relaxation of the vessels. Still, no small proportion —

"See nothing but prostration; they prescribe for nothing but debility; they give wine instead of iced water; order bark instead of local depletion. They exasperate the disease by stimulants; and then, thinking they have not gone

(1) Medico-Chir. Rev. vol. xxxi. p. 559.

far enough, they heighten the stimulant and double the debility." (1) (Vol. I. p. 283.)

"The giant, that lies prostrate on the earth mastered by superior power, has still a giant's strength, though he do not at that moment put it forth. Give him but the chance of throwing off the load that keeps him down, and he will soon show you that he is not weak." "The notion of debility in the intense forms of congestive fever I look upon to be an error not less palpable in its nature than destructive in its consequences; and if the havoc it produces do not confer upon it a prëeminence as bad as that of the very disease of which it is supposed to constitute the essence, it at least entitles it, in comparison with every other error in medicine, to the distinction recognised in society, between the hero and the murderer. The one destroys a single human being now and then; but the other numbers its victims by thousands." (2)

Is it probable, if venous congestion depend on obstruction, debility, relaxation, &c. that the portal veins, without valves, stretching down to the verge of the rectum, and terminating in double ramifications, would be less frequently the seat of that affection, according to Kiernan, (3) Annesley, (4) and others, than the hepatic veins? Would not an obstructed state of the latter lead to a like condition of the former?

The doctrine of relaxation, whether applied to venous congestion or to the arteries in inflammation, is an invention by Themison, the founder of the Methodist sect. He considered philosophy an incubus upon medicine. It was with him all relaxation or all adstriction, or a compound of both, as we now have it in inflammation. (5) But, of all hypotheses to account for these local dilatations of the veins, no one appears more obnoxious to objection than Armstrong's supposition that the phenomenon sometimes depends upon a diminished energy of the heart and arteries. For, in the first place, the dilatations should then be universal, and greatest in the lower extremities; and, secondly, they occur, also, in excited states of the general circulation.

But, so entirely mechanical is venous congestion considered by the best observers, that they are led to ascribe results, which can proceed only from vital actions, to the supposed mechanical obstruction alone. Even when affecting the head, in cases where no possible impediment to the return of venous blood can

(1) Stokes' Theory and Practice of Medicine, p. 18.
(2) Southwood Smith's Treatise on Fever, pp. 95, 97.
(3) Lon. Philos. Trans. 1833, pp. 737, 743, &c.
(4) On the Diseases of India, &c. vol. i. pp. 342, 345.
(5) Celsus, l. i. p. 12. — And thus Juvenal: —

"Quot Themison ægros Autumno occiderit uno!"

be supposed to exist, we find Dr. Armstrong speaking as follows of one of the causes of hydrocephalus internus: —

"The first of these conditions is what I call venous congestion. And the probability is, that the effusion of fluid then arises from an impediment to the return of venous blood." (1) (Vol. I. p. 180 — 183, and Sec. 12 and 16.)

Dr. A. however, soon starts an objection to his own views, and which shows, especially, the fallacy of the hypothesis in those innumerable cases where no mechanical impediment can be surmised.

"Some individuals," he says, "have thought that the enlargement of the glands of the neck produces effusion by preventing the return of the venous blood; for instance, the enlargement of the bronchial glands. This may possibly be the case; but I have seen these glands enlarged a great many times without producing any effusion." (2)

It is now important to say, that there occurs a preternatural fulness of the veins whenever an increased determination of blood takes place in the capillary arteries of any part, without reference to the physiological condition in other respects. It may be from the simple excitement which follows exercise, heat, &c. or from common inflammation. (P. 146.) When, therefore, the patient dies under these circumstances, more than a natural proportion of blood may be found in the veins of the part, especially in the head. In these cases, however, it is far more likely for reasons we have stated, to be expelled by the contraction of the veins, or to gravitate from them after death, than in true venous congestions. (Pp. 175, 256.) We cannot speak too particularly of the importance of this distinction. Nay more, in the cases now supposed, it is possible that the volume of blood in the veins may be actually increased by the contraction of the capillary arteries, after death, upon their increased contents. But, we have never seen one of the cases which we are now considering where the fulness of the veins was accompanied with a corresponding injection of the capillary arteries, whilst it is well known that in true venous congestions the arteries are generally in their normal state, (with the exceptions to be mentioned hereafter, Sec. 14th,) and the veins are commonly more turgid than under the foregoing circumstances. (P. 225.) And when we connect with these considerations what we shall have said of the symptoms, treatment, morbid anatomy, &c. of venous congestion, the distinction will become sufficiently obvious to a superficial observer.

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 256.

(2) We shall ultimately endeavour to show how serous effusions follow venous congestions, and how they substantiate our doctrine.

We shall conclude this section with a case in some respects unique, but more interesting in its tendency to illustrate the pathology of venous congestion. It came under our observation after the foregoing was in type; and, although we had other analogous illustrations of our subject, we have concluded to substitute this on account of its recent occurrence.

The subject, Mrs. F., was 33 years of age, and, till the sickness which terminated her life, had generally enjoyed good health. She had a well disciplined mind, and, therefore, was not subject to strong emotions. Her illness began on the 15th of June, 1836, and her death took place on the 27th of March, 1840.

We found Mrs. F. on the 15th June, 1836, suffering from a moderate degree of abdominal derangement, and some cough which was evidently a sympathetic result. She was fully relieved of this difficulty by the last of the month. But, there immediately supervened a partial paralysis of the lower extremities, one limb being rather more affected than the other. From the disappearance of all abdominal disease, and some other circumstances, we immediately made up our mind that the seat of the affection was in the cerebral portion of the brain. To this belief we always adhered, although there were no other symptoms than the paralytic affection. This condition advanced gradually, and her toes, dropping, soon began to catch in the carpet, when the unfortunate patient would often lose her balance, and fall. No contra-indication existing, we began early the use of strychnine in doses of one twenty-fifth part of a grain. At this period, the patient could still walk abroad short distances, and in one of these rambles, about the third day after beginning the strychnine, she was seized with spasmodic twitchings of her limbs, and was obliged to be carried home. In a few days afterwards, we resumed the strychnine in doses of a fortieth of a grain, sometimes once, and sometimes twice, in a day. It soon developed slight spasmodic actions; but, no cerebral or spinal symptoms arising, we persevered in its use till we became satisfied that remedies of this nature would prove abortive. All the abdominal and thoracic viscera remaining sound, we suggested but little other internal means. Failing in our first effort, and trying galvanism in vain, and seeing little of a tangible nature in her case, we advised her removal to the country, where she spent a few months, from time to time, with the only advantage that she ever received from art.

This is one of the cases to which we have adverted in our first volume, page 238.

Nevertheless, she was seen by several eminent gentlemen of the profession, here and at Philadelphia, who generally came to the conclusion that the disease was seated in the lumbar portion of the spinal marrow, or depending on a tumour near the medulla oblongata. Different means, which it is unnecessary to state, though all of them safe, were employed at their recommendation.

We may pass rapidly over the progressive stages of this intractable disease, since the phenomena consisted alone in unintermitting advances of the paralysis. The subject was taken from her feet in a few months, and her mode of exercise became limited to a carriage, and a self-propelling chair within the house. Soon after, she lost her ability to thread her needle, and next to sew. Then followed a permanent contraction of all her fingers, resembling the half-bent claws of a bird. About eighteen months before her death, all muscular power became extinguished in her arms. Now, also, began a failure of speech, which rapidly acquired its maximum till within three days of her death. Her speech had been too inarticulate to be understood without close attention, but varied a little according to atmospheric changes.

During the whole of her sickness, she suffered no pain, and scarcely any irregularity of the organic viscera, excepting occasional, but moderate, constipation of her bowels. The circulation in the extremities was generally languid for some months antecedently to her death, and she was more or less subject to extreme, though not unpleasant degrees of cold in her hands and feet, which would be often followed by a preternatural and annoying temperature. (P. 28—37.) The radial pulse became rather languid, though not irregular or increased in frequency, for some of the last months. Tongue always well, appetite good, and the patient allowed fresh animal, and light vegetable food. She lost gradually some twenty-five or thirty pounds of flesh, the weight being at the time of death probably about ninety pounds. This emaciation, however, was not manifest in her countenance, which was always natural, and always lighted up by a smile.

We saw our patient nine days before her death. We did not then remark any particular change in her condition, not even in her voice, from what had existed six months before, only more ema-

ciation, and an increased tendency in the hands and feet to alternations of cold and heat. Her countenance was, as ever, intelligent, and her moral and intellectual faculties unimpaired.

On the 25th of March, we were summoned for the first time for nine months to prescribe for a more alarming evil than had presented itself before. On the evening of the preceding day she was attacked with a difficulty in respiration, and symptoms of abdominal derangement. The tongue was loaded with fur, and the pulse rather sunken. Speech had also become suddenly too inarticulate to be understood, excepting by those who were familiar with the embarrassment. No cerebral symptoms. Mind unimpaired. Respiration was partially relieved by a mustard cataplasm, and fully so on the following day by the operation of blue pill. Dejections dark. On the afternoon of the 26th a prolonged nausea spring up, attended with frequent efforts at vomiting. On the morning of the 27th, at 4 o'clock, we were again called to her assistance. We found her insensible, gasping as one in a moribund state, and the whole aspect of the countenance in correspondence. Her radial pulse, however, had all the strength and fulness it had possessed at any time for many months, and of natural frequency. In ten minutes after, it became extinct, and in two or three more the last spark of life had vanished.

We examined the body about twelve hours after death, assisted by Drs. J. K. Rodgers, Cockcroft, and Willett. Being most interested with the brain, we examined this organ first. The scalp was almost bloodless. Dura mater natural. Veins of the pia mater everywhere *turgid* on the upper surface of the brain, *but in no other part.* Arterial capillaries perfectly natural. *Plexus choroides bloodless and pale.* Longitudinal sinus contained scarcely any blood, nor was there more in the lateral. No serous or other effusion. Substance of the cerebrum and cerebellum preternaturally hard, especially the former. Wherever the cerebrum was divided, its medullary part became studded with dots of blood. Not so with the cerebellum, whose veins were also in a natural state. No other alteration could be discovered. Everything natural in the medulla oblongata and its vicinity. We turned our attention to the pituitary gland, which was remarkably small; the sella turcica being only one-tenth of an inch in width, and one-tenth in depth. The anterior and posterior clinoid processes were separate.

We then laid open the whole tract of the spinal column, and obtained a perfect view of the spinal marrow and its sheath, in situ. All the vessels, and everything appertaining to these parts, were considered to be in a perfectly natural state, as were all the diverging nerves.

Our comments upon the foregoing case will only be in relation to our specific subject of inquiry. It is manifest that the venous congestion was of very recent origin, and that it sprung up as the result of sympathy with the late sudden development of gastric and other abdominal disturbance, connected with a predisposition to venous disease in the brain. This predisposition was incident upon that chronic condition of the cerebrum which had resulted in its partial hardening, and which, we apprehend, had been the cause of the paralytic affection. The congestion contributed largely as an immediate cause of the sudden termination of life.

The points to be considered are not susceptible of any interpretation by any part of the mechanical philosophy of venous congestion; but are perfectly intelligible upon our doctrine, which also explains the rapid supervention of death, when connected with the fundamental disease of the brain. It was a remark of Dr. Willett, that "had the same turgescence of the veins of the pia mater occurred in a child, it would have been considered a sufficient cause of death." We believe that the habitual affection of the brain was aggravated by the abdominal disease; but more so by the venous congestion, and that, had not the latter supervened on the former, the patient would have been restored to her preceding condition.

The relative points to be considered are,

1st. The injected state of the veins of the upper surface of the pia mater.

2d. The limitation of the injection to those veins.

3d. The absence of blood from the great sinuses of the dura mater.

4th. The bloodless state of the plexus choroides.

5th. The perfectly natural state of the arterial capillaries of the pia mater.

Not only is the whole doctrine of obstruction and remora contradicted by the foregoing facts, but, also, the hypothesis of Dr. Clutterbuck that the venous injection depends upon a contraction of the capillary arteries after death. (See APPENDIX I.)

And who, on looking at all the facts, will content himself with the unintelligible doctrine of simple mechanical relaxation?

We may advert especially to the bloodless state of the plexus choroides, which induced Dr. Rogers to remark, that "it was strange the plexus should be sometimes so bloodless, when other veins are so much injected." It is said by Sir C. Bell,

"If, but a temporary change take place in the circulation of blood in the brain, it will, upon dissection, be manifested in the state of fulness of those vessels." (1)

Such a fulness may not be in any respect the result of a morbid state of the venous tissue, but depend upon causes which are set forth in the paragraph immediately preceding this case. How certain, then, is it that the plexus choroides, so complicated with the venous system of the brain, would always be turgid with blood in cerebral congestions, did the phenomenon depend upon any species of obstruction to the circulation. And, how fully does this case establish the fact that the turgescence of the superficial veins of the pia mater was in no respect owing to "a temporary change in the circulation of the blood in the brain." And may we not add, that the absence of blood from that congeries of vessels is a beautiful demonstration of great laws in physiology and pathology, and that what may seem unimportant where the mind is not bent upon a specific inquiry, may unfold the most fundamental arcana of nature, with as much certainty as the fall of an apple forever indicates the law which suspends in their places the heavenly orbs, and directs their progressive movements? (Vol. I. p. 635.) But, should not the force of this consideration in respect to the plexus be admitted, we then bring to its aid the void condition of the great sinuses of the dura mater, from which scarcely a drop of blood flowed. And how well does the latter fact, in connection with the injected state of the superior veins of the pia mater, illustrate what we have said in respect to the retention of blood in congested veins of the brain, whilst it escapes when the rope of the hangman is loosened. (P. 255 — 257.)

We have met with many facts more or less parallel, and shall occasionally advert to others from sources which may be more acceptable. We would also refer the reader to a well authenticated case of our own, at page 364 of our first volume. (2)

(1) Anatomy of the Brain.

(2) The foregoing statement has been submitted to Dr. Willett.

SECTION IV.

INDUCTIONS FROM THE EFFECTS OF REMEDIES IN VENOUS CONGESTION.

From what we have now shown, it appears that the treatment of venous congestion has not proceeded upon any well grounded principles. It is the more remarkable, therefore, that unaided experience should have frequently arrived, with so much accuracy, at the methodus medendi. If a knowledge of the true pathology of congestion should fail of improving the best plan of treatment, it will at least render it more general, and guide us with greater confidence, prompt the application of remedies which might be often contraindicated by sympathetic developments, give its proper interpretation to the doctrine of debility, arrest the hypothetical practice, and afford the never failing satisfaction which attends our imperfect knowledge of the methodus operandi of physical agents. "Without principles, the practice of medicine is mere empyricism. A man should have a satisfactory reason for every medicine he prescribes."(1)

Should it sufficiently appear that the phenomena of venous congestion are primarily dependent on a disease of the venous coats, and that, too, of an inflammatory nature, the philosophy of bloodletting in this affection will become intelligible. Practice will be no longer empyrical; but appropriate remedies will be equally suggested by general principles. The physician, thus relying on the agency of the vital forces, feels that he may be contending with a foe upon equal ground,—is cheered on by the consciousness that the enemy may feel his weapons. His frequent triumph, in cases of great prostration, is the best evidence that his premises are right, and the effects of his bloodletting, cathartics, mercury, and other "debilitants," assure him, also, that "atonic congestion" is but a verbal distinction. But what hope can animate him, who sees nothing but the intangible operation of mechanical forces, or the elements breaking up by the reckless might of the chemical laws? And where debility is assumed as the paramount guide, bark and wine are the never ending scourge, and death the penalty.

(1) Dr. Armstrong's Letter to Dr. Boot. See Memoirs, &c. vol. i. p. 45.

"Observe," says Bichat, "how ignorant many are in medicine. Perceiving by the touch, a hardness of the liver, and employing the vague and insignificant word *obstruction*, aperients, the acetate of potash, &c. are the common means which they oppose to hydatids, to steatomatous tumors, to scirrhi with granulations like marble, to fatty livers, and to a hundred different alterations from which the increase of size may arise, as if it was *this increase*, and not the *disease*, which they have to combat." (1) (P. 221.)

The best observers, as we have shown in our Essay on Bloodletting, rely mainly upon this remedy in the worst forms of congestive fever. And there, too, we have endeavoured to explain why it happens that the abstraction of blood is often borne to a great extent in venous congestions of the brain; not only for the purpose of illustrating the philosophy of the operation of loss of blood, but to show by the tolerance of its loss, in *cerebral* congestions, the validity of our pathological theory. We must refer particularly to the 5th, 7th, and 9th sections of that Essay for much that is intimately connected with our present inquiry. We have there, also, stated in part, as we shall more circumstantially thereafter, the causes which modify the general pathological condition of venous congestions, and how they are liable, as in common inflammations, to modify the general principles of treatment. Yet, however sometimes varied may be the necessary curative means, it will be seen that general principles still operate, and divest the treatment of its usual empyrical character.

The present theories of venous congestion render it impossible to understand how the removal of a few ounces of blood should speedily effect the contraction of a large extent of veins that are supposed to be mechanically obstructed, or to exist in a state of great passive dilatation. The wonder is, that such a remedy should have been devised to give tone to debilitated vessels. In common inflammations, the capillaries may be relieved by reducing the force of the vis a tergo; and this, we apprehend, is the only intelligible ground for the doctrine of stagnation. But, it is manifest that no such principle can operate in the large veins when the seat of congestion. Here the force of the circulation may be depressed by the disease, whilst relief will follow its exaltation by bloodletting.

If, however, we suppose the pathological cause of the dilatation of the veins to consist in a low state of inflammatory action in the venous coats, or only approximating that mode of disease, we want only the common analogies supplied by the ordinary

(1) General Anatomy, &c. vol. iii. p. 237.

phenomena of bloodletting in the latter affection to appreciate the manner in which the contraction is effected. So obvious is the control of that remedy over congestions, that many intelligent observers, who adopt the mechanical construction, institute a parallel between its results in this affection and inflammation. Thus, Dr. Wardrop:

"The observations," he says, "I have hitherto made on the curative effects of bloodletting, apply chiefly to the employment of it in the treatment of diseases, of an inflammatory character. But there is another class of cases (venous congestion) wherein the abstraction of blood is an equally powerful remedy," &c. (1) (p. 227.)

Leeching, as we have endeavoured to show, (Vol. I. p. 134—156) operates in a manner peculiar to itself; and its results are often so similar in venous congestion to those in inflammation, that many practitioners prefer it to general bloodletting in the former affection. (2) How, then, are the injected veins of the brain unloaded by leeches applied between the shoulders, and that too, when only an ounce or two of blood may have been abstracted? The only intelligible solution appears to be that of their subduing some inflammatory action on which the enlargement depends. We have also seen, under our remarks on Bloodletting, that it is not the diminution of the volume of blood which produces the great contraction that takes place in vessels morbidly distended; but, that it is owing in the arterial system to a specific impression upon the vires vitæ. In inflammation, however, the principal contraction affects only the capillary vessels, and those alone are the immediate instruments of disease. But it is otherwise in venous congestion. Although the capillary veins are probably involved in the morbid process, their larger trunks are most obviously the seat of disease and plethora. Analogy, therefore, is opposed to the supposition that the influences of bloodletting operate in the same way in producing their contraction as in the capillary arteries. And since the latter do not appear to contract so much from the diminished volume of blood, as from their vital actions, so must we believe that the contraction of congested veins can only be established by removing some cause from their parietes which will enable them to obey their

(1) On Bloodletting, p. 61.

(2) Dr. Wardrop, for instance, depends more upon leeching; and whether the venous plethora be seated in the head, or thoracic, or chylopoietic, or uterine systems, "it is essentially relieved by the application of leeches to the *feet*, whatever theory or explanation may be given." — *On Bloodletting, pp.* 64, 65.

natural law. In venous congestion, too, not only the capillary arteries, universally, but, in a lesser degree, their larger trunks, are often in a state of morbid contraction. Bloodletting removes that contraction; and although there is thus actually determined upon the congested veins a greater quantity of blood, there is a simultaneous diminution of their volume. The whole venous system, with the exception of the congested portion, and that about the right cavities of the heart, is, also, in numerous cases, in a state of contraction. Its dilatation, from bloodletting, is partly the result of the greater volume of blood determined upon it; and in this respect the congested veins can enjoy no exemption. We thus see presented the remarkable phenomenon of one, and the principal, portion of the veins undergoing a dilatation, whilst the morbid portion suffers contraction, from the common influence of the same agent.

Will it be still said that the rising force of the circulation overcomes the congested state of the veins? If congestion depend on their relaxation, and they be distended beyond their power of contraction, how is the increased volume of blood determined upon them, and with an augmented force, to remove such condition?

Dr. Armstrong frequently adverts to this principle.

"When sufficient evacuations, "he says," have been made, certain degrees of venous congestion may remain, partly from *want of power in the heart and arteries;* and the judicious administration of stimulants may then contribute to communicate that power, and thus, in the end, to equalize the circulation." (1) (Vol. I. p. 222.)

So far, indeed, does he carry this mechanical doctrine of the modus operandi of remedies, and so purely physical is the pathology of venous congestion, that he affirms that "early *depletion* itself is to be considered a *stimulus*."

Let us suppose a case of simple morbid congestion of the veins, in which the action of the heart and arteries has become prostrated in consequence. There will exist the local or morbid accumulations of blood, and that mechanical accumulation about the centre which has been determined by the enfeebled action of the heart, and by the contraction of the veins at the circumference. Admitting all that is required by the hypothesis as to the vis a tergo in carrying on venous circulation, (which, by the way, was rejected by Dr. Armstrong, though of course inconsis-

(1) On Typhus Fever, p. 119.

tently with the hypothesis we are now examining,) it may be asked, how is the heart roused into action by bloodletting? Clearly, through the same medium in which its action had become enfeebled. The direct impression of the remedy is upon the arterial and venous capillaries, and the reviving action of the heart, as we have endeavoured to show in our essay on Bloodletting, is mainly a sympathetic consequence, however the result may be promoted by the removal of the capillary contraction. This contraction was vital, and, in connection with the depressing effect of venous inflammation upon which it depended, threw its prostrating influence over the heart; just as happens when the somewhat analogous change affects the capillaries when syncope from loss of blood is about to supervene. The contraction of the capillaries, however, in the former case is pathologically different, and bloodletting, perhaps even to syncope, is the remedy. That condition of the capillaries is thus overcome by the suddenness of the impression upon their vires vitæ, which the loss of two or three ounces of blood may effect. The influence of this change is communicated to the heart, which, at every pulsation, is stimulated to greater action by the increasing change in the capillaries, and the greater quantity of blood which is transmitted to the left cavities. When the contraction of the capillary vessels is thus released, and their morbid influences overcome, the heart performs its office of distributing the blood according to the quantity received. The whole is a vital process. Even the removal of the capillary contraction is in no respect the result of the rising force of the circulation, the former being progressively antecedent to the latter.

These changes, however, only relate to the interrupted equilibrium of the general circulation. The heart may now go on to acquire a preternatural energy, and still the local congestion, which has been the source of all the mischief, may remain apparently undiminished. This is conceded by Dr. Armstrong, the best master of the subject. It remains, then, to carry depletion still farther, before we can reach the foundation of the primary and *real* congestion. In doing this, the force of the circulation is again subdued, and the congestion yields only after the pulse is rendered soft and unresisting. It may be even necessary, at last, to contend by leeching, vesication, alteratives, &c.; or still the affection of the veins may persist and reproduce the former phenomena, or result in common inflammation; thus affording

the clearest demonstration that the removal of venous congestion has no dependence on the vis a tergo, and therefore, too, that stimulants are even unphilosophical as a *mechanical* remedy. The brain supplies us with frequent examples of the foregoing nature.

Sometimes the amount of disease is so great and so irresistible in the congested organs, that its influence on the capillary system cannot be overcome by depletion, especially where constitutional causes, as in miasmatic fevers, cöoperate with the local affection. The vital forces are also in a state of great prostration. Bloodletting, particularly by leeches, may now tend to establish the contraction of the capillaries, and thus aggravate the whole condition of disease. (Vol. I. pp. 126, 130, 142—145.) This is especially seen in complex, and specific affections, as scarlet fever, measles, congestive typhus, &c., constituting cases in which the vital forces must be sometimes increased, or a power to bear the loss of blood must be produced, by cautious stimulation. (Vol. I. p. 222.)

In every sense, the foregoing considerations appear to us of the highest practical moment. The equilibrium of the general circulation may be restored without our having removed the local congestions; and in the delusive hope that our object is attained, the increased action of the heart may be permitted to go on, to accomplish the great final purpose of driving the blood out of the relaxed, or obstructed veins; and, perhaps, by way of giving it a farther impulse, the stimulus of wine may be added to the "stimulus of bloodletting;" whilst the practitioner may only be convinced that his philosophy is wrong when it is too late to repair the mischief by "stimulating" by another bloodletting. (P. 271.) Examples are everywhere before us of these doings, and often with the neglect of bloodletting, in the treatment of congestive fevers, where great prostration is so frequently mistaken for real debility. Even Armstrong relies principally upon stimulants in the worst forms of congestive typhus, where, in its early stages, bloodletting is most essential, however stimulants may be ultimately necessary to renovate the vires vitæ. (Vol. I. p. 222—232.

But venous congestion may be attended as well by an exalted as a prostrated state of the circulation, and has been equally ascribed by Dr. Armstrong, and others, to these opposite conditions. And shall we go still farther, and conjecture that in one case

bloodletting relieves by increasing the force of the vis a tergo; but that, in the other its effect depends on an opposite principle? Or, is it not the only rational interpretation, that its modus operandi is the same in both the cases, and that it operates by subduing an inflammatory state of the veins?

From what has been said, therefore, it appears that the modus operandi of bloodletting, in removing that mechanical accumulation of blood which exists in every case of what is denominated collapse, is very different from its mode of operating in local venous congestions, although wholly vital in either instance.

We may here remark, that the contraction of the congested veins does not probably depend alone on the removal of morbid action from their parietes; but also upon the influence which is exerted by bloodletting in producing a contraction of their capillary system, and the law of continuous sympathy, which, we shall soon endeavour to show, predominates in the veins.

Again, in congestions of slow formation, and of long continuance, as frequently happen in the liver, however the subject may become enfeebled and emaciated, moderate bloodletting, especially leeches to the anus, and a non-stimulating diet, are the great curative means. (Vol. I. p. 135.) Nor can it be objected that the absence of violent symptoms, or such as distinguish venous congestions of more rapid formation, denote a difference in the pathological state of the veins, or the non-existence of a low state of venous inflammation. (P. 219.) The history of all chronic and disorganizing affections, shows us that the system may become so gradually adapted to their presence, as to undergo but slight influences. (P. 139, *note*.)

How do blisters operate in removing the supposed obstruction or relaxation of congested veins? Does not analogy suggest that they operate by removing some morbid condition of the veins on which their dilatation depends, and the probability that this condition is of an inflammatory nature? And here it is worthy of remark, that blisters are often as pernicious in venous congestions, especially of the brain, as in common inflammations, if applied before bloodletting and other remedies for the latter affection have been adopted.

How does mercury act in resolving the disease? "The power which calomel has in equalizing the circulation," says Dr. Armstrong, "is no where more conspicuously displayed than in diseases of a congestive character."(1) Does it remove obstruction

(1) On Congestive Typhus, p. 117.

where venous congestion is simply local, or does it invigorate a relaxed state of the vessels? Have we not some analogies in relation to its effects as manifested in common inflammation? (1)

We might extend these inquiries to cathartics, tartarized antimony, ipecacuanha, and other depressing agents, which are equally among the remedies for venous congestions and inflammations. We might inquire why abstinence from stimulants and stimulating food is generally necessary in acute and chronic congestions of the veins? How does it remove the supposed obstruction or strengthen the debilitated vessels, whilst every other part may be losing its tone? But, more than all, we may ask whether there may not be something inexplicable by the

(1) We have long since learnt, that there is no condition of disease which requires greater care in the use of mercurial preparations than acute venous congestions of the abdominal viscera. It may otherwise produce an uncontrollable irritation; and this is often true of muco-intestinal inflammation. If bloodletting be indicated, this should be always done before the exhibition of calomel. Evanson and Maunsell say, that "intestinal irritation by calomel is sometimes resorted to in the treatment of cerebral affections, as a source of counter-irritation." We think we may safely affirm when this is the case, that the cerebral affection is increased in consequence. The repetition of mercurial preparations is exceedingly hazardous in pernicious forms of venous congestions, (Sec. 12,) such as often prevail in hospitals, and especially in scarlet fever; where, also, we believe that irritating cathartics, and an indulgence in other food than farinaceous fluids, are important causes of the fatality of this disease. (Vol. i. p. 273.) If there be a tendency to sloughing in the mouth or throat, or about the anus, in any of these affections, it depends on a pernicious form of inflammation or venous congestion in the abdominal viscera, especially the intestinal mucous membrane, and mercurial preparations never fail to increase the whole condition of disease. Solid nitrate of silver, in its *purest* state, is the proper application to the sloughing part. It will generally arrest it at once.

Castor oil is generally the best cathartic to promote the operation of calomel, or to be given after calomel is discontinued, in congestive diseases of the abdominal viscera. The writers just quoted say of it, that "it has no effect in altering the secretions." (*a*) We consider this an important mistake. "In our Letters on the Cholera Asphyxia of New York," we endeavoured to call the attention of the Profession to the effect of castor oil in promoting the secretion of bile. We think it equal, and often superior in this respect to the mercurial preparations, especially after the use of the latter. It is sometimes capable of producing great irritation of the intestinal mucous membrane in excessive doses, though of a very different nature from that of calomel. In diseases of this climate, a careful regulation of the quantity is often important. In respect to calomel, where we once employed one, or five, or ten, or twenty grains at a dose, for its local effects, we now use the tenth or twentieth of a grain, or three, or five, or ten grains, in the ratios of the former series. And so of blue pill. We have learnt it in the school of adversity. Nevertheless, there are no remedies which we value more highly than calomel and blue pill. (See comments on the supposed effect of calomel in imparting a green colour to the bile, p. 111, (*note.*)

(*a*) Practical Treatise on the diseases of Children, pp. 69, 89.

physical doctrines in the singular fact, that the Peruvian bark and bloodletting will cure the congestions which attend pernicious intermittents, whilst the former will generally aggravate all other venous congestions; and whether there be not a remarkable and significant coincidence in the same relation of bark and bloodletting to the inflammations of other tissues, which accompany intermittents, whilst, as in the foregoing case, the tonic febrifuge is generally injurious in all other inflammations?

The argument thus derived from the treatment of venous congestion is often employed, as by Dr. Good for instance, to show the probability of "redundant rather than defective action in inflammation."(1) (p. 153—155.) But is this evidence as conclusive in inflammation as in venous congestion; since, if the former disease depend on a simply dilated, though not a passive, state of the capillaries, the relief may be owing to the diminished force of the vis a tergo, and to that contraction of the capillaries of which we have spoken in our Essay on Bloodletting?

SECTION V.

INDUCTIONS FROM THE CAUSES OF VENOUS CONGESTION.

"Common congestive fever proceeds from one set of common occasions. But, in order to the production of such a condition from the operation of these agents upon the body, a certain concurring state is generally necessary, which state we call *predisposition*." — ARMSTRONG. *Lectures on Acute and Chronic Diseases, vol. i. p.* 157. (See p. 243.)

If we consider farther the causes of simply local congestion of the veins, we shall find them, as in inflammations, consisting of such as make their impression on the vital properties. We see it influenced by climate, habits, constitution, &c. — constantly arising as an epidemic in some places, whilst it is rare in others.(2) It affects the robust far more frequently than the weak; high

(1) Study of Medicine, vol. ii. p. 204.

(2) For this reason its existence may be even denied by some. But this would be like the Prince, who, having always lived in a warm climate, and having constantly

livers, the sanguine, and especially tipplers, more than the temperate, or other constitutions.

"Too much animal food, too highly seasoned dishes," says Mr. Annesley, in speaking of venous congestion of the liver, "indolence and insufficient exercise in the open air, and more particularly the inordinate use of spirituous liquors among the lower class of Europeans, are the most powerful and frequent causes. In addition to these may be enumerated the influence of terrestrial exhalations and exposures to sudden chills, cold or wet; neglected dyspepsia, inattention to regularity of bowels; and previous attacks of intermittent and remittent fevers." (1)

To these causes he ascribes, in another place, inflammation of the liver. (2) They are severally laid down by Hippocrates, in many places, as the causes of venous congestions.

When venous congestion occurs in combination with constitutional fever, we find it associated with such as are equally attended by local inflammations, and which, like the general affection, are supposed to depend more or less on the agency of specific causes, as in typhus and intermittent fevers, the plague, &c. Or it often coëxists with affections of a strictly inflammatory character, as the measles, smallpox, scarlatina, &c.; or, if it supervene on chronic derangements, they are constantly such as predispose to common inflammation. Both affections are produced by the shock of a fall, or of a surgical operation, or by intense cold, indigestible food, mental emotions, &c., from their direct or indirect impression on the nervous system. Armstrong says, that surgical operations, and many other causes will produce either local venous congestions or inflammations. (P. 234.) A low or a high temperature, whether locally or generally applied, he regards as "irritants," and may produce either congestion or inflammation. (3) (See Appendix III. on *Cold.*) Do they operate mechanically in one case and vitally in the other?

Upon what principle is it that certain poisons will produce sometimes inflammation and at other times venous congestion? Why do others produce almost exclusively venous turgescence of the brain? Why are their developments more remarkably manifested in the veins of that organ than in any other part? Is it not that their action is peculiarly on the nervous system,

seen water in a fluid state, naturally concluded that there was no such thing as water becoming solid. (*a*)

(1) On the Diseases of India, &c. vol. i. p. 349. (2) Ibid. p. 437.

(3) Lectures on Acute and Chronic Diseases, vol. i. p. 147 — 157.

(*a*) Locke on the Understanding.

and, of course, more or less on its associate tissues, and that here the vital forces are especially liable to their impression? Can any mechanical principle resolve the phenomenon? Here, indeed, we find a remarkable coincidence among all the poisons which manifest an early impression on the nervous centre; as putrid animal matter, whether applied to wounds, injected into the circulation, or taken into the stomach; the natural and morbid animal secretions of a poisonous nature; the narcotic poisons, opium, hyosciamus, hydrocyanic acid, &c.; the narcotico-acrids, belladonna, tobacco, monkshood, hellebore, nux vomica, &c.; (1) many poisonous gases; certain fungi; alcohol, &c. We state at present only the general facts in relation to many causes, which will become the subjects of more particular consideration hereafter, especially in Section 12. And we may now say in relation to the anatomical facts which respect the foregoing poisons, that they are generally such, both as it regards the arterial and venous systems, as are said by Dr. Wardrop to be characteristic of stupor.

"In place," he observes, "of an increased supply of *arterial* blood, there is in *stupor* a congestion of *venous* blood within the head, a state wherein the veins are preternaturally distended." (2) (Pp. 225, 227, and Sec. 12, 14.)

The rapidity with which venous congestions often spring up is another important evidence in favour of our doctrine; and here we may find innumerable parallels in the history of common inflammations, arising, also, from common causes.

If we turn to phlebitis, or admitted inflammation of the veins, we shall find it equally depending on epidemic constitutions of the air, or occurring in a sporadic form, and induced by many of the causes which produce venous congestion, which we regard as a sub-acute modification, or sometimes, as will be seen, constituted by that morbid irritability alone which precedes the absolute invasion of inflammation. Dr. Lee, in speaking of phlebitis, observes that, —

(1) "It is probable," says Dr. Rush, "from the uniformly stimulating manner in which poisons of all kinds act upon the human body, that *bleeding* would be useful in obviating their baneful effects. Dr. J. Dorsey has lately proved its efficacy in the case of a child that was affected with convulsions, in consequence of eating the leaves of the Datura Stramonium." (*a*) Rush states the cases of four patients that were saved, after poisoning by opium, by extensiv bloodletting, and thinks it one of the best remedies. (See, also, our vol. i. p. 328.)

(2) On Diseases of the Heart, Part 1, p. 83.

(*a*) Medical Inquiries, vol. iv. p. 357.

"With regard to the nature of this inflammation, it is difficult to determine whether it be a common or specific kind. It certainly arises where the individuals are not exposed to the ordinary causes of inflammation, and it often rages as an epidemic, particularly in hospitals; and in this respect it resembles erysipelas, and hospital gangrene, and other specific inflammatory diseases, which are generally supposed to depend on a vitiated state of the atmosphere. Like these diseases, too, it ceases without any assignable cause," &c. Again, "sporadic cases of phlebitis are met with at all seasons of the year, and in all the different ranks of life, and are not less destructive than in their epidemic form." (1)

The same may be affirmed of venous congestions; and it is also important to observe, that this affection is often rife when uterine phlebitis prevails, and is more or less associated with that disease, particularly in the liver. As to the supposed specific nature of the inflammation, we apprehend its peculiarities depend greatly upon the peculiar constitution of the venous tissue, as will be noticed hereafter. (Sec. 8, 12.)

It is a common circumstance that congestions of different organs supervene in quick succession. This is partially true of phlebitis; and when they occur in connection, as in uterine phlebitis, hepatic congestion is apt to be antecedent, especially in the epidemic forms. This is also true of erysipelas, which there is some reason to believe may be a venous inflammation. (See APPENDIX IV.) Here, too, venous congestions of the brain are apt to supervene.

"There are cases," says M. Andral, "where *cerebral congestion* recognises for its cause neither intense febrile disturbance nor an inflammation; but where it is connected *with other congestions* which occur in different organs, and *which alternate one with another*." (2) He then cites two remarkable examples.

The foregoing observation of this acute observer is particularly important on account of his devotion to the humoral hypothesis. It not only illustrates the doctrine of sympathy, but has a special bearing upon the great question before us. It places the subject of venous congestion so clearly on vital grounds that we shall resume its consideration hereafter. We may now say, however, in respect to the cerebral lesion which occurs in erysipelas, that

"A surcharged state of the veins and sinuses of the brain, and a quantity of fluid beneath the arachnoid tunic, are the principal, if not the only recent changes observed in post mortem examinations." (3)

(1) On some of the most important Diseases of Women, p. 98.

(2) Medical Clinic on the Brain, p. 121. (See vol. i. p. 628.)

(3) Mr. Travers' Farther Inquiry into Constitutional Irritation, &c. p. 143.

Mr. T. refutes the opinion that "the vicinity of the brain and its membranes appears to explain the peculiar danger of erysipelas affecting the head." He "believes the sympathy to be independent of contiguity."—p. 144.

Bichat has well pointed out an analogous sympathy among the serous and mucous

Armstrong,(1) Alison, Louis, and others, think that these conditions are wholly inadequate to explain the phenomena. (P. 245, and Sec. 12.) This is evident enough, in the mechanical acceptation, from what we have said of ligatures on the jugular veins. We must resort to a diseased state of the veins themselves for any satisfactory interpretation. But whether erysipelas be constituted by common or venous inflammation, it will not affect our argument as to the physiological results.

Apoplexies are often owing, primarily, either to irritation of the stomach or to venous congestion of the liver; and the latter affection is not an unusual cause of pulmonary hemorrhage, which, as we shall endeavour to show, generally depends, immediately, upon venous congestion of the lungs. It is especially to venous congestions of the liver, connected with peculiar influences of remote causes, and the law of sympathy which predominates in the venous tissue, that we must ascribe the epidemic apoplexies as described by Hippocrates, (2) Morton, (3) Ramazzani, (4) Lancisi, (5) Baglivi, (6) Morgagni, (7) Forestus, (8) Hollerius, (9) Jacchinus, (10) Agathias, (11) F. Hoffman, (12) Werlhoff, (13) Bartholin, (14) Ephemerides Acad. Cæs. Leop. (15) Kramer, (16) Euvagrius (17) Nicephorus, (18) Alpinus, (19) Mistichelli, (20) Lommius, (21) Piso, (22) Lauter, (23) Torti, (24) Praxagoras and Asclepiades, (25) Sydenham, (26) Zimmerman, (27) Cleghorn, (28) Kéch, (29) Bailly, (30) the German authors, (31) &c.

We have made these references partly with a view to what we have said in our first Vol. p. 381. Dissections disclosed ve-

membranes; and Hodgkin, and others, have farther illustrated the subject. In our Essay on Bloodletting, we have endeavoured to indicate the remarkable nature of the sympathy which prevails throughout the circulatory system. So, too, where the vital forces are not strongly pronounced, as in the fibrous membranes when affected with rheumatic inflammation. What then may we not expect, of this nature, in the venous tissue where the vital forces are so clearly defined? (See Sec. 8, 10.)

(1) Lectures on Acute and Chronic Diseases, p. 256.

(2) Pop. 1, s. 1, 3; s. 2 and 3. Aph. s. 3, 16, &c. — (3) Pyretologia, pp. 33, 75. — (4) Constitut. Epidem. s. 54, &c. — (5) De Sub. Mort. pp. 69, 90, 95. — (6) Op. 499. — (7) L. 1, Ep. 3, s. 11, 13; Ep. 4, s. 12; Ep. 49, s. 30, 31. — (8) Obs. Med. t. i. l. 10, obs. 70. — (9) De Morb. Intern. t. i. c. 7. — (10) Com. in Rhazis, Morb. part. c. 9. — (11) De Bello Gothi. l. 2. — (12) Op. de Apoplex. — (13) De Febribus, pp. 6. 21, 106. — (14) Hist. Anat. — (15) Cent. 1, 2, App. pp. 4, 5. — (16) In Commerc. Nor. Art. 1, pp. 346, 459. *Says he could predict a prevalence of apoplexy from the barometer.* — (17) Eccles. Hist. l. 4, c. 29. — (18) Eccles. Hist. p. 235. — (19) Med. Egypt. — (20) Trat. dell' Apoples. l. 1, sezz. 3, pp. 70, 93 — 98. — (21) On Fever, pp. 190, 230. — (22) Select. Obs. et Concil. de præternera Morb. Ob. 173. — (23) Op. — (24) Therapeut. Special. ad Febres, &c. passim. — (25) Cæl. Aurel. de Morb. Acut. et Chron. l. 2, c. 10. — (26) Op. — (27) Account of in Switzerland, 1717, and Exp. in Med. vol. ii. — (28) Diseases of Minorca. p. 173. — (29) De Feb. soporos. — (30) Trait. Anat. Pathelog. — (31) Pictorius' Edition, in which is a remarkable example at Rome in 531.

nous congestions of the abdominal viscera and brain; the lungs and heart being comparatively natural in a majority of instances. And if we regard the fact that miasmatic fevers, attended by great visceral derangement of the abdomen, often prevailed simultaneously with the epidemic apoplexies, and were frequently associated together, as well also, the well known connection of sporadic apoplexy with gastric and hepatic derangements, we may safely conclude that the epidemic form was intimately connected with the latter conditions of disease. (Sec. 16.) The whole subject, therefore, involves so extensively the operation of sympathy, that the pathologist obtains from its investigation no little insight into the nature of venous congestion. It should be considered, also, that the vascular system of each organ partakes of the susceptibilities of the organ of which it forms an integral part; and it is doubtless for this reason that venous congestion of the brain is one of the most common results of abdominal derangements, and the more so if these affections consist in part of that disease. (See Sec. XIII.)

SECTION VI.

INDUCTIONS FROM THE SYMPTOMS.

If we next advert farther to the symptoms of venous congestion, they will be still found to sustain the foregoing conclusions. (P. 233—248.)

Hippocrates abounds with accurate accounts of the venous congestions of all the organic viscera. (P. 218.) We take the liver as an example, in its intense degrees of congestion, and where the disease presents the phenomena of inflammation. We exhibit an instance in which miasmatic causes have operated, and where the disease may exist in a local form, or become complicated with idiopathic fever. Thus:—

"*Morbus hepaticus.* Accidit *maxime* in *autumno in mutationibus anni.* Hæc igitur æger patitur. Dolor acutus ipsi in hepar incidit et sub ultimas costas, et in humerum, et in claviculam, et sub mammam. Et suffocatio fortis tenet. Et aliquando lividam bilem revomit, et rigor, et febris primis diebus debilior habet, et dum attingitur, hepar dolet, et color ipsius sublividus est, et cibi, quos prius comedebat, suffocant ipsum, et ingesti urunt, ac torquent ventram. Atque hæc quidem in principio patitur. Morbo vero progrediente, et febres remittunt

et a paucis cibis repletur. In solo autem hepate dolor restat, et ipse aliquando fortis, aliquando quietus. Quandoque vero acutus corripit, et sæpe derepente æger animam efflavit."

"*Alius* hepaticus morbus. Dolores quidem eodem modo premunt in hepar, et color differt a priore. Malicorii enim formam habet. In tempore vero æstatis maxime incidit. Fit autem a bubulæ carnis esu, et vini potu nimio. Hæc enim omnia isto tempore hepati sunt infestissima, et bilem maxime ad hepar adducunt. Hæc igitur æger patitur. Dolores acuti incidunt, et non deficiunt ulla hora, sed semper magis premunt. Aliquando etiam vomit bilem pallidam, et ubi vomuit, paulo melius sibi habere videtur. Si vero non vomuerit, bilis ad oculos pervenit, et fiunt valde pallidi. Et pedes intumescunt." "Si enim *inebriatus* fuerit intempestive, aut venere usus fuerit, aut aliud quid fecerit, quod commodum non est, hepar statim ipsi durum sit, et intumescit ac a dolore *pulsat*. Et si quid facere festinarit, derepente hepar et totum corpus dolet." (1)

"Quando autem conturbatus, et secretus sanguis foras non procedit, quandoque etiam sanguinem in venas corporis distribuunt, ut et *venæ repletæ doleant*, et tumores laxos producant." (2)

"Venæ autem violenter perfrigeratæ, ad pulmonem, et cor assiliunt, et cor tremore concutitur, ut præ hæc necessitate anhelationes incidant, et spiratio cervice erecta. Non enim spiritum suscipere potest, donec id, quod influxit, a sanguine superatum, et calefactum in venas fuerit diffusum. Deinde et tremor, et anhelatio sedatur; sedatur autem, prout copia ejus fuerit. Si enim amplius influxerit, tardius; sin minus, citius. Et siquidem densiores, ac crebriores fuerint defluxus, crebirus comitiali morbo corripitur; sin minus, rarius. Hæc itaque perpetitur homo, si ad pulmonem, ac cor fluxus processerit. Si vero in ventrem, profluvia alvi apprehendunt. Si vero ab his viis exclusus fuerit, et in venas, quas dixi, defluxum fecerit, mutus fit, et suffocatur, et spuma ex ore effluit, et dentes constringuntur, et manus convelluntur, et oculi distorquentur, et nihil sapiunt, aliquibus etiam stercus inferne secedit." (3)

Here is an example showing the enlargement of the liver which results from venous congestion, and its complication with fever; and that the popular phrase of the "*bile passing into the blood*," was held by Hippocrates "*to be consonant with the science*." (4) (Vol. I. p. 605.)

"Morbi *a bile* fiunt, *quum* bilis in hepar influxerit, et in capite constiterit. Hæc igitur æger patitur; *hepar intumescit*, et præ tumore ad septum transversum expanditur, et dolor statim in caput irruit, maxime vero in tempora et auribus non acute audit. Sæpe vero etiam oculis non videt, et horror, ac febris corripit. Atque hæc quidem in principio morbi ipsi fiunt, intermittentia quandoque valde, quandoque minus. Quanto autem magis morbus progressus fuerit, tanto amplior dolor in corpore existit." (5)

(1) Hip. de Intern. Affect. s. 2. ver. 189 — 293. (2) Ibid. de Nat. Pueri, v. 98.

(3) Ibid. de Morbo Sacro, v. 153 — 164.

(4) Compare his philosophy of hereditary diseases with the humoral rationale. Thus: —

"Sic etiam puer vivit de matre in uteris, et quali mater sanitate prædita est, talem etiam puer habet." — *De Nat. pueri*, v. 416. (Vol. i. p. 464 — 466.)

(5) Hip. de Intern. Affect. s. 3. v. 208 — 214.

Hippocrates dwells with great particularity, in numerous places, upon venous congestions of the liver, the brain, &c. Doubtless his attention was led to their investigation by his doctrine of the origin of the veins, and their supposed distribution from the liver.

"Radicatio venarum hepar. Radicatio arteriarum cor. Ex his aberrant in omnia sanguis." (1)

He had a perfect knowledge of the mutual influences of the various organs upon each other, (Vol. I. p. 655,) (2) and this, together with the foregoing consideration, gave him a clear conception of the manner in which congestions of the liver develope the same affection in the brain, and vice versa, and of the supervention of autumnal fevers upon hepatic congestions; upon all which subjects he is ample, and exact. Affections of remote parts often began in the liver:—

"Quoniam omne hujusmodi ab hepate est, et ex hoc sunt hepatici in his morbi, et morbi regii ab hepate ad subalbidum calorem vergunt, et qui ex aqua inter cutem, et ex pituita alba laborant." (3)

The lungs were often the seat of sympathetic affections, resulting in hæmoptysis:—

"Quicumque sanguinem spumosum spuunt, dextrum præcordium dolentes, de hepate spuunt, et multi pereunt." (4) (Sec. 16.)

And here is an example of modern hypothesis brought to bear upon venous congestion.

"Morbus hic fit a *pituita*, et *bile*, quum in venas confluxerint; *venæ autem hæ sanguine plenæ*. Si igitur quid alieni in ipsis præterlapsum fuerit, ægrotant." (5)

He had a full knowledge of the influence of the passions in the production of disease; and we speak of this fact to show that he had the usual understanding as to the manner in which the supposed venous congestions are produced by fear. Thus:

"*Quicumque vero fugit perterrefactus, sanguinem sisti significant.*" (6) (P. 234.)

(1) L. de Alimento, v. 71.

(2) Si vero harmoniam non assequunti fuerint, neque gravia acutis consona fiant, prima symphonia perit. Et si etiam secunda genceretur, propterea quod unus per omnia corruptus est, omnis tonus vanus est. — *Hip.* de *Dieta l. i. v.* 48.

(3) De Morb. Pop. l. 2, s. 1, v. 82. (4) Coac. Prænot. s. 2. v. 323.

(5) De Intern. Affect. s. 1, v. 414.

(6) Hippocrates evinces a far better knowledge of anatomy, healthy and morbid, than is commonly allowed. He describes, in a general manner, every organ of the human body. He distinguishes between affections of the brain and its membranes; and so of other parts. He constantly speaks of the anatomical conditions which result from disease.

We accidentally neglected speaking of Hippocrates in our summary notice of the vital principle, in our first volume, page 712. He defended and variously expound-

Dr. Boot has given a most faithful summary of Armstrong's account of the symptoms of venous congestion, as manifested at particular periods of its existence. It is drawn from life, and is not only applicable to the disease in its local forms, but is often witnessed when venous congestions are complicated with idiopathic fever. Thus:—

"When congestion takes place in the *liver and its associated veins*, it is indicated by a sense of weight, distension or pain in the hypochondriac region; by a short, irregular, anxious, but not difficult respiration; by nausea or vomiting of a colourless or bilious matter; by diarrhœa, the stools watery or sanguineous; sometimes there is constipation, the motions when elicited being light-coloured."

"The symptoms vary in proportion to the *degree* of venous congestion. In its *extreme* form, there is a deadly coldness over the whole surface, a small indistinct pulse, a weak respiration, complete prostration of strength and of sensibility, and a general oppression in the functions of the parts implicated. (Vol. I. p. 201.) In an *intermediate form*, the animal heat is only sensibly diminished at the extremities; the pulse is slow, heavy and oppressed; the respiration anxious and laborious; the muscular power weakened, but not prostrate, and the functions generally less disturbed. In the *milder* form, there is merely a sense of languor and lassitude; the patient is fatigued by exertion, complains of a sensation of chilliness, of a weight and uneasiness about the head and præcordia: the pulse is languid, and the countenance pale and dejected. These different modifications depend on different degrees of the same pathological condition, which is proved to exist by the concurring testimony of symptoms, of

ed this principle. It may be thought that his exposition is sometimes fanciful, especially in his Book 1st. de Diæta. But is it more so than some recent doctrines which we have examined in our essay upon that subject? The following are examples of his clear method of reasoning:

"Quisquis de his, quæ in somnis obveniunt, recte conjectat, is magnam habere vim ad omnia ipsa reperiet. Anima enim vigilat, et quum quidem corpori inservit, in multas partes distributa, non sui juris est, sed partem aliquam singulis corporis partibus, sive sensibus distribuit, auditui, visui, tactui, gressui, actioni, ac omni corporis facultati. Ipsa autem animæ cogitatio sui juris non est. Quum antem corpus quieseit, anima in motu est, et corporis partes perreptans, domum suam gubernat, et omnes corporis actiones ipsa perficit. Nam corpus dormiens non sentit; ipsa vero vigilans cognoscit, ac visibilia videt, et audibilia audit, vadit, tangit, tristatur, animadvertit. In summa quæcumque corporis, aut animæ munia, ea omnia anima ipsa in somno obit. *Quicumque igitur hæc recte judicare novit, magnam sapientiæ partem novit.*" (*a*)

"Semper autem anima et majora, et minora habens, suas ipsius partes circumambulat non oppositione, neque detractione partium indigens, verum augmento et decremento earum, quæ jam sunt, opus habens." (*b*)

"Consistere autem possunt inter se et sœmella, et masculus, propterea quod anima quidem *idem est in omnibus animatis. Corpus* autem uniuscujusque *differt. Anima* quidem igitur *semper similis* et in majore, et in minore. *Non enim alteratur neque per naturam, neque per necessitatem.*" (*c*)

(*a*) De Insomniis, v. 1—12. (*b*) De Diæta, l. 1, v. 116—118. (*c*) Ibid. v. 319—324.

the effect of remedies, and by an appeal to morbid anatomy. The appearances, on examination after death, are simple and conclusive; for more or less distinct traces of congestion are observable in *those parts* which are the most *intimately connected with the fullest development of the venous system;* and it is generally found attended by *effusions of mucus, serum, or of blood;* but *without* those concomitant effects which denote the existence of inflammation. In many cases, however, the traces of congestion after death are not observable, from the blood retiring from the capillaries into the larger vessels, *as occurs often in cases of common inflammation*, (see Sec. 7 and 9;) and this may be expected in proportion to the degree and duration of the congestion." (1)

Who has not witnessed this array of phenomena where he has seen nothing in other respects analogous to idiopathic fever? Who has not imagined obstructed circulation in some particular organ, or vitiated secretions, (p. 221,) or a mysterious sinking of life, as the pathological condition? But, can it be entertained that a mere local "obstruction to the circulation" will produce such appalling phenomena as are often witnessed in venous congestions? We have shown you that it will not. (P. 253—255.) When, however, we come to speak of the symptoms of undoubted venous inflammation, the paradox will vanish. (See Sec. 9.)

Nor can we so well advance the knowledge of this intricate subject, as by quoting, from Dr. Armstrong, his no less accurate account of the symptoms of typhus fever when complicated with venous congestions:—

"The attacks of the most dangerous forms of the congestive typhus are generally sudden, and marked by many remarkable symptoms:—an overpowering lassitude; feebleness of the lower limbs; deep pain, giddiness, or sense of weight in the encephalon; a dingy pallidness of the face; anxious breathing; damp, relaxed, or dry withered skin; and those peculiar conditions of the temperature which have been noticed above. The pulse is low, struggling, and variable; the stomach irritable; frequently there is an inability from the first to hold up the head; and the mind is more often affected with dulness, apprehension, or confusion, than with delirium. The whole appearance of the sick impresses the attentive practitioner with the idea that the system in general, and the brain in particular, are oppressed by some extraordinary load. Both the manner and look of the patients undergo early and great alterations. Sometimes they slowly drawl out their words, or utter them in a hasty and yet imperfect mode, like people who slightly stammer, when embarrassed; they not unfrequently seem as if stunned by a blow, half drunk, or lost in a reverie; at times have the bewildered aspect of persons suffering under the first shock of an overwhelming misfortune. The eye is occasionally glary and vacant, without redness; but at other times it is heavy, watery, and streaked with blood, as if from intoxication, or want of sleep. At the commencement, the

(1) Boot's Memoir of the Life and Medical Opinions of Dr. Armstrong, vol. i. p. 130—132.

pulse is often less altered, as to frequency, than might reasonably be expected; yet, in general it becomes very rapid towards the close. The tongue is usually little altered in the first stage, but, in the last, frequently rough, foul, and brown. The bowels are mostly *very torpid* in the beginning, and the stools procured, dark and scanty; whereas, in the *advanced* stage, the bowels are generally loose, and the stools copious and involuntary," &c. Then follows a circumstantial and very accurate account of the various characteristic symptoms which are apt to spring up in this affection. (1)

Dr. Armstrong is so accurate and copious beyond any other writer in all that relates to the symptoms of venous congestion, that we shall quote some of his general remarks from his last work upon this important subject. "The general symptoms" of venous congestion, especially when complicated with idiopathic fever, are,—

"1st. More or less reduction of the heat of the skin. 2d. More or less prostration of strength. 3d. Diminution or oppression of the heart's action. 4th. More or less lassitude or torpor. 5th. Disturbance in the functions of some important organs."

And as to "particular symptoms:"

"Suppose the *brain and its appendages* to be the seat of *venous congestion*, there are four symptoms of such a condition on which you may rely: 1st. Confusion; indifference or insensibility to surrounding objects; or giddiness, with pain. 2d. An intoxicated, a stunned, or an alarmed expression. 3d. A blanched state of the conjunctiva, with a watery appearance of the eye. 4th. Either a dilatation or a contraction of the pupil."

The third of the foregoing conditions is only applicable to the early stages of the disease. The eyes often become injected quite early, especially in congestive fevers of much severity; or in uncombined cerebral congestions, as constantly witnessed in children. The fourth condition is not generally remarkable till some time after a full development of the disease, unless in apoplectic affections; and then other causes are concerned. (Vol. I. p. 342, &c.)

"If the *spinal cord and its membranes* be the seat of venous congestion, you have the following symptoms: 1st. General convulsions or partial spasms. 2d. Wandering pains, or tenderness of the surface of the body."

This is all very true; but the first of these conditions is far more frequently witnessed as the effect of cerebral congestion, when the spinal cord may offer no mark of the disease; of which we have constant examples in the cerebral congestions of children, and in "adynamic" fevers.

"Suppose the *liver and its associate vessels to be* the seat of the congestion, what are the symptoms? 1st. Nausea, retching, or vomiting. 2d. Fulness,

(1) On Typhus Fever, Sec. 3, p. 49, &c.

with flatulence in the epigastric region. 3d. Some uneasy sense of load, tightness, fulness, or pain, in the region of the liver. 4th. Diarrhœa, or constipation.

The tongue is most frequently moist, and covered with a ropy saliva, under all the forms of common congestive fever."

"It usually happens, in common congestive fever, that *one part is most affected;* but sometimes different parts are *simultaneously* congested, and this will be known by the combination of the symptoms which I have mentioned above." (1)

The last statement can only be received in a general sense. Some of the symptoms may be wholly absent where they are supposed to denote the locality of the congestion, especially the first. The tongue, also, generally becomes more or less dry, and covered with a variable coating, whilst there is no common inflammation. The condition of the tongue as stated by Armstrong, however, is especially applicable where venous congestions are not complicated with idiopathic fever; and it appears to us that he has so represented this symptom from having confounded local venous congestions, and turgescence of the veins from mechanical causes, with congestive fever. (P. 231—243, 262, 273.)

In all the foregoing account of the symptoms, and in the facility with which venous congestions may be recognised in this or in that organ by the local symptoms alone, who does not see the imposing contrast betwixt these affections and the result of ligatures upon the veins, as we stated them in our second section?

We might cite other able observers to the foregoing effect; and had we space to go more extensively into their analysis of the symptoms which appertain especially to the congested organs, and this, too, whether the disease be local or complicated, it appears to us the mechanical doctrine should surrender upon this ground alone. Annesley, and Robert Jackson, have very graphic accounts of the congestive fevers of the East and West Indies. They trace their progress with systematic analysis from the incipient stages, where local venous congestions are often antecedent to the general explosion, through their various intricacies down to the fatal catastrophe; and, although the symptoms of local inflammation be strongly pronounced, venous congestion, and effusions of serum, or blood, are apt to be the only morbid appearances. Mr. Annesley, who distinguishes venous congestion of the liver with great accuracy, remarks that,—

"With respect to pain, oppression, weight, and aching about the region of the liver, the epigastrium, and under the shoulder-blades, although also charac-

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 160—164.

terizing inflammation of the substance of the liver, we think that they as frequently mark congestion of the organ, particularly when they supervene suddenly." (1)

So, also, Dr. Hope :—

"Congestion of the liver occasions symptoms very similar to those of chronic hepatitis, except that they are less obstinate." (2)

Professor Alison, in his entire chapter on "local congestion of blood," makes no distinction between the phenomena of venous and arterial plethora. (3) Sydenham and Gallup are excellent on congestion. (P. 231, *note.*)

It is in venous congestions of the brain that the most unequivocal phenomena are presented. Here we generally meet with the vital signs of inflammation, though often more subdued, whilst morbid anatomy may disclose only a turgescence of the veins. Dr. Mackintosh states the experience of the best observers: "It is a nice matter to discriminate between a case of purely venous congestion of the brain and one of inflammation." (4) Dr. Armstrong is copious upon the subject; and where only venous congestions were discovered in the brain, the vital signs had been those of strongly marked cerebral inflammation. Our modern clinics are full of examples; whilst, it is within the experience of all, that cerebral diseases have been regarded and treated as strictly inflammatory, where dissections have disclosed nothing but a preternatural fulness of the veins, accompanied frequently with effusions of blood, or bloody serum, or serum *per se*. Malcolmson, in speaking of the inflammatory symptoms of beriberi, remarks that, "when no decided inflammation could be detected, *distension* of the veins and *ecchymosed* spots have been observed on different parts of the intestine and stomach." (5) We shall adduce a great variety of illustrations, where the character of the symptoms is modified according to the nature of the remote causes.

The symptoms of venous congestion will constantly vary in their intensity according to the severity of the disease, the nature of the affected organ, constitutional influences, and especially according to the nature of the remote causes. Different varieties of miasmata, and specific poisons appear to induce, each one, certain peculiarities, depending probably as well upon their gen-

(1) On Diseases of India, &c., vol. i. p. 348.
(2) Principles and Illustrations of Morbid Anatomy, &c. p. 133.
(3) Outlines of Physiology and Pathology, c. 5.
(4) Elements of Pathology and Practice of Physic, vol. ii. p. 28.
(5) Essay on Beriberi, p. 222.

eral influence upon an organ or the system at large, as upon their specific impression upon the venous tissue. Age is also an element, and in tropical climates the symptoms are more prominent than in the temperate. In the latter regions, they are rarely so strongly pronounced as the symptoms of acute inflammation, though often as much so as is generally characteristic of chronic, or of sub-acute inflammation. (Sec. 9.) The symptoms are greater in proportion to the rapidity with which the congestion takes place, and *vice versa*, as in inflammations. When the access is slow the disease is exceedingly insidious, and perhaps a fatal extent of it, unattended by any constitutional malady, may exist before the subject is admonished of its presence. We have seen patients affected in this manner pursuing their ordinary avocations, and have the records before us. (P. 219.) There are other causes which contribute to modify the symptoms of venous congestion in their relation to inflammation, that will be noticed hereafter. A fundamental one is the constitution of the venous tissue itself.

Will it be said that any soreness and pain, that may be present, arise from the distension of the vessels? This is refuted by our objections to the mechanical doctrine, and would also appear improbable from the entire absence of those symptoms, in the great accumulations of blood where pressure operates on the principal trunks of the veins, as in cases of ligature to the jugular veins, &c. Nor do we suppose that the local symptoms of venous congestion appertain exclusively to the affected veins, but, as in inflammation, the irritation is propagated to the entire parenchyma of an organ. Simple distension of vessels is not a cause of pain. Pain, however, is often entirely wanting in venous congestions of some organs. It is commonly attendant upon the brain. But its absence has its frequent parallel in chronic inflammations, and even in those of an active nature.

"It is a fatal error in practice," says Dr. Armstrong, "to suppose that visceral inflammations are always denoted by excessive pain and other striking signs." "The most destructive inflammation in typhus fever may go on in the brain and bowels without pain." (1)

When this happens, Dr. Armstrong ascribes it to an imper-

(1) On Typhus Fever, p. 150, &c. So, also, Huxham, (*a*) Johnstone, (*b*) and others; and Fothergill (*c*) affirms the same of angina maligna, — a fact well known to all.

(*a*) On Fevers, p. 281. — (*b*) Epidemic of Kidderminster. — (*c*) Essay on, &c. p. 47.

fect oxygenation of the blood, which then operates as a narcotic.(1) This is precisely what often happens in venous congestions. But we think that we shall have assigned other grounds for the phenomenon. We may now say, however, that pain is frequently wanting in pneumonia, and other common inflammations, and especially so in phlebitis, and that it is the tendency of venous inflammation to render the system obtuse, not only in its sensibility, but in all the organic powers. It is for this reason that pneumonia supervening on typhus fever, or on delirium tremens, is often so indistinctly marked as to be wholly overlooked by tolerable practitioners. Nevertheless, characteristic local symptoms usually attend those affections, (and even the cerebral congestion which is induced by carbonic acid gas,)(2) but may not engage the attention of the patient himself.

It is the opinion of Mr. Douglass, that the visceral veins are more frequently the seat of inflammation than is commonly supposed. He has seen three cases in which the liver was concerned. In these, however, the inflammation was entirely more active than in ordinary congestion.(3) M. Andral,(4) and Dr. Dickeson,(5) and others, relate similar affections of the liver. The origin of these cases was obscure. M. Ribes believes that hepatic phlebitis is produced sympathetically by disease of the intestinal mucous membrane; and certainly we have ample proof that hepatic and cerebral congestions spring up in the same manner. We think, too, that this is not unfrequently true of jaundice. The liver may be often primarily affected, but as often sympathetically. (P. 279.) Dissections show a congested state of the hepatic veins,(6) and

"In many cases," says Dr. Stokes, "the intestinal mucous membrane has been so highly engorged so to be almost black."(7)

(1) Lectures, &c. vol. ii. p. 148. This opinion was suggested by Hunter, (*a*) and Bichat, (*b*) who call the venous blood a "sedative." Williams appears to adopt the same view; (*c*) and we have seen it, in our Humoral Pathology, the foundation of important theories. (See our vol. i. pp. 279 — 280, 420.)

(2) See Bichat's Physiol. Researches, p. 249. Roch. sur la Vie, &c. pp. 377, 454.

(3) On Phlebitis, &c. (4) Clinique Méd. t. ii. p. 307, &c.

(5) Med. Chir. Rev. vol. xxxi. p. 273.

(6) See Bright on Jaundice, in Guy's Hospital Reports, vol. i. p. 604.

(7) Theory and Practice of Medicine, p. 108.

(*a*) On the Blood, &c. p. 54, &c. (*b*) Rech. sur la Vie, &c. c. 6, p. 327 — 353; c. 8, p. 411.

(*c*) Lectures on Diseases of the Chest, lec. 9. p. 85.

Among the direct evidences of inflammatory action in simple venous congestions is a very frequent buffing and cupping of the blood, and this, too, where the action of the heart may be preternaturally slow, or the circulation prostrated. We are sensible that this has been denied by Armstrong. So it is said by Dr. Wardrop, in a systematic Essay on Bloodletting, that,

"There is usually no appearance of the buffy coat in blood removed from persons affected with violent inflammations until the latter stage of the disease, and at the very period when the further abstraction of blood would be pernicious." (1)

But in the instances to which Armstrong refers, the venous congestions have been complicated with idiopathic fever; a condition of disease which, it is well known, may greatly modify the character of the blood. Much also depends on the manner in which the blood is drawn. If it trickle from the arm, as is often the case in venous congestions, and especially in congestive fever, or the orifice be small, or the blood be received into a bowl, the inflammatory characteristics may be generally wanting, as they often are under such circumstances in common inflammations. But we have commonly remarked, in simple venous congestions, and in congestive fevers, that when the blood is received in a large stream into a wine-glass, as has been generally our custom, it has presented the foregoing appearances; whilst they have been often wanting, especially the cupping, in the larger vessel. Most practitioners, however, are familiar with the accuracy of our statement in certain instances of a purely congestive nature, as in purpura hemorrhagica, and in apoplectic affections. Lind, (2) Trotter, (3) Rouppe, (4) often found the blood buffed and cupped in the worst cases of scurvy, and this, too, according to Lind, near its fatal termination. Hamilton, (5) Malcolmson, (6) Christie, (7) and others, affirm the same of beriberi. (See Sec. 7.) These affections are especially distinguished by venous congestions. Even when typhus is attended by petechiæ, "we learn from experience," says Mills, "that the blood is commonly observed here, as in other inflammatory disorders, cupped, buffed, and tenacious." (8) In our

(1) On Bloodletting, p. 41. (2) On Scurvy, p. 512, &c.
(3) Obs. on the Scurvy, p. 75. &c. (4) De Morb. Navigantium, l. 2. c. 2.
(5) Eden. Med. Chir. Trans. vol. ii. p. 22, &c. (6) On Beriberi, p. 128.
(7) Hunter on Diseases of Lascars, *passim.*
(8) On the Utility of Bloodletting in Fevers, p. 6.

Essay on the Humoral Pathology, (p. 453–460,) we have quoted other observers to the same effect in respect to "putrid," and "adynamic" fevers. A very accurate observer states, as the result of his experience, "that the blood in adynamic fevers presents frequently, we might say generally, an inflamed surface." (1) Dr. Rush, after having bled four times in a case of "malignant fever," the weight of congestion was still so oppressive that the patient's

"Hands and feet were cold, and her countenance ghastly, as if she were in the last moments of life. She was then bled again, and her pulse rose. At the end of 3, 10, and 17 hours it fell, and rose again by three successive bleedings, The blood was always sizy. The pulse afterwards acquired such a degree of vigour as to render 7 more bleedings necessary to subdue it." (2) (Vol. I. pp. 224, 231.)

So also Southwood Smith,—the buff appearing especially after the circulation has been restored by the loss of blood. (Vol. I. p. 453—455.)(3) If there be an absence of buff during the depressed state of the circulation, in simple venous congestions, it is probably owing in part to the imperfect manner in which the blood flows from the orifice, as also to the modifying influence of the embarrassed and contracted capillaries and the oppressed condition of the heart; whilst, as we have said, when congestion is associated with constitutional fever, the state of the blood is more or less modified by the latter affection. (Vol. I. pp. 195—196, 405—

(1) Med. Chir. Rev. of Burnie on Adynamic Fever.

(2) Med. Inquiries and Observations, vol. iv. p. 298. — Rush then comments on the fatal practice of attempting to raise a pulse by stimulants that had been depressed by bleeding "in malignant fever." They may be necessary, where great depression occurs.

We add a case from Sydenham precisely parallel with the foregoing, not only to confirm the principles involved, but to show how genius, directed by an observation of nature, travels a common road. We give it in the language of Southwood Smith. "Having described," says Smith, "in his own powerful manner, an *excellent specimen of congestive fever* to which he was called, he states that he ordered the patient to be bled; that the bystanders regarded the suggestion with horror; (Vol. i. p. 292,) that the man seemed at the point of death; that to them it appeared that the abstraction of blood must inevitably extinguish the last remaining spark of life; whilst to him it was manifest that the patient was oppressed by an overwhelming load that demanded the abstraction of blood. Accordingly on the removal of some ounces, the state of oppression ceased at once, and a fever arose of a true inflammatory nature, for the subdual of which repeated bleedings were required."— *Ut cit.*

Put this and Rush's case together, and what becomes of the parallel drawn by Armstrong between such cases and the collapse from syncope, the shock of accidents, &c. whether in respect to the philosophy of their vital phenomena, or their treatment? (*See following note.*) As to the ultimate "inflammatory fever" in the foregoing cases, we shall show how readily venous congestion passes into inflammation.

(3) On Fever, p. 96.

407.) This, however, is not common in either case; the blood being generally more or less buffed and cupped from the beginning, if abstracted in the manner we have suggested. On the other hand, the buff never appears in those simple determinations of blood upon the right cavities of the heart, which arise solely from idiopathic fever, from the shock of a fall, or of other injuries, and where no local congestions exist of the nature we are considering. (P. 234.)(1)

But the buff may be absent in some fevers when attended by disorganizing inflammation, as in typhus and yellow fevers. In some of these instances, the blood will be so influenced by the action of the capillary vessels at large, that it will not even coagulate; and this very phenomenon has been assumed as a ground for the humoral pathology. But it also happens, in cases of congestive fever, where the blood had been "dissolved," that it afterwards became sizy, as stated by Rush,(2) Mackintosh,(3) &c. The same fact has been observed in acute phlebitis, or the buff may not appear. In sub-acute inflammations, also, the

(1) It is from a want of discrimination between these conditions that we find Dr. Armstrong, and others, depending wholly upon diffusible stimulants in the treatment of "extreme forms of congestive fever." (*a*) The vital phenomena are overlooked, and conclusions, founded on a supposed analogy in the anatomical appearances, take the place of inductions from vital signs and the light of experience. In both cases there exist a state of depression, and an accumulation of blood about the right cavities of the heart. In falls, &c. stimulants may be more important than bloodletting; therefore the philosophy is, that they are equally so in "extreme forms of congestive fever." For a like reason, bloodletting is not adapted to the latter case, because it would be pernicious in common syncope. Moderate stimulation may be important where reaction does not take place in bad forms of congestive fever; but their are instances, as we have shown, by the records of experience, in our Essay on Bloodletting, in which the loss of blood is most imperatively demanded; though certainly with all the precaution which is so well recommended by Dr. A. in the "intermittent form of congestive fever." An important difference in these examples consists in the relative states of the great capillary system; in the different modes in which their powers and functions are affected. In one class there is a simple depression of the vital powers, and a contraction of the capillary vessels, where nothing is wanted but a stimulant to rouse the action of the heart, and otherwise excite the vessels. In the other, those powers, and those vessels are most profoundly involved in a morbid process. In the latter, there are generally, also, true venous congestions which equally demand a direct application of the lancet; and it is often that the prostration of the circulatory organs is maintained by these local congestions. As well might we compare the redness in blushing with common inflammation, or erysipelas, or scarlatina. And how constantly is the remark justified, that "bleeding, like opium, has lost its character, in many cases, by being prescribed for the name of a disease?" *See last note.*

(2) Med. Inquiries, &c. vol. iv. (3) Elements of Pathol. &c. vol. i. p. 83.

(*a*) Lectures, &c. vol. i. p. 172, &c.

blood is often only slightly buffed, less so than is usual in venous congestions, and not unfrequently presents a layer of reddish colour. (1)

From what is known of the influence of the general circulatory organs in modifying the inflammatory buff, it would be likely to abound most, and be most readily formed, where those organs are most extensively and actively excited. It is therefore remarkable that this phenomenon should be so often and fully developed in simple venous congestions and congestive fevers; since, in all severe cases, as well also in acute phlebitis, there is a greater depression of the circulation and of the vital forces than in inflammations of other tissues. In proportion as this depression exists, the vitality of the blood is also impaired. (2)

As it respects the state of the pulse in venous congestions, a great variety is occurring; far more so than in any other condition of disease. This is especially true, if the congestion be complicated with idiopathic fever. Armstrong's general description is often true, but is affected by the mechanical doctrine. Thus: —

"A small, weak, irregular pulse; or, an oppressed pulse."

(1) Gendrin, Hist. Anatom. des Inflam. t. ii. p. 430.

(2) From what has been now said, it appears probable that inflammatory buff is not so much imparted by the action of the diseased organ, as by a modified state of the whole vascular and nervous systems, which is superinduced by the local affection. This also appears from the effect of bloodletting in often removing immediately this peculiar condition of the blood, since it cannot be entertained that the whole mass of blood is thus impressed by any alteration that may be made in the instruments of morbid action. It is therefore a philosophical conclusion, founded also upon principles that relate to the formation of blood, that so far as the mutation is owing in one case to the system at large, it is equally so in the other. (Vol. i. p. 642, *note*.)

This leads us also to comprehend how far the blood may be variously modified when idiopathic fever affects the entire organization, and how it may counteract the tendency of any coëxisting inflammation or venous congestions to establish their particular influences. We learn thus, how idiopathic fever may more effectually prevent the formation of buff, when complicated with acute inflammations, than the depressing nature of uncombined venous congestion shall modify in like manner the constitutional influences which grow, abstractedly, out of its pathological cause. And yet again, such are these complex influences, such the depressing influence of venous inflammation, that, in simple venous congestions, and even when connected with common inflammation, (though not complicated with idiopathic fever,) the effect of inflammatory action shall be counteracted as it regards that change in the movements of organization which dispose the blood to throw up the buffy coat.

We shall not enter into a consideration of the relative appearances of the blood in pregnancy, or the buff which is sometimes apparently the result of a simply accelerated state of the general circulation. As to the latter fact we believe it to be entirely overrated; and it is stated by Sir C. Scudamore, in which others agree, that he has

The former condition is particularly applicable to the stage of depression; the latter, with various modifications, to other general states of the system. Armstrong thus explains it:

"By an oppressed pulse, I mean that the stroke is protracted more than natural, or that it is made with a more laborious jerk than natural. It is a kind of jerk that you will very easily recognise when you have once felt it." (1)

But, it should be constantly kept in mind, that this justly paramount authority makes no distinction between that state of collapse which follows the shock of falls, &c. and the cold stage of idiopathic fever, nor between the local congestions which may spring up in the former case, and the idiopathic fever which may be complicated with congestions; nor, indeed, between typhus fever, and the consequences of dissection wounds and injections of putrid animal matter into the veins. (Sec. 2 and 12.) Taking the same liberty with our author which he so freely, and even harshly exercised towards all others where he considered the right interpretation of nature obscured by ignorance or prejudice, we will not, upon the same principle, neglect saying that the foregoing confusion of perfectly different conditions, (some of them, also, scarcely of a morbid nature,) in leading our author to rely upon stimulants, in those truly congestive diseases where bloodletting is most demanded, (p. 293, *note*, and Vol. I. pp. 221 — 226, 254 — 256, 290, 302, 312,) is one of those common, and important demonstrations which we have noticed, of the irresistible tendency of theory to determine the most momentous con-

not observed the buff in cases of simple fever, or after violent exercise. (*a*) In all the instances in point, we believe that sufficient proof exists that the phenomenon, especially if accompanied by cupping of the blood, depends upon a modified state of action remotely allied to the inflammatory, or the presence of actual inflammation has been overlooked. (Vol. i. p. 622.) The subject as connected with pregnancy has been well investigated by Rush, (*b*) Parry, (*c*) Wardrop, (*d*) but we cannot suppose the action to be inflammatory; nor that Scudamore (*e*) has assigned the true cause. It should be borne in mind, too, that the buff in pregnancy differs in appearance from the true inflammatory crust, and is wanting in the important character of a central depression. As to the buff which is said to occur in some cases of "irritation from excessive loss of blood," we have fully considered that subject in our Essay on Bloodletting, Sec. 11.

Finally, M. Piorry, a very close observer, and with all the present facts before him, comes to the conclusion that the true buffy coat always denotes the existence of inflammation. (*f*)

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 161.

(*a*) See Elliotson's Lectures, Lon. Med. Gaz. Dec. 3, 1831; and Scudamore on the Blood, p. 147.
(*b*) Medical Inquiries, vol. iv. p. 350.
(*c*) Elements of Pathology.
(*d*) On the Diseases of the Heart, part 1, p. 70.
(*e*) On the Blood, p. 148.
(*f*) Trait. de Diagnost. et Semeiol. t. 1, s. 779, 1837.

cerns in medicine, and which is so eminently shown in the Brunonian, the Hahnemann, and the whole empyrical ranks. (Vol. I. p. 279 — 280, &c.)

There is a more general *hardness* of the pulse in simple forms of venous congestion than might be expected, when it is considered that this symptom relates to the arteries, and that it is only the vasa vasorum which are the immediate instruments of a low inflammation in congestive disease, whilst there is a tendency to a depression of general action, and not unfrequently a diminished frequency of the pulse. But even under these circumstances, hardness of the radial pulse is sometimes strongly pronounced, and may be distinguished in a greater number of instances after the general circulation becomes depressed, if the pulse be examined in the manner described in our first volume, page 235. It is more constantly attendant on simple venous congestions, than when idiopathic fever is complicated with them. It is often remarkably developed when the result of continuous sympathy, as in the throbbing carotids, in cases of venous congestion of the brain, and where the general circulation may be greatly prostrated. But the pulse, at large, never possesses the fulness and strength which distinguishes many common inflammations. In a majority of cases, however, there may be felt the *incompressibility* of the pulse as described by Dr. Wardrop, in his valuable work on Bloodletting, and which he considers as pathognomonic of inflammation.(1) Here our author's views are valuable to our purpose, since, as we have seen, (p. 227, &c.) his theory of venous congestion rests upon mechanical principles, though in direct opposition to his doctrine of incompressibility of the pulse. For our able author, in his nice observation of the phenomena of nature, states, that in a particular case of venous congestion,

"An incompressible feeling remained in the pulse until a large quantity of blood was abstracted." "There are cases of congestion in which bloodletting must be carried to a great extent," &c.

Our author is clearly of opinion that incompressibility of pulse is pathognomonic of venous congestion.(2) The symptom is often more fully developed by the loss of certain quantities of blood. (P. 292, and Vol. I. p. 224 — 227.) And here we shall add, to what we have formerly said, an example from Dr. Wardrop. Thus, in a case of venous congestion of the brain,

(1) "An incompressible state of the pulse may, I venture to say, be considered as pathognomonic of inflammation, in whatever organ or texture that inflammation exists." (*Ut infra*, p. 26.)

(2) On Bloodletting, pp. 26, 64, 65.

"There was so little deviation from the natural state of the pulse, that I even hesitated whether to take some blood by venesection or by cupping. The former mode being determined on, his pulse soon began to rise, and an *incompressible* feel remained in it until a large quantity of blood was abstracted." (1) The amount taken was 52 ounces. Dr. W. has other strong exemplifications.

This is one of the numerous cases which are occurring that denote the importance of seeking the pathology of disease through the entire combination of symptoms. (Vol. I. p. 233—239.)

Nor should we neglect saying that the pulse is generally much more compressible in venous congestions than in inflammations. This arises, in part, from the depressing influence of venous inflammation, whilst, on the other hand, the inflammatory action preserves, more or less, its disposition as in other tissues, to excite the pulse and render it incompressible. It therefore happens that every gradation of these conditions may occur, from the excited and incompressible pulse, to the comparatively soft and almost extinguished as in the cholera asphyxia. These conditions, too, will be considerably determined by the nature of the remote causes, and the latter especially by an accumulation of blood about the right cavities of the heart. When venous congestion is complicated with constitutional fever, the pulse may be variously modified. Thus, in the plague, according to Russel,

"Where the pulse was low, small, and quick, it sometimes seemed to disappear, as it were, on pressure, but not so when high and full." (2) So, also, Chisholm: "It is observable, that, in several cases of the malignant fever, the slightest pressure would produce a cessation of the pulse." (3)

But, it has been commonly found, that when the pathological condition of the veins has been partially relieved, or the mechanical determination of blood upon the heart has been taken off, by bloodletting, the strength and resistance of the pulse has been increased. (Vol. I. pp. 189, 222, 224, 225, 231, 229, 235, 238.)

It is well remarked by Dr. Wardrop, that "*each of the different textures of an organ, when inflamed, is attended by some peculiarity of the pulse.*" (4) So, of many other phenomena. It is one of the great modern discoveries in the healing art. We have now become familiar with the distinctive characteristics of acute phlebitis. Looking at another tissue, we find that—

"The phenomena of inflammation presented to us in the serous membranes differ from those which the same principle, or action, or deviation from the

(1) On Bloodletting, p. 65. (2) Treatise on Plague, p. 86.
(3) On Pestilential Fever of the West Indies, vol. i. p. 162. (4) Ut cit.

physiological state, presents in any other tissues or organs."(1) This is relatively more or less true of every distinct tissue.

We have already alluded to the strong pulsation which is often felt in the arteries leading to a congested organ, even when the general circulation is greatly prostrated. This is sometimes as strongly pronounced in venous congestions as in inflammations of the brain. (2) This fact probably involves what we have yet to say of the tendency of venous congestion to pass into common inflammation.

But, again, in respect to hardness of pulse, as well as incompressibility, their absence is not wanting in some very grave inflammations, (3) as we have already indicated, (Vol. I. p. 234,) and especially in sub-inflammations. In uterine phlebitis, the absence of hardness is as frequent, in the ratio of the number of cases, as in simple venous congestions.

Another remarkable coincidence not unfrequently happens in an intermitting state of the pulse, in all the lower grades of venous inflammation. It is often seen in venous congestions, especially of the liver. Mr. Ferrall, (4) and others, speak of it as a frequent attendant on varix; a disease which we shall endeavour to show is primarily constituted by inflammation. It probably arises from very complex sympathies originally determined by venous inflammation. (5) And here it is important to remark that, both hardness and incompressibility are always more or less affected by frequent intermissions or other considerable irregularities in the stroke of the pulse. Both may nearly disappear at the moment of the greatest change, and return as the pulsation becomes more steady.

(1) British and Foreign Med. Rev. No. 7. p. 34.

(2) So of the "epigastric pulsation;" a very common phenomenon in our abdominal congestions when the general circulation is prostrated. "Hepatic congestion seldom exists to any extent without epigastric pulsation." —*Med. Chir. Rev. No. 72, p. 569.* See, also, Hippocrates, de Morb. Pop. l. 4, ver. 129. Aretæus, de Morb. Acut. l. 2. c. 8. Baillie's works. Piorry, de l'Irritation Encéph. des Enfans, in Répert. Méd. Chir. Brux. Août, 1837. Faussett in Dublin Journ. July and Jan. 1837.

(3) Galen, (*a*) and Aretæus, (*b*) notice a soft pulse in Peripneumonia.

(4) On Varix, in Dublin Journ. Med. & Chem. Science, vol. ii. p. 212.

(5) It is a mysterious phenomenon, and appears to depend upon the cause we have assigned, when not owing to an organic affection of the heart. It often attends chronic congestions, and is sometimes very remarkable during convalescence from acute forms. There is then some remaining congestion, which is most apt to exist n some part of the liver. It is not, *per se*, a serious symptom. It generally contrain-

(*a*) De Puls. ad Tyrones, c. 12. "Pulsus undosus, et obscurus, et mollis." (*b*) De Caus. et Sig. Morb. Acut. l. 2, c. 1. "Pulsus habent in initio magnos inanes, creberrimos."

SECTION VII.

MORBID ANATOMY OF THE VEINS.

"The principles which I have taught, being true now, will be equally true hereafter, and will be transmitted when my name has been long forgotten; will remain essentially unaffected either by circumstances or by time." — ARMSTRONG'S *Lectures on Acute and Chronic Diseases, vol. ii p.* 162.

IT may be objected that dissections have not sufficiently illustrated the state of disease which we suppose to constitute venous congestion, upon the popular ground that special anatomists have not given much attention to this inquiry. There is a great amount, however, of scattered facts, either of a direct or indirect nature. These we shall not neglect, whilst we shall still proceed, in part, by induction from analogy, &c. to illustrate the truth of our doctrine. (1)

It is not our intention, at present, to advance any theory as to the intrinsic nature of inflammation, our principal object being to ascertain the pathological cause of venous congestion, and to place its treatment on rational ground. We shall go on to consider only what is admitted or proved, and aim at conclusions from acknowledged data. We may say, also, that the various facts which we shall bring from other authors were not originally intended to bear upon our subject. They are, therefore, the more entitled to confidence, as supplying the force of "internal evidence." For this reason, too, we shall say but little of our own experience. The most common phenomena, says Zimmer-

dicates stimulants and stimulating food. Venous congestion of the brain, like cerebral inflammation, often produces an *unequal*, as well as intermittent, or irregular pulse.

(1) We are sensible that certain philosophers are as little disposed to admit of inductions from analogy as they are intolerant of light from the vital phenomena. Nevertheless, the principle must continue to prevail, that medical science is mainly founded upon analogics, which is the only "rigorous fact" about it. "Analogy," says Bacon, "is the great chain of nature, and the basis of all the sciences;" but most of all, the science of disease. We have quoted other philosophers as avowing this obvious truth, at the head of this essay; and since it is held by many that medicine may be reduced to the exactness of mathematics, and analogy should be abandoned to the natural and moral philosophers, it may be worth while to show still farther the degree of importance which was once ascribed to this species of evidence, and how it contributed from time to time in rescuing physic and other sciences from the trammels of false philosophy. This we shall do in our APPENDIX II. on *Analogy*.

man, are worthy the most serious attention ; because it is from these we learn to generalize and establish our principles. He also adds, that the most common phenomena are the least generally known, because they are common :—

"As it is impossible to arrive at the knowledge of a whole, before we are acquainted with its parts, it will easily be conceived of how much consequence it will be not to neglect the least circumstance, even that which seems the most known. This known circumstance is, as it were, the *chain* that unites together the truths we are in search of. It draws us nearer to the unknown, and enables us to see nature more nearly. It is by means of little circumstances, also, that we are enabled to follow her through the mazes she so often pursues, and that we estimate the degrees of probability of the phenomena she presents to us." (1) (P. 267.)

This is our apology for any seeming prolixity, either here or elsewhere. In beginning, then, our consideration of morbid anatomy in its various relations to venous congestion, we may premise that the vital phenomena and the effects of remedies, with the aid of analogies, are generally a better ground of induction, in an abstract sense, than the ultimate results of disease, which are liable to so many modifications from adventitious causes.

It is important to bear in mind, that, in the disease under consideration, it is agreed by the best observers, that, whilst the veins are the seat of great injection, the communicating arteries, in a general sense, are apparently in their natural state ; some even affirming that they are preternaturally empty. (P. 225.) To this general condition, however, there are exceptions, which will be occasionally stated ; and in our 14th section we shall endeavour to ascertain the cause of the cöexistence of arterial plethora, and to show that it is a proof of our doctrine of the pathological cause of venous congestion.

We have already quoted many of the most distinguished observers as to the general condition of the veins in congestive diseases, (p. 224—247 ;) and in going into the details of the subject, it is eminently due to Dr. Armstrong that he should again open our inquiry. We therefore take from him the following general account of the morbid appearances in "congestive fevers."

"1st. An over-accumulation of venous and arterial blood in the part, the functions of which had been disturbed during life ; and *especially in the veins.*" 2d. An effusion of a serous or of a mucous fluid, according to the structure of the part. 3d. Sometimes an effusion of blood." (Pp. 224—232, 234, 239—241.)

(1) On Experience in Physick.

The two latter appearances we hold to be important evidences of the inflammatory and vital nature of venous congestion, the rationale of which will be transiently noticed, till we come to its specific investigation in our 16th Section; where, too, our author's doctrine of "transudation of blood" will be considered, as his sometimes parallel philosophy of "rupture," in cerebral hemorrhage, has been in our first volume, page 371. (1) Our author states next, more circumstantially, that, —

"1st. The *heart and large vessels* having been *the seat* of the congestion, will be found, especially the right side of the heart, far more crammed with venous blood than in ordinary examples of death."

Here, again, we stop for the purpose of saying that this statement is not warranted by facts. Take the disease which our author adduces at another time, the "Asiatic cholera," as a fine example of venous congestion, or the celebrated "typhoid fever" of Paris, in which venous congestions abounded, especially in the brain, or any "adynamic" fever, and, unless the patient die in the stage of collapse, (pp. 232, 247, *note*,) there is generally no remarkable accumulation of blood about the right cavities; but, we may say, it is often remarkably otherwise. The statement is purely hypothetical, and the reason of it may be found in what we have already said, and in our author's affirmation that "the heart and large vessels had been *the seat* of the congestion." It is a sequel to the doctrine of *remora*, by which the local congestions are partly explained by our author. (Sec. 2.) Again:

"2d. When the *lungs and bronchial lining* have been the seat of the congestion, the lungs are so gorged with blood, as to resemble the spleen in structure; the bronchial lining is overloaded with blood, and generally there is an effusion of *mucus* or of *serum*. Sometimes *blood* will be found in the bronchial passages."

This portrait has too much of the *rouge*, for a general statement. The condition of which our author speaks forms the exceptions to a far more common and less injected state of the lungs.

"3d. When the *brain and spinal cord, and their membranes*, are congested,

(1) We may add to what we have there said, that it appears to us remarkable, that since many of the best pathologists have come to the conclusion that hemorrhage in all other parts is the result of capillary action, they should not have carried, with the exception of Cheyne, Latour, and perhaps one or two others, this fact to cerebral hemorrhage, in the way of analogy. It is one of the numerous demonstrations of the importance of analogy in all medical science, and the fallacy of the times in an exclusive substitution of "rigorous facts" for fundamental principles, or hypothesis for the great laws of organized beings.

you find the pia mater loaded with venous blood; you find the sinuses loaded with blood, (p. 265—267;) you find the membrane of the spinal cord loaded with venous blood. This is accompanied generally with effusion of *serum between the membranes and into the ventricles* of the brain; and there will sometimes be *blood in the ventricles or between the membranes, and sometimes in the substance of the brain.*"

This statement is true to nature, in a majority of cases, whether in simple venous congestion, or when complicated with idiopathic fever; but so much the worse for the mechanical doctrines. We think, however, in a general sense, the language is too emphatic, and may lead to false conclusions in many cases of fever, and even of congestive apoplexy, where the venous turgescence may be less strongly pronounced. Then follows a remark which we would earnestly commend to morbid anatomists, and to such as believe that effusions of serum, &c. depend sometimes on inflammation, and sometimes on an "opposite condition," and sometimes on a mere process of filtration. (See Sec. 16, and essay on the writings of M. Louis, and Vol. I. p. 180.) Thus,—

"When the effusion is extensive, the *other signs* of congestion are less obvious, as is the case *in inflammation* when effusion occurs."

The reason shows that one affection is as much inflammatory as the other. The philosophy must be the same in both cases, and on no mechanical view of the pathology can it be explained. Our author shall state it, as we did in our Letters on the Cholera, to which we shall soon refer:

"This effusion," he says, "*acts on the veins* in congestion *as* the effusion which takes place from inflammation *does upon the arteries.*"

Commending the foregoing fact once more to the full consideration of the reader, we pass on to another organ:

"4th. When the *liver and its associate vessels* are the seat of congestion, the liver is excessively gorged with blood, so that if you cut it across, the superabundant blood spouts out; and the veins of the mesentery are extremely loaded with blood; the splenic, the superior, and inferior mesenteric veins, are so gorged as to form a beautiful tree."(1)

Here, again, the colouring is too high. The full extent of the lesion is rarely realized, except in congestions that are attended with idiopathic fever, where it often applies with great force. And that our author was sensible of the fact appears from what is said of his opinion by his biographer. Thus we read, that,

"In many cases the traces of congestion after death are not observable, from the blood retiring into the larger vessels, as occurs often in cases of *common*

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 165–166.

inflammation; and this may be expected in proportion to the degree and duration of the congestion." (1) (P. 256.)

Having bestowed some attention upon the state of the venous tissue in morbid congestions of the veins, we shall venture to state very briefly the result of our observations.

Whether this affection have existed in a simple state, or complicated with idiopathic fever, we have remarked in several instances that the vessels, particularly of the external coat of the veins, have appeared manifestly increased in number and size. We have also observed the important fact, that the coats are not attenuated; but, on the contrary, in instances of protracted congestion, we have found them palpably thickened. This was especially the case when congestion had been connected with tumours, and analogous developments that may form in the parenchyma of organs, and where pressure could not be supposed to operate. At other times, when the disease has been more rapid, we have discovered a softening of the inner coat. This phenomenon we have especially observed in some congestive fevers, and in the cholera asphyxia.

The foregoing statement is embraced in the summary of this article which we had the honour of transmitting, in the spring of 1837, to Dr. Johnson, as stated at the commencement of this essay. We have since been sensibly gratified at observing that Dr. Mackintosh, at a later period, has laid before the public a similar statement as to the veins in cholera. (2)

"As to the blood-vessels," he says, "he wished to direct the particular attention of the Section to them. He would show by *many* preparations, the diseased state of the inner coat, the organization of which was completely altered, so as to render them unfit for their functions; and this disease *extended throughout the whole series of vessels*, until it terminated in a gelatinous pulp." "Would this state of the vessels have considerable influence? And how far would it be concerned in producing that state of blood, always observed in cholera? Dr. Clanny could fully confirm all the observations of Dr. M."

Besides what we have stated at page 258–259, the following was there appended as a note.

"Without having entertained our present views when we published the results of our investigations in this disease, we stated that the small intestines generally denoted some morbid vascularity. If it resided in the serous membrane, it appeared not to exist in the minute ramifications of the vessels, and was

(1) Boot's Memoir of the Life and Medical Opinions, &c. vol. i. p. 132.

(2) Discussion at the British Association for the Advancement of Science, for 1837.

restricted to the venous system. But this membrane was generally natural. The mucous tissue, and the connecting cellular substance, embraced, in most instances, the vascular fulness, which appeared to be generally limited to the veins. Sometimes, however, the injection was very minute, and a florid redness extended over the whole surface of the membrane."

The latter clause of the foregoing extract appears to exemplify Mr. Annesley's remark, that "congestion (venous) after a brief duration may terminate in the inflammatory state."

When the cholera subsequently appeared among us in 1834, we had made up our mind that the altered condition of the venous tissue performed an important part in that disease, (which we still regard as an idiopathic fever,) and that the proximate cause of the venous affection is a malignant form of inflammation.

And since we have been thus led into this review, we may add other extracts as showing farther our opinion of the agency of the veins in the foregoing disease, and the beginning of that induction which has resulted in the present essay on the pathology of venous congestion. But we are more especially induced to quote from ourselves on this occasion, on account of the general bearing of the following remarks upon various topics which have been examined in the course of our present undertaking. We read, then, as follows:

"We look in vain for that florid redness which distinguishes acute inflammation. Its hue is dull, nor does it exist in those minute vessels which are the instruments of that mode of disease. It is evident that this appearance exists in the venous, and not in the arterial, system. The larger arteries, leading to the intestines, are exhausted of blood; the veins are commonly full, and there is never presented any of that minute injection, which would be necessary to the existing condition of the larger vessels, did the redness reside in the terminating series of the arterial vessels. But this redness (venous) does not uniformly belong to the *mucous*, and may then be found to exist in the serous tissue. The constitution of these tissues is so different, and the ordinary results of their morbid affections are so characteristic of their peculiarities, that striking modifications should at least arise, when inflammation of one or the other may happen to be the proximate cause of asphyxiated cholera. This irregular development of *congestion* and *sometimes* of *inflammation*, in

one or the other of these coats of the intestines, will doubtless account for the tenderness of the abdomen in some instances, and for its absence in most." "It has happened in many instances that the appearance of redness is wholly wanting, where the mucous tissue is softened and disorganized." "The most remarkable instance of softening of the mucous tissue I have witnessed, occurred in the black female, &c. Every tissue of the small intestines was so softened, that they were easily penetrated by my finger. It will be recollected that the appearance of congestion, in that case, was confined to the serous membrane."

"I regard the cholera asphyxia, as a *nova pestis*. I cannot recognise its parallel in ancient or modern authors.(1) I consider it a *fever*, of which the *collapse* is the first stage, and reaction the second." "The variety of local developments carries us, alone, to the true philosophy of the disease; and, when it shall be established that the whole system is simultaneously impressed by the morbific poison, then shall we understand why one organ, more susceptible in one individual than in another, or more subject to the influences of those sympathies by which it is connected with the whole system, and *vice versa*, is found sustaining more or less than a proportion of the morbid action, and, in its necroscopic character supplying corresponding evidence of disease. Then may we comprehend the greater coincidence in the vital phenomena, than in the morbid appearances; and can better understand the rapid destruction of the powers of life where the whole system participates in the invasion, than by referring these astonishing events to the influence of disease as established in a single organ."

"I am far from believing, however, that the pathological state of the stomach and intestines has not a very important influence on the whole system; augmenting the violence of the constitutional symptoms, and precipitating death." "There is a particular determination of morbid action upon those organs; and their well known properties, and important relations, can leave as little doubt, that the whole circle of sympathies is variously and extensively disturbed."

"The astonishing discharge of serum from the mucous membranes cannot be the result of a mechanical process. The vital properties, and the modified functions of the extreme vessels, must be concerned in the production of this fluid; but, the se-

(1) See Graves on the Origin, &c. of the Asiatic Cholera, in Dublin Journ. of Med. Sci. Jan. 1840.

cretion is so rapidly performed, that the component parts of the blood are separated without supplying that usual evidence of change, which marks the productions of analogous processes. With as much propriety may it be said, that the profuse discharge from the skin is a simple exudation from the blood, as the separation of the serum by the mucous membrane may be attributed to the same mechanical principle. Whether the mode of action be always the same, or what particular modifications may arise, it will probably be difficult to determine. Dissections must be made with an *especial reference* to this inquiry, which may yet reflect *other light on the pathology of inflammation.*"

"There is one analogical fact, in the view of the subject just taken, that may encourage the presumption that the action is allied to the proximate cause of inflammation. It is now generally conceded that profuse hemorrhage from the mucous coat, whether of the stomach or intestines, is frequently the result of acute inflammation, and proceeding from the secretory organs of that membrane. It appears, therefore, to be a fair suggestion, whether the fluid evacuated, in a great proportion of choleric patients, may not be the product of an action analogous to inflammation." "The system sustains the depletion, with less prostration of its powers than would arise from any other mode of abstracting the vital fluid." (Vol. I. *Bloodletting*, sec. 5, 7, &c.)

"Let it not be imagined, that, in having thus given a fair representation to the subject, I am already becoming a convert to the doctrine which regards inflammation of the bowels as the proximate cause of cholera. I am not only willing, but could even desire, for the reasons I have assigned, that it may be yet determined that this mode of action is *analogous to something with which we are familiar, and the treatment of which may be embraced under the settled principles of our science.* We shall have accomplished much, if we can relieve the intestines of the weight of disease which they so generally sustain, and, in doing which, the constitutional derangement will be released from an overwhelming sympathy, and the blood-vessels will be no longer exhausted."

"If what has been denominated *premonitory diarrhœa* be regarded only as the result of some local affection, and ultimately becoming an exciting cause of the constitutional derangement, then will be explained the mystery which has enveloped the issue of the treatment, when applied under circumstances of local

or general disease: then may we comprehend how abstinence alone, when diarrhœa portends the danger, will enable nature to effect her escape; or, how it happens when neglected for a day, or an hour, it becomes the exciting cause of a perilous disease, which had been but the preceding moment in a state of incubation, and which now baffles the efforts both of nature and of art; or, if submissive in cases of rare felicity, its successive and capricious developments still require the vigilance of zeal, and the decision of a firm observer, to conduct it to a slow and successful issue."

Here, too, we endeavoured to show the fallacy of the nervous and humoral doctrines of the disease. — *Letters on the Cholera Asphyxia of New-York*, 1832, *pp.* 36, 57 — 83, 130, 133, 137.

The cholera has passed away, and little of its recollection remains, but that some fraudulent nightmare had bestrode the earth and has left a fading sense of weariness or exhaustion. Such, indeed, was the seeming illusion, that its posthumous accounts were rather regarded as a legendary tale, and only worthy the consideration of the dreaming philosopher. And, whilst now obtruding ourselves, we may say, that we early heard that, in certain professional quarters, our own unpretending work "should have been left to the columns of a newspaper," in which it had been originally placed by the distinguished individual to whom the letters were addressed. Afflicted by disease, which had made its invasion before the cholera had become the subject of our toil, and which, through a tedious succession of years, has steadily pursued us to the moment we are again about to appear before the public, we had been discouraged in our humble efforts, but for the cheering voice of one man, who renewed our energies, and gave rise to the present undertaking. (1)

But, however the cholera may have justly lost its interest in an abstract sense, we cannot but think, that in its pathological relations to other complaints, it has contributed an amount of light which must rescue it from oblivion.

It is not our purpose, by the foregoing particularity of our statement, to establish any special merit for ourselves; but to show that we have here no acknowledgment to make for any suggestions from others; whilst the simultaneous observations of Dr. Mackintosh, who had no theoretical objects to advance, apply with great force to our inquiries. But, we will not leave this refer-

(1) Samuel Cooper, in his Edition of Good's Study of Medicine, 1834, art. Cholera.

ence to claims of originality, without saying, that we believe, also, that our method of investigating our several subjects is, in a general sense, peculiar to ourselves; and that, where we have followed the opinions of others, we have referred specifically to the fact. But, in saying this, we are far from supposing that there will not be a vast number who will see nothing in our facts, and nothing but error in our conclusions. We may therefore be wholly wrong. This, however, does not affect our principle, nor the magnitude of the enterprise upon which we have embarked. If we fall, it will be beneath a wreck of such proportions as will render us almost insensible to the result. But, the axe must be first laid at the foundation upon which we have reared our superstructure. The Vital Powers must be first stricken out of existence. The human organism, and all its associate living matter, must be brought within the realm of physics; and when this demolition of nature shall have been fully perpetrated, the various phenomena of natural and healthy actions, the results of natural and morbific agents, the modus operandi of the curative means of disease, and all their relative contingencies, must be shown to be susceptible of a rational interpretation upon those principles which have been substituted for the laws which were ordained at the Universal Creation. That we have scattered many faults over the whole, we have certainly no doubt; and in stating what we may have unadvisedly said of ourselves, and that our labours have advanced under many and great discouragements, we have had in view some apology for such errors as may not relate to the great principles about which we have been concerned. For errors in doctrine, we neither ask nor expect indulgence.

As a farther corroboration of the inflammatory nature of venous congestion, we refer the reader to other anatomical characters which are ably pointed out by Dr. Mackintosh. We may say, also, that acute phlebitis was not an unfrequent result of saline transfusion in cholera, and was made an objection to its use. (1) What does this indicate? Would a like effect be produced by similar transfusions in the absence of vēnous congestions?

Nor can we leave the subject of cholera, without saying that it is eminently due to Dr. Horner, as well as an advantage to our investigation, that we should refer to his luminous paper on that malignant disease. Dr. Horner states the following anatomical results.

(1) London Med. Gaz. vol. x. p. 453, and other Records.

"3d. Vascular derangements and phenomena which are confined almost exclusively, if not entirely so, to the venous system. 4th. An exfoliation of the epidermic and venous lining of the alimentary canal, whereby the extremities of the venous system are denuded, and left patulous."

Dr. H. does not explain his opinion as to the pathological condition of the veins; but he considers them as being the principal source of the phenomena.

"The *superficial venous layer*," he says, "performs an important part in the pathology of cholera." "The precise state of the *venous* system of the digestive canal is among all the traits of cholera, that which will most fully account for its destructiveness to human life." "When cholera has lasted for a few days, this venous intertexture, which I have denominated, for reasons stated, the *superficial venous layer*, is exfoliated from the stomach, and large intestines especially, but also in a degree from the small." He supposes the inflammation to reside in another tissue. "The copious secretion of fibrine proves that this action is not a simple acceleration of the circulation, but an actual inflammation. This inflammation having reached a certain degree, and a certain duration, is followed by sloughing of the membrane itself, that is to say, the part where the most important actions of life occur, to wit, the *superficial venous layer*." (1)

Dr. Horner's essay is of great value. It is one of the first indications we have of the agency of the venous system in the phenomena of the malignant cholera, and the structural lesions of the alimentary canal are minutely set forth. The ancients, it is true, and down to the present century, were fully sensible of the important pathological bearing of venous congestions; especially of the abdominal viscera, (p. 218,) and to Dr. Armstrong is due, in part, the credit of restoring to the pathology of cholera this particular characteristic. It is also worthy of remark, that Armstrong adverts to the peculiar conditions of the *mucous membranes*. Thus:—

"Congestion of the liver frequently exists with a simultaneous congestion of the brain and bronchial lining. And these parts seem to be all affected in what is called the Cholera Morbus of India; the worst form of which is nothing more than *congestive fever;* and the *bronchial lining is almost always affected.* But, as far as I can find, the affection of the bronchial lining has not been noticed, because

(1) American Journ. of the Medical Sciences, May and August, 1835, pp. 59, 61, 291, 292, 293.

In reference to what we have said in our first vol. pp. 686, 687, we may add a remark which is quoted by Dr. Horner from Sir C. Bell. "It has been supposed that the fluids excreted from the surface of the intestines were furnished by very minute *foramina*, which are *visible* by particular preparations, in the interstices of the villi. See the letter of Malpighi to the Royal Society of London on the pores of the stomach, and the paper by M. Galeati in the Bologna Transactions, on the inner coat, which he calls the *cribriform* coat. The *pores*, according to Galeati, are *visible* through the whole tract of the canal, and particularly in the great intestines."—*Ibid.* p. 279.

that membrane seems *never to have been examined* in the dissection of bodies after the cholera morbus of India." (1)

Dr. Mackintosh detected the nature of the lesions of the veins, and traced them into the large vessels of different organs; though, in this respect, however uninteresting to the public, we must claim an originality to avoid the necessity of admitting that our views were not original.

But, we are less interested in ascertaining the pathology of the malignant cholera, than in presenting a chain of analogical facts, which shall indicate the true pathology of venous congestion. If the venous turgescence in cholera depend upon inflammation of the veins, it is an unavoidable conclusion, in the absence of other apparent causes, that the same pathological state occurs whenever this phenomenon is manifested as a morbid condition. But, similar facts will multiply upon us, not only as it respects the congestions of the organic viscera, but those of animal life. As to cholera, there are two circumstances that are remarkably worthy of observation, and should not be forgotten in our subsequent inquiry. We mean the great rapidity with which disorganization takes place in the veins of cholera subjects, and the absence of all local symptoms that would imply the existence of such destructive inflammation of the venous tissue. And putting aside our own pathological researches, to which we are now as indifferent as we were once anxious for their fate, the observations of Dr. Mackintosh, in a pure example of venous congestion, illustrate not only the rapidity with which low inflammation may disorganize the venous tissue, but exemplifies its remarkable disposition to extend over the entire venous system of an organ. (Vol. I. p. 488.)

The remaining facts of a direct nature, which dissections have disclosed, are the discoveries made by Dr. Cartwright in "malignant fever," by Dr. Hanney in purpura hemorrhagica, by M. M. Ribes and Breschet in typhus fever, and M. Rigaud in the plague. Some of these will be noticed when we speak more particularly of those affections. There is, however, scattered through many learned works a great amount of incidental proof that the veins often undergo great disorganization in their congestive affections. Thus, Dr. R. Jackson remarks that in some cerebral congestions of the West Indies —

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 164. — We generally use the American edition of 1837, and sometimes the original one of 1834.

"The vessels are so much distended that, losing contractility, they appear gorged, so as to exhibit an appearance of *gangrene* at various points." Again, "the choroid plexus is usually a clotted mass, in which the traces of organization are scarcely to be discerned." (1)

And thus, our able Cartwright, who is one of the few that has noticed the state of the venous tissue in congestive fevers: —

"The portal veins, in every instance, were much engorged with dissolved blood." "The cellular substance investing the vena portæ, and the delicate tissue surrounding the hepatic veins, *were always more or less inflamed.*" "In most cases the liver was somewhat enlarged, and generally of a greenish colour. In two cases it was black." (2)

These were the most uniform appearances. Dr. C. does not appear to have examined the inner tunic of the veins.

Where mephitic gases operate with violence, but the subjects survive for some days, we find the pulmonary venous system engorged, and "reduced to a dark brown, greenish, or livid softening." (3) The foregoing examples will readily call to the mind of the reader a multitude of similar records. How constantly, also, are other tissues of organs softened or broken down where venous congestion has existed. This cannot be the effect of a passive or mechanical accumulation of blood. The changes in this respect are frequently more remarkable than from common inflammation. Dr. Jackson, in speaking of congestive fever in certain temperaments, says that "the liver was often enlarged, distended with black blood, and its substance rotten — often lacerated." (4) The scurvy affords numerous examples of a like nature. In the intermittent fever, Senac often found the liver "turgid with blood, and that very black." "In others, the vena portæ was converted into a *very large tube.*" (5)

"That excellent anatomist, Dr. Macartney," says Cooke, "found that the morbid appearances in typhus fever were not those of *common visceral inflammation.*" Wherever they occurred, "according as the head, lungs, abdominal viscera, were engaged in the disease," they were those of *venous congestion.* "In some instances there was a mere *pulpy*, or swollen, and dissolved state of the mucous coat of the alimentary canal. These congestions were always of a purple or venous colour," (6) &c.

"In fevers of a low, nervous, and malignant type," says another, "the mucous membrane of the intestines, and chiefly of the colon, is sometimes found of a uniform dark-red colour, turgid and soft, in consequence of an intense venous

(1) Hist. and Cure of Febrile Diseases, vol ii. pp. 78, 89, 136, 146.

(2) American Medical Recorder, vol. ix. p. 40, 1826.

(3) Williams, on Diseases of the Chest, lec. 19.

(4) Ut cit. vol. i. p. 89. (5) On Fevers, p. 154.

(6) Cooke's Morgagni, vol. ii. p. 522, *note;* where there is a condensed view of the facts.

injection of a passive character, the black pitchy secretions of the membrane, resembling decomposed venous blood." (1)

"I have examined," says Cleghorn, "the bodies of near a hundred persons, who perished with malignant forms of intermittent fever, and constantly found the cawl, mesentery, colon, &c. of a dark, black complexion." (2)

Chirac states that he constantly found the veins of the liver, and those of the brain distended with grumous blood in the epidemic fevers which he describes. (3) Nearly all authors agree in stating that, in yellow fever, the liver is almost always large and gorged with blood. This, and the analogous gastric affection, are the most remarkable lesions. It was found by Jackson, in the severe forms of congestive and yellow fever, when the disease

"Was not opposed, or but feebly opposed by art," and as "drawn from a large field," "the blood-vessels which are spread over the membranes of the brain were generally numerous and turgid, often distended as if they had been artificially injected. *Lungs generally natural.* Veins of omentum, stomach, and appendages, distended as if injected." "On the interior of the stomach, the veins were generally turgid." "Liver generally enlarged, and its vessels filled with dark fluid blood." "Spleen distended, gorged with blood." The same appearances, though to a greater extent, in the "concentrated fever of the West Indies." (4)

In the "genuine yellow fever of authors," says Craigie, with many authorities before him, "in general the blood-vessels (veins) of the cerebral membranes are numerous, distended, and filled with a dark-coloured blood." "The choroid plexus is often quite like a clot of blood." "It resembles a clot of black blood, more than an organized part." "The vessels of the stomach are generally much distended with dark-coloured blood, the mucous coat abraded, loose," &c. So of the intestinal tube. "Liver distended, heavy, and generally larger than usual, of a spotted and variegated colour, like marble; the blood-vessels filled with dark fluid blood." "Spleen generally distended, sometimes ruptured." (5)

We have more of the foregoing sort of facts from Dr. Craigie, in our examination of his objections to venous congestion. (APPENDIX I.) But what is especially interesting to remark in this account of yellow fever is the statement that "the lungs *never* present morbid changes unless accidentally in the open, violent, and simple tropical fever." This fact has an important bearing upon the doctrine of *remora*. We should say, however, that it is unimportant to us whether the foregoing lesions are modified by climate. We regard them as imparting only a malignancy to yellow fever, the fever itself being a constitutional and inde-

(1) Cyclopædia of Prac. Med. Lon. Art. Inflam. p. 730.
(2) On the Epidem. Diseases of Minorca, c. 3, p. 104.
(3) Trait. de Fièv. Malig. et Pest. &c. (4) Op. cit. pp. 77, 78, 81, 82, 89, 94.
(5) Practice of Physic, pp. 203, 211.

pendent malady from the onset. Indeed, if we allow the cases described by M. Louis, as observed by him at Gibraltar in 1828, to have been yellow fever, which we are scarcely inclined to do, the question would be settled against him as to the supposed dependence of this fever on any particular local affection.

In respect to the plague, Rigaud observes of 68 dissections, that "the *arteries* are almost always empty." (1) "The veins, on the contrary, are distended and full of black clotty blood; and while the innêr coat of the *arteries* has presented no alteration, *that of the veins has been found spotted largely and irregularly, as if with ink.*" (2)

Since the foregoing was written, it appears that Dr. Bulard has communicated to the Brit. and Foreign Med. Rev. (Oct. 1839, p. 550,) the results of his anatomical investigations of the plague during a residence of six years in Turkey and Egypt; as follows: —

"Sinuses of the dura mater and meningeal vessels generally very full of blood; *membranes* themselves *normal.* Stomach frequently loaded with blackish fluid; internal surface marked with petechiæ. Liver loaded with black blood. Spleen treble or quadruple its normal size, loaded with black blood. Kidneys twice or thrice their natural size, loaded with dark coloured blood, with an appearance of renal hemorrhage in the pelvis. *Pancreas natural.*"

Mr. Boyle states, that in the epidemic fever of Sicily, the liver was morbidly large, softer than natural, and the entire system of the vena portæ always crowded with blood. He regarded this as an essential feature of the disease. (3) From Dr. Mouat we learn the important fact of his having witnessed, in India, several cases of hepatic disease, in which there was a discharge of pus from the bladder, and other remote parts. In these cases, dissection disclosed the existence of abscesses in the liver. The other effusions were "secondary," and clearly depended on venous inflammation. (4)

M. Malliat (5) found the brain the seat of congestion in most of the cases of the Bona fever, and concludes, therefore, that intermittents are constituted by lesions of the nervous system. But this induction is a fruit of exclusive anatomy. The intes-

(1) This statement is made by others in relation to other fevers where venous congestions prevail. It is important as showing that it is not the tendency of "dark coloured blood to obstruct the capillaries," nor does it countenance the opinion of Dr. Craigie, that there is a stasis of blood in the capillaries during the hot stage of fever, &c. (P. 209.) The "clotty blood" should go with our facts, vol. i. p. 451 — 459, 668, &c.

(2) Lon Med. Gaz. vol. xvi. p. 518. (3) Edin. Jour. vol. viii. p. 184, &c.

(4) Calcutta Quarterly Med. Jour. July, 1837.

(5) Trait. des Fièvres, ou Irritation Cerebro-Spinalis Intermit. en Afrique, &c.

tinal canal was affected in 1000 or about half the cases. His dissections supply abundant proof of the connection of a modified condition of the venous system of different organs with the constitutional affection.

In beriberi, "the liver is invariably described as being uncommonly large and very dark coloured. Mr. Marshall states that in his case, on removing it from the body, a great quantity of dark coloured blood flowed from the large vessels, and that it then became reduced in size, and natural in colour. The whole substance of this viscus is commonly gorged with blood." (1) So also Mr. Hamilton, who regards its pathology as arising, "in a great measure, from obstructed circulation in the internal parts, more particularly the liver and lungs;" and that it is not a disease of debility, as supposed by Colquhoun, Hunter, Christie, and others, "but a disease, in the treatment of which, venesection may be used with the greatest prospect of success." (2)

Mr. H. was led to the use of bloodletting by the venous congestions; and, on applying the remedy, he found the blood buffed. If the "obstructed circulation was the consequence of congestion," (which appear to be the same thing in our author's sense,) what occasioned the congestion? Mr. H. says, a damp atmosphere principally. This condition of the veins having been regarded only as an accidental circumstance, the pathology of beriberi has been a mystery; at least, an important part of it. Mr. Malcolmson sustains our conclusion by his account of the symptoms, and predisposing causes; but he was intent on spinal irritation, although there appears to have been but very few anatomical facts relating to the medulla spinalis. (See Appendix I.) As to the liver he says: —

"Few organs suffered more frequently, although there are seldom any severe symptoms. The most usual are a yellow eye, sallow complexion, and fœces of much discoloured bile, usually in too great abundance, but occasionally the stools are nearly white." "The liver is sometimes altered in its structure." "The disease is sometimes fatal in a few hours, but is often chronic, and in these the patient is liable to sudden death, to rapid aggravation of symptoms, or supervention of new and more formidable ones, by which he is soon carried off; (3) and if he survive them, he may live a long time bedridden, dropsical, and a true paralytic." "Other chronic disorders are apt to end in symptoms analogous to some varieties of beriberi;" the symptoms of the latter being quite various, and drawn by our author in a graphic style. "New comers are most liable to it," and it prevails mostly on the coast, in the vicinity of rice fields, low swampy grounds, rich alluvial soil, and is promoted by a damp air. He constantly refers to analogies with European diseases of a

(1) Cyclopædia of Prac. Med. Lon. Art. Beriberi.
(2) Edin. Med. Chir. Trans. vol. ii.
(3) See our Essay on Bloodletting, vol. i. p. 199.

congestive character, — manifesting all their varieties. (1) Mr. Howard, (2) and others, are also of the opinion, that beriberi "does not appear to possess any other striking peculiarity but the extreme fatality which attends it."

All agree that the powers of life are greatly prostrated in the acute forms. (P. 219.) The most judicious, like Hamilton, Scott, Marshall, Malcolmson, Stevenson, Macdonell, Paterson, Christie, &c., employ bloodletting, — Marshall, very extensively. Wright, Horklots, and Campbell, are opposed to it on the ground of debility. (P. 261.) Paterson bleeds in the beginning, but "does not consider the disease at all inflammatory." (3) There is little doubt that the congestive affection in this disease, (which, modified in a peculiar manner by the remote causes, imparts its characteristic phenomena,) is often complicated with idiopathic fever. Malcolmson believes it generally local, but frequently connected with a general fever. (4) (See APPENDIX I.)

It is well known that M. Ribes discovered an inflammatory state of the congestive veins in typhus fever, &c.

"I am led," he says, "to conclude that the veins, the venous blood, and the cellular tissue, are primarily affected in adynamic fevers, in scurvy, and in the plague." (5)

His facts, though brief, are very important in establishing inflammation in the congested veins; but we neither assent to the humoral philosophy, nor to the doctrine of the locality of fever.

M. Breschet, when speaking of the constitutional effects of phlebitis, states that he found evident marks of inflammation in the cerebral veins of subjects who had died of typhus fever. (6) M. Bouillaud entertains the same opinion; and M. Andral recites a case which he thinks operates in favour of the doctrine. (7)

In our 12th Section we shall introduce a variety of examples of venous congestion as produced by different specific poisons; where, also, the nature of the congestion appears to be more or less modified, as in inflammations of other tissues, according to

(1) Prac. Essay on the Hist. and Treat. of Beriberi, pp. 24, 28, 42, 43, 54, 55, 132, 222, 223, &c. 1835.

(2) Report, &c. Sept. 29, 1824. (3) Report for 1822. (4) Ut. cit. p. 39.

(5) Mém. de la Société Méd. d'Emulation, t. 8, p. 628.

(6) De l'Inflam. des Veines, ou de la Phlébite, in Jour. Complimentaire de Diction. des Scien. Méd. t. 2, p. 325; t. 3, p. 317.

(7) Clinique Méd. t. 2, p. 306. In this case, which M. Andral denominates *une fièvre continue grave*, and subsequently calls *une véritable phlegmasie de la système veineuse abdominale*, the inferior mesenteric vein, and the trunk of the vena portæ and its hepatic ramifications were in a state of inflammation, but not of suppuration. Another similar case follows immediately.

the peculiar properties of the remote causes. See, also, APPENDIX III. on *cold*.

The foregoing facts are sufficiently familiar to the profession; but it is important to connect with an essay of this nature their general summary. We shall also present, from time to time, many analogous illustrations of our subject. It is in diseases like the foregoing, where a constitutional affection becomes so generally complicated with a certain local condition of the blood-vessels, under all circumstances of climate, constitution, and treatment, that pathological anatomy manifests its highest value. Here is something almost invariable, which seems inseparable from the general effects of the morbific causes, at least in such cases as have not an immediately fatal termination. Nor is it to any lesion of structure to which we are now looking, but to another physical condition of an important system of blood-vessels, which no ingenuity can refer to physical causes. If you allege an accumulation of blood about the heart and lungs, and a consequent *remora*, we have shown you already, that this is impossible, and that all other supposed mechanical obstructions are wholly imaginary. In respect to the localities of the congested veins, we find the greatest diversity, — according, probably, to the particular susceptibilities of one organ or another in different individuals. Much, too, will depend upon the stage of the disease. In typhus, and bad forms of remittent fever, if the subjects die early, the great seat of venous plethora is in the brain, or, perhaps, in remittents, in the liver also. When disease progresses longer, the same state of the veins springs up in the other parts, either from a development of predisposition induced by the primary causes, or from the general influence of the febrile action, or from a direct sympathy with the veins antecedently diseased. (Pp. 279, 290.)

In our essay upon the comparative merits of the Hippocratic and anatomical schools, and in our examination of the writings of M. Louis, we have endeavoured to show the superiority of the vital signs in marking the true pathology of disease. In no affection is this more clearly demonstrated than in venous congestion, especially when those signs are taken in connection with such as result from the influence of remedial agents. On the other hand, an incautious dependence upon morbid anatomy has led many distinguished observers into false inductions; and

although we believe that the progress of inquiry will continue to develope new physical proofs of the inflammatory nature of venous congestions, yet shall we endeavour to show, (as well for other purposes connected with our investigations,) that such proofs are not necessary to establish this pathological condition. It has never, for instance, been shown that the presence of the red globules of the blood is necessary to inflammation. On the contrary, it is well known to be compatible with a low state of inflammation, where the organic blood-vessels are very minute, that no remarkable appearance of vascularity shall exist. Inappreciable degrees of it may give rise to all the phenomena of the disease, and even result in serious lesions of the part. It may be constituted by the white vessels alone, which are manifestly concerned in all morbid products, and are, doubtless, the essential instruments by which the changes are produced. When the vital properties are altered, inflammation has begun. The afflux of the fluids will then depend upon the peculiar modification of the forces, the nature of the tissue, &c. It may, therefore, be red blood, or it may be white. The latter condition may resemble the other in all its characters but the colouring principle. We know not, however, what it is; but we know the phenomena, and may reason upon them. "*Physicans*," says Bichat, "*have not paid sufficient attention to the difference of inflammations according to the difference of systems ;*" (1) and we may add, that the more important influence of remote causes, whether external or constitutional, in modifying the nature of inflammation, is too often overlooked.

The foregoing facts are often evinced in the serous membranes, and in all those parts where the sanguineous vessels are least obvious in their natural state. In the tunica arachnoides, according to Cloquet, Beclard, Louis, Bichat, and others, red vessels have never been detected when the membrane has offered every other proof of an inflammatory affection. (2) The same principle is seen in mollescence or other degenerations of the brain, (3) and of the mucous membrane, where their dependence on inflammation

(1) Gen. Anat. vol. iii. p. 46.

(2) Duchatelet and Martinet, Hope, and S. Smith, say otherwise; but the vascularity was probably in the pia mater.

(3) "The brain," says Mr. Hunter, "appears to be an exception to these general rules, (vascularity, &c.); for in all diseases of the brain, where the effects were such as are commonly the consequence of inflammation, such as suppuration from accidents, I never could find the above appearances."— *On the Blood, &c. p.* 283.

is shown by their obvious connection with it in other instances.(1) In many scirrhous affections there is great disorganization, whilst the natural volume of blood is greatly diminished. We may readily see how unimportant is "vascularity" by observing that in the ligaments the vessels do but carry the colourless parts of

(1) It is well known that some distinguished physiologists, as Rudolphi, Gendrin, Chaussier, Ribes, deny the existence of vessels in the serous membranes, notwithstanding the phenomena of their inflammations, their lesions of structure, and their obvious function of secretion, which analogy refers to the vital process. True, all these results are referred to a contiguous part, though the membrane itself be the manifest seat of the phenomena; whilst it is generally true that the diseases of the serous membranes affect the organs they envelope less than those of other membranes.

Bichat says, "I have seen the arachnoid membrane, in a subject that had been affected with chronic inflammation, evidently thickened on the internal surface of the dura mater, without this having experienced the slightest alteration." (*a*) Foville witnessed a like instance, as did also Hodgkin, (*b*) who give a circumstantial account of the appearance of the arachnoid, from the incipient stage of the disease. Hope (*c*) has found it as thick as a wafer. Hodgkin found adhesions with the dura mater. "The lung of a panther, who died of Phthisis, presented M. Bazin with a hypertrophied condition of the pleura pulmonalis, which, in the healthy state of the animal, is not thicker than in man." (*d*)

So also these membranes are said not to possess nerves, because they elude our observation. But, is not the exquisite and characteristic pain in all parts, which often attends serous inflammations, a better proof than a mere negative fact which rests upon the fallacious testimony of sight? "When the white organs are inflamed, they receive an augmentation of life, a superabundance of sensibility," &c. (*e*)

Bichat undoubtedly "demonstrated," what analogy would suggest, the absorbents of the serous membranes; that "their origins, a thousand times intermixed with each other, and with the orifices of the exhalants, contribute especially to form the texture;" and that, "the difficulty of distinguishing the absorbent and exhalant pores is no reason for denying their existence." (*f*) Cruveilhier, and Mascagni, suppose that the serous tissues are made up of absorbent vessels. Nor is this all. "If we irritate a serous membrane," says Bichat, "at the end of some time, it will be covered with an infinity of reddish streaks, so numerous as to change its whiteness into the red colour of the mucous surfaces. It may be made *wholly black* by fine injections;" &c. "In chronic and acute inflammations, the serous membranes exhibit a vascular net work so full of blood that the redness is often deeper than that of the muscles." "We see that the blood is evidently contained in the capillaries of the membrane." (*g*) And so Hodgkin. (*h*) "The non-vascular appearance of the tendons, cartilages, ligaments, &c. in the living body, is illusory." (*i*) Analogy left no doubt of it, and experiment has now confirmed it. These are cases in which we are called upon to give up our senses to the understanding, — to reject any negative observations by the microscope, or any method which art may devise.

We have added the foregoing note for a variety of purposes, which will be more or less obvious in the progress of inquiry. See APPENDIX V. on *Tubercle*.

(*a*) Gen. Anat. vol. iii. p. 141.— (*b*) Morbid Anat. of Mucous and Serous Membranes, vol. i. pp. 69, 75, 87.—(*c*) Principles of Morbid Anat. p. 278.— (*d*) Brit. and For. Med. Rev. vol. v. p. 221.— (*e*) Bichat's Researches on Life and Death, p. 201.— (*f*) Gen. Anat. vol. i. p. 341; vol. iii. p. 350— 351.—(*g*) Ibid. vol. ii. p. 7.— (*h*) Ut supra. pp. 28, 37.— (*i*) Bichat, ibid. vol. ii. pp. 7, 9, 61.

the blood in the early stages of inflammation, which are as well characterized by physical signs as the latter.

"It is a great mistake," says Bichat, "to try to represent inflammation as being every where the same, as exibiting always the fluids, like their vessels, in the same state. Boerhaave, for example, thought there could be no inflammation without an *error loci*. There is according to the state of the parts, their structure, their vital properties, a thousand different modifications in the new anatomical order that this affection gives to the organs." But, he agreed with Hunter, that "the principle is always the same, it is always the same disease." (1)

"In chronic inflammation," says one of the physical school of inflammation, "there is but a very slight degree of pain, and seldom much heat or *redness*. Sometimes the local symptoms are so obscure as to be perceptible only to the scrutinizing eye of an experienced observer." (2)

Let us hear Mr. Hunter to the same effect:—

"That the degree of inflammation which becomes the cause of adhesions gives but little pain, is proved from the dissection of dead bodies; for we seldom or never find a body in dissection which has not adhesions in some of the large cavities, and in parts which the friends of these persons never heard mentioned, during life, as the subject of a single complaint.

"That adhesions can be produced from very slight inflammation is proved, in ruptures, in consequence of wearing a truss; for we find the slight pressure of a truss exciting such action as to thicken parts, by which means the two sides of the sack are united, though there be hardly any sensation in the part. We also see, in cases where this inflammation arises from violence, that it gives little or no pain." (3)

M. Dupuytren considers the *white gangrene*, as he does, also, the gangrena senilis, an inflammatory affection of the arterial capillaries; and upon this principle he restored a patient, "*par enchantment*," by abstracting blood from the arm. The hands and and feet were the seat of the affection. The fingers and feet were blanched, and of an icy coldness, wholly insensible, immovable, and dry as parchment. (4) This "white gangrene" is also particularly noticed by Dr. Graves, and referred to inflammatory action.

During the inflammatory stage the part has the appearance of "white wax." "Again, in urticaria, we often see some portions of the inflamed skin assume a *white* colour, and the same occurrence may be noticed likewise in the wheals caused by nettles or the stings of bees." (5)

(1) General Anat. &c. vol. ii. p. 61.

(2) Lon. Cyclop. of Prac. Med. Art. Inflam. p. 756.

(3) Hunter on the Blood, and Inflammation, p. 287.

(4) Rev. Méd. and Trans. Méd. 1833.

(5) Clinical Lectures; lec. 8, p. 301. Dr. Craigie, (*Prac. of Phys. p.* 402,) and other late writers, ascribe *dry gangrene* to inflammation. It is the only philosophical construction, however it may have grown out of the doctrine of "stagnation."

See a remarkable case of recovery from dry gangrene in Lon. Medico-Chir. Trans. 1839, Art. 17. It occurred in a child three and a half years of age. Nature amputated

The indefatigable Louis affords abundant proof that redness is not essential to inflammation, and that—

"Paleness of inflamed structures takes place sooner or later, as is exemplified in the various shades of colour of hepatized lung." . So, also, frequently of "softening." "It ought to be noticed, that continuous with a red and softened portion of mucous membrane, we often find another equally softened, but without redness. If the first, therefore, be inflammatory, it is probable that the other is also." (1) (See APPENDIX ON ANALOGY.)

We shall have many facts like the foregoing in our examination of M. Louis' doctrines. They are important in a variety of aspects.

"In general," says Dr. Graves, "we connect the idea of integumental inflammation with the appearance of redness, and this phenomenon is explained on the hypothesis that a preternatural quantity of blood is circulating in the inflamed parts. To what cause, then, are we to attribute the coexistence of increased vascularity, and a remarkable pallor of the parts; a state displayed in a very remarkable manner in *phlegmasia dolens?* It is easy to conceive that in certain stages of inflammation, the quantity of serous or white blood may be suddenly much increased, and that this increase may be accompanied by all the phenomena of inflammation except *redness*. The phenomena of phlegmasia dolens prove that a *white inflammation may be quite as intense as red inflammation;* a fact which I saw exhibited in a remarkable manner in the case of a woman labouring under p. dolens, and in whom the disease suddenly attacked the eye, and destroyed it in a short space of time, disorganizing it rapidly without the supervention of any redness during the destructive process." (2)

Mr. Morgan maintains that inflammation may exist and prove fatal, without any "*apparent alteration*" of the parts affected, and—

"We are not to conclude, provided there was good evidence to the contrary during life, that a person may not have died of inflammation, simply because the part does not appear red." (3) "Although the part may be perfectly

one arm at the elbow, and the other between the elbow and shoulder, and the left leg above the ankle. "Stumps were formed which might shame many formed by the operator's knife."

(1) Researches on Phthisis, sec. 135, 136. p. 93.—We shall, however, have something more to say on this conclusion of our author, in our brief examination of his writings.

(2) Lon. Med. Gaz. March 18, 1837, p. 943.

(3) Principles of Surgery, p. 58.—And yet we are told by this able writer, that "nothing satisfactory can be elicited by considering the symptoms during life, or examining the alterations of structure left after death;" and that, "there is one way only of arriving at the truth, and that is by inducing the disease in the transparent parts of an animal, observing with the *microscope* the changes which ensue," &c.—*p.* 33.—This had been abundantly done; but has it improved our philosophy, or rendered us more skilful?

blanched," says Prof. Gross, "yet this does not prove that this process (inflammation) is not, to a certain extent, present." (1)

The universal contraction, also, which affects the capillary system after death, must be felt by the instruments of disease, and more or less of their contents must be expelled in consequence. From these, and other considerations, it is evident that redness, or vascular fulness, are contingent results, and not necessary elements of inflammation.

"I have said twenty times, and I repeat it again, that the only cause which prevents the red globules from passing into vessels with fluids, is the want of relation between the nature of the fluid and the sensibility (irritability) of the organ. The opposite opinion is of the school of Boerhaave." (2)

Scirrhus, and its ulceration, appear to be excluded by some from the genus of inflammations, and, like another inflammatory product, tubercle, to be regarded as results of some mysterious "modification of nutrition." All their phenomena have yielded to the apparent absence of red vessels. Even Andral, (3) however, as we shall show, assigns the latter at last to inflammation; as do, also Broussais, (4) Langenbeck, (5) Geddings, (6) &c. The erudite Dr. Craigie ascribes, in a general sense, (p. 189,) "softening," "induration," "hypertrophy," "suppuration," and all the varieties of "gangrene," to inflammation. (7) That excellent pathologist, Prof. Gross, in his late work considers scirrhus, tubercle, ulcerations, softening, to be, invariably, the results of inflammation. (8) Abernethy defended the inflammatory nature of scirrhus.

All the foregoing, in a general sense, is still the doctrine of those who give the highest place to the phenomena of nature; and we think the conviction strengthens when we consider the obstacle to a full avowal of what a powerful class regard as heretical "notions." We may say, too, that in France, scirrhus is often held to be inflammatory by those who have not rejected inflammation as a disease. (9) Breschet, and his associates, whose authority we now quote, consider inflammation indispensable to scirrhus. If the vessels be almost obliterated, it is

(1) Patholog. Anat. vol. i. p. 99, 1839. (2) Gen. Anat. vol. ii. p. 74.
(3) Précis d'Anat. Path. t. 1. p. 501. Dic. de Méd. et Chir. Prat. t. 4. p. 444.
(4) Prop. 196. (5) Nosol. p. 316. (6) American Cyclop. Prac. Med.
(7) Practice of Physic, vol. i. p. 396-402. 1836. — He makes an exception sometimes in behalf of suppuration.
(8) Patholog. Anat. vol. i. pp. 84, 99, 162, 182, 1839.
(9) See Art. on Cancer, by Breschet, in Diction. de Méd.

owing to the specific nature of the inflammatory action, the deposition of lymph, &c.; whilst on the other hand, when carcinomatous tumours are soft, as in fungoid disease, the vessels are large, and either of a bright red, or a venous livid hue.

The opinion of Hodgkin is valuable on all that is connected with inflammatory diseases. He does not agree with others,

"In regarding those accidental productions, to which the term 'malignant' has been applied, as so completely similar to the products arising from *ordinary* inflammation."

This is sound, and obvious doctrine; the inflammation being of a specific kind, as expounded by Mr. Hunter. Our author also takes, as it appears to us, a luminous view of the affinities of "scirrhus and fungoid tumours."

"Although, as I have endeavoured to show, these formations, by their structure, mode of development, and, for the most part, by their influence on the system, are with propriety to be referred to one common type, and grouped into one family, yet, as we shall presently see, there are certain peculiarities, which will justify us in forming at least three specific divisions. Their limits, it is true, are so ill defined, and they pass so gradually into each other, that it is often difficult to decide to which of the species a particular specimen should be referred. In the best marked cases, however, there is no difficulty of this kind.'(1)

From these "best marked cases," we pass by insensible degrees through all the varieties of carcinomatous affections,

"We not only meet with the various intermediate gradations between well-marked, true scirrhus and the fungoid disease, but we also find the various combinations of these in the same subject." (2)

Nay more; it sometimes happens that we meet with some of the conditions, which are reputedly distinct, more or less associated in different parts of the same tumour. There is but "one common type," and this is inflammation, which, according to its shades of modification, gives rise to the diversified results. It will be seen, in our 12th section, that these considerations have an important bearing upon what we consider modifications of venous congestion, or phlebitis. They depend, intrinsically, upon the nature of the remote causes.

Our author appears to incline to the inflammatory origin of tubercle; and we see indications, in many quarters, of a determination to rescue this subject from the trammels of morbid anatomy and chemistry. Mr. Williams, in speaking of tubercle, remarks, —

(1) On the morbid Anat. of the Serous and Mucous Membranes, vol. i. pp. 277, 347. 1836.

(2) Ibid. p. 292.

"If the resemblance is so perfect in the generalities, should some slight variation in particulars, still explicable from a difference of cause, by widely separating the two phenomena be made a pretext to multiply the objects that the mind should grasp? If the fact may be referred to a simple and established law, shall we still leave it in the *already burthened and unwieldly list of anomalous exceptions?*" (1) (Pp. 119, 317—318; and Vol. I. p. 626, *note;* and APPENDIX V. *on Tubercle.*)

We are sensible that it will be the opinion of some that we are becoming too circumstantial, too prolix, for a subject so "well settled against us." This is indeed one of our objects for minute inquiry; though a greater one will be ultimately found in the important bearing which the whole of this investigation possesses in relation to venous congestion. There are various modifications of venous inflammation which will derive illustration from the varieties of inflammatory action as affecting other tissues; (p. 300;) whilst we simultaneously effect our greater purpose of showing that we must make the dissecting knife yield, where it may appear to come in collision with the clearest evidences of truth.

"It is not even certain," says Dr. Davis, "that we are yet acquainted with all the *possible forms* of inflammation, so as to be competent to assert broadly and emphatically, that this or that variety of inflammation should have a natural and necessary tendency to end in disorganization of structure." "Affections, demonstrably inflammatory, may be sustained for years without producing malignant disorganization." "Many diseases, loosely attributed to irritation alone, are often characterized by symptoms which a *more accurate diagnosis* would enable us at once to ascribe to actual inflammation." (2) (Vol. I. p. 239—278.)

Meckel goes so far as to say, "all the anomalies of the vascular system arise from inflammation. All the regular or irregular formations of the abnormal state are produced by inflammation or an act analogous." (3) So also, Broussais, (4) Dr. Parry. (5) It is apparently the surprise of a learned critic, that "some physiologists still deny that all new formations depend on inflammation." (6)

The last mentioned doctrine may not be wholly true; but it can never be controverted, as lately attempted, by inductions from the peculiarities of the polypus, or the abnormal conditions of plants. We rather hold, that it is better to reason from analogies that relate to man himself, than to connect his physiological characteristics with plants and stones. And here we bear

(1) Rational Exposition, &c. of Diseases of the Lungs and Pleura, p. 165. 1838.
(2) Principles and Practice of Obstetrical Medicine, part 20. 1835.
(3) Gen. and Patholog. Anat. vol. i. p. 143. (4) Examen, &c.
(5) Elements of Pathol. and Therapeutics; Collections, &c.
(6) Translator of Meckel's Anat. ut cit. p. 144. 1832.

aloft our motto, taken from another chapter of nature, "*jure possimus morbo alicujus partis, ad morbum alterius analogice disserere.*"

We are fully sensible, however, that they who adopt the foregoing views of scirrhus, tubercle, &c. are considered, in the language of a leading review, as being "trammelled by the usage of an exclusive and erroneous pathology;" and, withal, "ignorant of the late advances in the science." But, there are certain doctrines, which we shall subjoin in a note, that appertain more or less to the schools which differ from us as to the foregoing views of inflammation, to which, we think, some special objections might be made. However this may be, we have thought it may facilitate the judgement of the reader in forming his conclusions as to the relative merits of the Hippocratic inductions from the phenomena of nature, and the systems which repose upon researches in morbid anatomy. It might have been sufficient to have left this special consideration to the specific contrasts we have had occasion to present at different steps of our inquiry, since a single fundamental error in physiology may betray a vicious system of philosophy. (Vol. I. pp. 632, 635.) But, we have concluded, on the whole, that it will be more a matter of fairness to group together some of the prominent doctrines to which we object. (1)

(1) It has been recently contended, for instance, that, "in the the present state of medical science, it must be admitted" that scirrhous tumours are only a "lesion of nutrition." This doctrine has some powerful adherents. "Dr. Carswell considers, that, in *carcinoma* of the stomach, the muscular and cellular tissues of the organ are converted, by the *nutritive* process of transformation, into a homogeneous mass." (*a*)

"According to the *recent views* of organic science," (which means pathological anatomy,) says D'Amador, "medullary sarcoma, melanosis, cirrhosis, &c. are to be regarded rather as altered secretions; but, it will always remain to be ascertained whether these *morbid* secretions may or may not have their *analogies* among the *natural* secretions." We agree to the latter affirmation.

M. Andral carries the humoral doctrine into the foregoing philosophy. We find the following under his division of "*Lesions of Nutrition.*" After adverting to the "*reigning fashion some few years back*" as to "acute and chronic inflammations," he apostrophizes, — "and who will *now* take upon himself to deny, that in certain tumours called *scirrhous*, there may not, as in the *general induration* of the cellular tissue in *new-born* infants, be a *concomitant* alteration of the blood?" (*b*)

And, as a contrast to what we have just quoted in our text from Dr. Williams, we may state the following substitution, by another distinguished Frenchman, of a humero-chemico-microscopical pathology, for the Hippocratic and Hunterian philos-

(*a*) Hodgkin, on the Morbid Anat. of Serous and Mucous Membranes, p. 352 — (*b*) Patholog. Anat. vol. i. p. 151.

He, who shall deny the presence of inflammation in venous congestion on the ground of the absence of vascularity in the

ophy of diseased action according to modifications established by the nature of remote causes, constitution, &c. Thus: —

"A question essentially obscure," says M. Velpeau, "but one of the most interesting in pathological anatomy, presents itself to the mind. *Whence* comes it, that, in a class of tumours, all resulting from the effusion of some of the materials transported by the circulating system, there are found *two kinds* of disease *so profoundly distinct?* Might it be that the blood, the lymph, the MILK, pus, or serosity, *collected between* the organic layers, might in time *give rise* equally to the *gelatinous, hydatid,* and *sero-sanguineous cysts,* and the fibrinous, *cheesy* or *milky, scirrhous, encephaloid, colloid,* and *melanotic tumours?* Or, are the last four species, primative elements not possessed by the others, elements *not found* in *natural* liquids? It is to be hoped that the *microscopical* researches of M. Donné upon the pus, urine, blood, and other products of the animal economy; those of M. Turpin upon the transformations, a species of vegetation, the *multiplication* of the globules of the milk, those of M. Bonnet, upon the *nature* and composition of morbific productions, and those of Messrs. Beauperthin and Adet de Rosseville on the animalcula of decomposed fluids or other animal matter, will some day throw some light upon this obscure point. *Until then,* we must allow that we know nothing satisfactory upon the *origin* of these diverse productions." (*a*) (Vol. I. pp. 56, 57, *notes.*)

But, do we, in reality, know any thing more as to the pathology of smallpox, cowpox, syphilis, measles, all kinds of fever, or of any other disease? The simple essence of the whole consists in variously modified conditions of the vital properties, and what are considered by our anatomist as the causes, are only effects. The student may rest assured that the moment he yields to a speculative philosophy like the foregoing, it will necessarily vitiate all his theoretical and practical habits. The simple guide of nature will be lost, and he will find himself at the mercy of every varying breeze. It is for this reason that we find in the works of all authors, who have not a stability corresponding with the laws of nature, conclusions that are at variance with each other on almost every page. This is the secret of Bichat's occasional self-contradictions, since even this great man was not wholly free from the blemish which we are considering. If there be not a consistent whole, there is some fundamental defect in principle; and where there is unity in all parts, it is the best evidence that the writer is at the fountain of truth.

Again, it is said by a distinguished head of the anatomical school, that "*red* and *white* softening, thickening, ulceration, and intestinal perforation, are owing at different times, to wholly different causes," and, indeed, that their causes are as "*opposite*" to each other as the "colours" are different. (*b*)

It is said, that pus may be formed indiscriminately by the actions of organized matter, by the physical dissolution of natural or morbid products, or of coagula of blood, or in the torrent of the circulation; (p. 188, &c.;) and yet, that purulent matter is so much like life, "we have arrived at the principle on which granulations are formed." (*c*) And so, Dupuytren, Laennec, Villermé.

It is said that "Dr. Jahn, a man of undoubted learning and great talents," endeavours to show that "the common consequences of inflammation, — the adhesion of serous membranes with contiguous parts, &c. — are *normal* conditions in several animals." (*d*) This doctrine is extending in abnormal conditions. It is said, indeed,

(*a*) Velpeau's Treatise on Diseases of the Breast, in American Med. Library, p. 66, 1840. — (*b*) M. Louis. See references in Essay on, &c. — (*c*) Sir E. Home, in Philos. Trans. 1818, p. 194; 1819, p. 1 — 11. — (*d*) British and Foreign Med. Rev. No. 7, p. 126, &c.

inner coat of the vessels, so analogous to the serous tissues, must be equally skeptical of its existence in strongly marked

that Mr. Hunter was totally wrong in all his views of inflammation, whether they respect the condition of the blood-vessels, or the "terminations" of the disease. That vital actions have no connection with the process, but that the work is carried on, from beginning to ending, by mechanical powers, and especially by "stagnation of blood." (Our Essay on *Inflammation*, Sec. 1.)

We cannot recapitulate what has been done in organic chemistry, or how far the laws in that department of nature have been substituted for the laws of life. A view of the whole subject may be found in our former Essays, especially in those upon the *Vital Powers*, *Digestion*, and Section 2d, on *Inflammation*.

It is said, that coagula of blood, long dead, may form for themselves a highly vascular organization, and carry on an independent circulation, and this, too, in the midst of venous blood; (*a*) whilst others contend that venous blood is almost as good as arterial for the purposes of life, (*b*) however it may be regarded in the humoral pathology as the most prolific cause of "malignant fevers;" besides many other similar anomolies. (P. 203.)

It is said, that it is "an important question in general physiology, whether plants and animals of a high degree of organization may be capable of producing from various parts of their tissues, beings, corresponding to those of the inferior orders of their kingdoms," whilst many maintain that the human machine is a special laboratory for various insects. (P. 130, and vol. i. p. 706.) That, "M. Raspail is strongly impressed with the idea that many, if not all, of the exanthemata are, in truth, the result of insect operation on the skin;" and that "cancer, tubercle, cholera, influenza, variola, and certain disorganizations of the liver, kidneys, &c. are the work of entozoa, or insects in the animal economy." (*c*) That Professor Schoenlein of Zurich, supposes that the pustule of porrigo is a proper vegetable fungus. (*d*) That, intestinal worms are the products of *inflammation*. (*e*) That, the genitals of both sexes, when *inflamed*, are a laboratory of insects. (*f*) That, animals may be created out of silex by the action of galvanism, or form spontaneously in water, (pp. 95, 124,) or in a solution of muriate of barytes in distilled water. (*g*) That, hydatids of the uterus, though similar to those of the brain, are "the product of a degenerated conception." (*h*) That, cancer depends upon a vesicular worm, being duly inducted into the *systema naturæ* as the *hydatis carcinomatosa*; (*i*) whilst others suppose that the different kinds of carcinoma have their distinct species of worms, such as the *hydatis cruenta*, *h. lymphatica*, *h. carcinomatosa*, &c., being the latest substitute for the old doctrine of "remote causes," and for the "reigning fashion some few years back as to acute and chronic inflammations" being the cause of these and analogous affections; (*j*) and that, the formation of granulations, &c. which takes place around the worms is intended by nature to protect the animals against any injury from the inflammation and suppuration they may excite, or from the noxious effects of dead hydatids in their vicinity. (*k*) That pulmonary tubercle is a degeneration, or something analogous, of another species of vesicular worm. (*l*) That, the red globules of blood are infusory animals. (Vol. i. p. 707.) That, "thickening" of the intestinal mucous membrane in typhoid fever is sometimes "the consequence of the reaction occasioned by the meteorism," having at such times no

(*a*) See Sec. 9. — (*b*) Dr. Williams' Lectures on Diseases of the Chest, lec. 3. — (*c*) Lon. Med. Chir. Rev. Oct 1838. — (*d*) Lon. Med. Gaz. from Muller's Archives. — (*e*) Bremser, Rudolphi, &c. — (*f*) M. Donné, and others. (The *vibrio lineola* and another species.) — (*g*) Retzius, in Froriep's Notizen; 5, p. 56. — (*h*) Velpeau, Nouv. Rech. sur l'Origine de la Grossese, Hydatique, &c. — (*i*) Dr. Baron, &c. — (*j*) M. Andral, ut supra. — (*k*) Dr. Adams, &c. — (*l*) Carmichæl, Adams, Baron, &c.

cases of phlebitis; since, according to M. Cruveilhier, and others, (however the affirmation be only generally true,) the injection is confined to the outer tunic, and is less than in common inflammations, whilst "the deep red colour of the inner membrane, when it may happen to appear, is owing to an imbibition of the colouring matter of the blood, and takes place after death?" (1) (P. 313, *Rigaud.* "*Arteries.*") Not only may an absence of vascularity exist, but Tonellé states that the inner coat of an inflamed vein may be actually *white;* and he maintains that this appearance, both in veins and lymphatics, is a full proof of a state of inflammation. He also mentions that the veins thus affected in uterine phlebitis are in a state of great dilatation and tortuosity, and that these are often connected with others in a

dependence upon inflammation. (*a*) That, the par vagum is not necessary to the process of digestion; or, whether necessary or not, this vital process is carried on by the laws of chemistry, and may be just as well accomplished by chlorine, pepsin, oxalic acid, &c., and as well out of the stomach as in it, or even in fresh wounds, or by the putrefactive process. (Pp. 101, 105, 186.) That, typhoid fever, like the soul of Des Cartes, has its peculiar seat in a gland of unknown use; an analogy which may have been farther suggested by the doctrine of Gum, (*b*) Greding, (*c*) and Morgagni, (*d*) that the calculi of the pineal gland is sometimes the *cause*, and again the *effect* of diseases of the mind, and this because they were found rather abundantly in the brains of a couple of fools. (*e*)

And, were we to turn for a moment to some of the coincident improvements in therapeutics, we should find in animal magnetism, and in the profundities of the humoral pathology, a practical exemplification of the wisdom of the age. (Vol. i. pp. 397, 632.) That the foregoing doctrines will continue to prevail, more or less, we have no doubt. New creations of inferior by superior animals, or the more uncompromising doctrine of "spontaneous generation," will never want their advocates with those who recognise no "vital principal" in living matter, and, therefore also, no uniform laws in diseases whose various phenomena and results are the same, or very analogous. The creation of animals by galvanism, &c. will still go on; and, coming to the chemistry of life, it can scarcely be expected that a philosophy so universal shall not be hourly heralded forth, and gravely approved by the magnates of the land.

(1) Cruveilhier Anat. Patholog. liv. 11. 17. And so, not unfrequently, of the arteries. *See Hope in Cyclop. of Prac. Med. Arteritis; and Laennec, and Dutrochet.* — The absence of vascularity in the inner coat of the veins led Laennec, and some others of the French school, to the belief that it is not subject to inflammation. — Morgan supposes the colour, when present, to reside in the vessels. — *Principles of Surgery, p.* 107. And so Bertin. — *Diseases of the Heart, &c. p.* 46. — The veteran Clutterbuck's statement is also entitled to a notice, who says that "the lining membrane of the larger veins is found to be highly reddened, (in violent cases of fever,) so as to resemble the tunica conjunctiva in an inflamed state." — *Essay on Pyrexiæ, &c. p.* 63.

(*a*) M. Louis, in Researches on Typhoid Fever, vol. i. pp. 324, 363, &c. — (*b*) De Lapillis Glandulæ Pinealis, &c. 1753. — (*c*) Adversaria Med. t. ii. p. 522. — (*d*) De Caus. et Sed. Ep. 5, a. 12, and Ep. 61, a. 3, 4. — (*e*) See, also, Rhæderer, de Cerebro, 1758; and Wenzel, Obs. sur la Cervelet, &c. p. 165.

state of disorganization. (1) This is conformable to a general remark of Bichat, who says that,—

"The serous membrane may have been very much inflamed during life,—the skin in erysipelas, — the cellular texture, — the mucous, as seen in angina, —and yet exhibit almost their natural appearance after death; and that he should have been often tempted in opening dead bodies to deny the existence of any affection which had been real." (2) (See Hunter, p. 319.)

The foregoing remark is also valuable as showing the light in which morbid anatomy was regarded by the founders of the anatomical school.

Here is an example of what we have now said, from another source. In a case of crural phlebitis, —

"At the point which was in contact with the pus, the internal coat was *paler* than it should be. *Above*, it was *stained* and discoloured as far as the iliac vein, and even to the cava." "It is *singular* that the coats of the vessel, which, for full three inches was filled with pus, should have been *so unaltered* as they were." (3)

We seek in vain for other modes of action which determine the symptoms, or the products, or the changes of structure which are allied to the results of inflammation, although there be wanting some of the more usual characteristic phenomena. Analogies may be sometimes obscure; but there may be connecting media in the wide range of inflammatory actions which shall illustrate an instance of doubtful pathology, and rescue the case from an imputed "lesion of nutrition." We shall also find in the influence of remote causes, in the structure and physiology of different organs, in constitution, &c. much that will explain the varieties. To these causes, also, are to be referred those modifications of inflammation which have been distinguished into sthenic and asthenic hyperæmia, whose loose or proper modes of treatment are held to be conclusive against the common nature of their pathology. But, although stimulants may aggravate, where depletion will subdue the affection, and vice versa, there are not wanting numerous instances, as in burns, colds, erysipelas, &c., where either method may succeed. (Sec. XV.) Where common physical products arise, which are conceded in one instance to be the offspring of inflammation, least of all should they be ascribed in another case to an "opposite condition of disease," by those cultivators of morbid anatomy, who hold that this science reveals the secrets of morbid actions.

(1) Des. Fièv. Puerp. Obs. à la Maternité, 1829, Archiv. Gén. 1830.
(2) Gen. Anat. vol. ii. p. 20.
(3) Lond. Med. Chir. Rev. July, 1828, p. 426.

The operation of the vital powers gives rise to all the phenomena of health. If the action of those powers be modified, the phenomena may be varied, and other results may follow, without any apparent change in the instruments of action. This is especially exemplified in idiopathic fevers; and where a change occurs in the physical state of the blood-vessels, as in inflammation, its various modifications, as in scrofula, lues, rheumatism, gout, phlebitis, croup, cancer, erysipelas, or from miasmata, the virus of snakes, bees, mad-dogs, &c. it cannot be inferred from any apparent peculiarities. Where so much is palpable, how shall we limit the phases of inflammatory action? The morbid products may alone afford a diagnosis in a generic sense, which the symptoms, constitution, the seat of the affection, &c., had more clearly and specifically indicated from the beginning; and in idiopathic fever, in the primary stages of smallpox, scarlatina, &c., it is through the vital phenomena alone that we attain any knowledge of their pathology. And so in most of the "neuroses." The various narcotic poisons produce one uniform, and generally but one, anatomical lesion, venous congestion of the brain; and yet how perfectly dissimilar may be the symptoms. The scalpel discloses nothing to explain the causes of the peculiarities. Can we produce the inflammatory part of smallpox, measles, &c., as we may common inflammation? Will antiphlogistics, or other means, shorten their duration, at least in a general sense? (Vol. I. p. 470.) Have they not a natural termination which belongs not to common inflammations? The modifications are owing, mainly, to the "hidden vertues," as Woodall expresses it, of the remote causes. (Vol. I. p. 666.)

"There are," says Lord Kames, "many species of things, whose specific difference cannot be expressed in language, even when it is evident to the sense, or to the understanding. Thus, green, red, and blue, are very distinct species of colour; but who can express in words wherein green differs from red or blue?" (1)

"It is obvious that the mere difference in the capacity of vessels, though it might, to a certain extent, amount to redness and swelling, could never enable us to explain the alteration of function any more than a knowledge of the size of the capillary vessels would instruct us as to the mode in which their secretions, &c., are performed during health." (2)

When morbid anatomy shall overstep the limit which is prescribed by the laws of vitality, and attempt to resolve the elements

(1) On the Progress of the Sciences, b. 3, *Appendix*.
(2) Edin. Med. and Surg. Jour. vol. 30, p. 317.

of disease into structural changes, it will incur an opposition that may turn us back upon symptoms as the only safe guide to practical and philosophical conclusions. Changes in the vital properties, and change of function, must be always antecedent to change of structure, however the latter may ultimately modify the symptoms. "Affections, demonstrably inflammatory, may be sustained for many years without producing disorganization." (1) The physical products of disease can, at best, only denote the nature of an antecedent functional action in which the essence of the disease consists, and which has more or less terminated in the particular part when the lesions of structure and morbid depositions have taken place. On the contrary, if disease consist in structural lesions, as maintained by Cruveilhier, Louis, and many of the French school, where is the remedy? To what practical result does morbid anatomy conduct us, if it inculcate such a doctrine? Organic lesions are to the physician what they are to nature, — ulterior results; and they are equally unacceptable to both. If the positive symptoms of inflammation are to be set aside from want of some of its terminations, or even of vascularity, the foundation of practical medicine will be swept away, and clinical lectures should be confined to the dissecting room.

We agree with Mr. Travers, that the anatomical doctrines of the French school, especially on the subject of inflammation, are pernicious "heresies," affording

"The most convincing proof that the opinions of Hunter are not understood." That, however, "the determination of blood to a part neither constitutes excess of nutriment nor the act of inflammation;" "that the quantity of blood circulating in a part has, in fact, only an indirect reference to the healthy function, and to the morbid function a relation *as subject to variety as the changes of which the part is susceptible.*" (2)

Finally, we believe that what are usually denominated the "terminations of inflammation" should be received more or less according to the true import of that expression. That the effusion of lymph, or serum, or pus, or the supervention of gangrene, denote a greater or lesser abatement, or modification, of the inflammatory action upon which they have primarily depended. The direct tendency of all the products is to subdue the morbid condition. It is nature carrying on the work of depletion in the very instruments of disease, whilst, as in the effusion of lymph,

(1) Davis's Principles and Prac. of Obstet. Med. part 20.
(2) Further Inquiry into Constitutional Irritation, pp. 214, 220.

she may simultaneously accomplish another great final purpose. Where vital action remains, there may be, therefore, varying but modified degrees of the original pathological state during the progress of the effusions, whilst each of these effusions will be modified in its character according to the precise condition of the vital properties of the part. (See *Inflammation*, Sec. II. and Vol. I. p. 627.) During the effusion of perfectly natural pus, we apprehend that the original action has principally subsided in the immediate instruments by which the purulent matter is generated, or from which puriform granulations may have sprung. So of depositions of oseous matter in textures to which this product is not natural. The lymph and serum, which follow inflammatory action, are not like the lymph and serum of healthy parts; and when the former forms the bond of union between divided parts, it only acquires the nature of the parts after the original formation has either been absorbed and new lymph deposited, according with a natural physiological process, or, till assimilated to the healthy parts by acquiring the organization of those parts. All these conditions, however, depend upon a morbid process; and, however inflammation, as such, may have subsided, it has been nevertheless antecedent and as necessary to the actions on which those results depend, as the specific actions are to the results themselves.

For the reasons now assigned, it is evident that vascularity will have been more or less reduced whenever any of the common products of inflammation shall have supervened; whilst these considerations will enable us to understand the ground of that philosophy which ascribes ascites or other dropsical effusions, in one instance to inflammation, and in another to a process like mechanical filtering. (Sections 15, 16, and Vol. I. p. 180.)

SECTION VIII.

ORGANIZATION OF THE VEINS, &c.

The veins possess a peculiar organization.

"Their external fibrous coat," says Mr. Travers, "is thinner and looser in texture than that of arteries. It is abundantly supplied with blood-vessels, and

is so closely interwoven and condensed with the inner coat, as to make them in a degree inseparable. Bichat, who particularly notices this fact, remarks, that it is a character distinguishing the venous from the arterial texture. The middle and internal coats of arteries, on the contrary, preserve a distinct character, and though closely adhering, do not appear, in any degree, to intermix." (1)

The abundance of the vasa vasorum, and the peculiar structure of the venous coats, denote peculiar functions, and a liability to changes of action; and although the sanguineous vessels are chiefly to be seen in the external coat, whilst the inner is more like the serous membranes in respect to its sensible vascularity, they are so interwoven that morbid action interchanges more readily than among other tissues. These conclusions from anatomical facts are borne out by observation, if we compare together the vital manifestations, in a morbid sense, of the two systems of blood-vessels. Thus, again, Mr. Travers, whom we quote particularly on account of his excellent conceptions of the uses of morbid anatomy:

"The most material fact, however it may be explained, is this. The inner, or lining membrane of veins is subject to diffused or continuous inflammation; that of the arteries very rarely, if ever. I am aware that a preternatural redness of the interior surface is sometimes observed to run through the course of an artery;" &c. "The inner membranes of arteries and veins are susceptible of adhesive inflammation. That of the former is defined, whether excited by pressure or by wound, or occurring spontaneously. I never saw the inner coat of an artery furred with lymph; and even when lymph is deposited in quantity sufficient to obstruct the current of blood, the deposit occupies a narrowly defined space, and the inflammation, by whatever cause excited, or however acute, is similarily circumscribed. In the veins, on the contrary, the inflammation extends from the point of irritation towards the heart, and from branch to trunk. The lymph coats the veins like a fringe; and though the quantity effused is sometimes sufficient to obstruct the tube, the inflammation is often bounded by the obstruction." (2) And so Mr. Hunter. (3)

"The cicatrization of veins," says Bichat, "is the product of inflammation. When an artery is tied, its walls must adhere before our objects can be attained. But nothing is more difficult or slower than this process, on account of the indisposition of the arterial tissue to become inflamed. Thence, hemorrhages are frequent, even after 20, 30, or 40 days. This indisposition in the arterial tissue to become inflamed, should make us vigilant after tying the larger arteries. The artery may even become obliterated without inflammation; and I am disposed to believe that such cases are more common than from inflammation. But the veins always adhere very quickly (avec promptitude) when they are tied, and their wounds cicatrize immediately. In large wounds, it is generally unnecessary to tie them, not only on account of their valves, but because their extremities contract, rapidly inflame and adhere. Venous hemorrhage

(1) Cooper's and Travers' Surgical Essays, p. 192. (2) Ibid. p. 194.
(3) Med. and Chirurg. Trans. Vol. i. p. 18.

also, takes place immediately, and not after so long a time as from the arteries. *Every thing proves that the vital activity is more strongly marked in the venous than in the arterial system, in respect to the tonic powers.*" "Among all the bodies I have examined, I have found but very few in which there were traces of inflammation in the arterial texture." "It is rarely the seat of diseases, either acute or chronic, on account of the obscurity of its vital properties. It can exert, then, but a very slight influence upon the other organs. (1)

"Autant les veines," says M. Dubois (d'Amiens,) "sont disposées à s'enflammer par le fait de piqûres, de déchirures, de sections, etc.; autant les artères sont réfractaires à l'action de ces agens." (2)

Subsequent researches have constantly illustrated the foregoing statements, at least in a general sense. Farther on we shall offer a remarkable example where nearly all the veins in the body became almost simultaneously affected with phlebitis, consequent on parturition. Dr. Lee has recorded a case in which the veins of the uterus, the external and internal iliac, the vena cava from the commencement of the common iliac to the entrance of the hepatic veins, the right spermatic vein, and the veins of the left inferior lobe of the lungs, were in a state of inflammation. (3) These, and analogous cases are important in this place as showing a predominance of sympathy in the venous tissue, and thus conducting us to a better understanding of the manner in which venous congestions spring up in consequence of disease of some remote organ, as, when cerebral congestion supervenes or gastric derangement, &c. The disposition of phlebitis, especially when spontaneous, to pervade the venous tissue, by contiguous or remote sympathy, is now well understood.

M. Cruveilhier states, that he has witnessed many fatal cases of phlebitis after operations by trepan for the removal of diseased bone. In these instances, we must suppose that a low degree of inflammation had existed antecedently in the veins. Phlebitis sometimes follows the extraction of a tooth. It is frequently consequent on ulcers, and other conditions of inflammation in the surrounding parts. It often attends carcinoma of the breasts (4) Sir C. Bell had seen twenty cases. Mr. Hewson speaks of two cases of phlegmasia dolens supervening on suppressed menstruation.

"Mr. Harrold, of Chestnut, writes us," says a Review, "that, when attending the London Hospital in 1790–1, phlebitis was of very frequent occurrence, and the cause was at length traced to the use of soiled sponges applied to the incis-

(1) Gen. Anat. &c. vols. i. and ii. pp. 333, 352, 424.
(2) Pathol. Générale, t. 2. p. 290. (3) Med. Chir. Trans. vol. xix. p. 47.
(4) Dr. Ramsbotham's Lectures on the Diseases of Women and Children.

ions, to cleanse them before the bandage was applied. Sir W. Blizzard gave an order that no such sponge should be employed, and the phlebitis disappeared." "It has occurred to us," says the same Review, "to witness several instances of idiopathic inflammation of the saphena vein. In one instance, it followed exposure to cold; (1) (See APPENDIX 3.) In others, it was without any ostensible cause. In a case of extensively varicose condition of the saphena veins, phlebitis was rapidly induced by tapping the veins rather smartly with the fingers, which proved fatal in a few hours; a good example of the danger of even the most trivial interference with the veins in some individuals." (2)

"A ligature," says Mr. Liston, "should never be applied to a vein unless as a last resource." (3)

Drs. Graves, and Stokes, have related cases of phlebitis of the lower extremities, supervening upon, or connected with, intermittent and other fevers. (4) "In five cases, or one in 60, swelling of one leg, from well marked phlebitis, occurred at a late period of typhus fever," in the Massachusetts Hospital, at Boston. Phlebitis of the lower extremities, supervening upon idiopathic fever, has not been infrequent in the practice of Dr. Tweedie. (5) The same is affirmed by Dr. Cheyne of several patients under his care, who were suffering dysentery. (6) The relation of these facts to the venous congestions which attend the same affections will become more obvious hereafter. And here it is important to consider, that the fevers which were attended with the foregoing cases of phlebitis, are remarkably distinguished and aggravated by venous congestions. This leads to the farther consideration whether the predisposing causes of the phlebitic affection do not also lay the foundation of a lower degree of inflammation in other veins whose vital properties have a different modification; and how far, also, it may be probable that venous congestion was antecedent to the phlebitis, and a sympathetic cause of the latter; since, also, it is in tissues of the same nature, especially in the venous, that sympathies abound most.

(1) Dr. Ramsbotham has also known crural phlebitis to have been produced by "the local exposure of the limb to cold." (*a*) —Sir H. Halford, in April 1832, read an account at the college of physicians of three cases of crural phlebitis which were probably produced by cold. It is especially worthy of note, as illustrating the sympathy amongst the veins of distant parts, and as showing the probable nature of venous congestion, that these three patients died of cerebral congestion, which Sir Henry thinks was connected with the antecedent crural phlebitis.

(2) Med. Chir. Rev. Lon. vol. xxx. p. 289; vol. xxix. p. 264.

(3) Elements of Surgery, p. 96. (4) Dublin Hospital Reports, vol. v. p. 28, &c.

(5) Edin. Med. and Surg. Jour. vol. xxx.

(6) Report of the Whitworth Hospital.

(*a*) Lectures on the Diseases of Women and Children.

Later observations have confirmed the opinion of Carmichæl, —

"That phlebitis is a frequent cause of such deaths as follow amputations and other extensive wounds." (1)

"In the dissections conducted by Dobson, Bingham, and Crofton, after the battle of Waterloo," says Mr. Hennen, "we met with no less than twelve cases where the veins were inflamed." (2)

Morbid poisons, of all kinds, readily excite diffuse inflammation of the veins when applied to their inner surface, — more readily than when inserted in the texture of any other tissue. If the veins be injected in a direction from the heart, they immediately become enlarged, and suppurate; blood is extravasated, and some of the phenomena of typhus fever ensue. It is worthy of remark, also, that these irritants, when thus injected, have no such action upon the arterial, or other tissues, unless of a secondary nature.

The experiments made by Cormack, with creosote, led him to conclude that "there is a very marked difference in the effects produced by introducing this poison into the veins." A drachm and a half introduced into an artery did not destroy "a rather large sized sheep-dog," whilst thirty drops injected into a vein of "a large mastiff immediately arrested the action of the heart," "and the animal died in a terribly convulsed state." (3) And so of saline injections in the cholera asphyxia, producing venous inflammations, (p. 308.)

Various other proofs to the foregoing effect will be mentioned. Now, this way

"*Of discovering the presence or absence of this or that vital force in a part by the affections which increase that force there, deserves an important consideration in the examination of these forces. Authors have not employed this method of discovering them.*" (4)

It is upon this principle that we explain the absence of disease in the genital organs of both sexes, before their vital forces, and their sympathetic relations to the system, are fully developed; why, in manhood, they become obnoxious to disease, which sheds abroad its powerful influence over the system; and why the glands of Peyer have nothing to do with the constitutional symptoms of "typhoid fever;" and why, because ergot acts upon the uterine muscles of the impregnated female, the analogy will not hold, as supposed, in restraining hæmorrhage in the unimpregnated, or from other parts, and may fail in the cure of

(1) Dublin Trans. Venous Inflam. p. 364.

(2) Principles of Military Surgery, p. 188.

(3) On Creosote, &c. ex. p. 6, 10. (4) Bichat, ut. cit. vol. i. p. 416.

leucorrhœa. The principle is of vast importance, not only as respects our pathological inductions, but therapeutical treatment. Upon it, we explain the facts which we have now related to show the susceptibility of the venous tissue to inflammatory affections, and to their rapid diffusion. They grow out of a principle, as we shall endeavour to show, which is intended to subserve an important function in venous circulation. And since venous congestion is a disease of very frequent occurrence, and often manifests itself in a sudden manner, if it depend on our pathological cause, we grant that the foregoing principle should be reasonably shown by physiological facts to exist in the natural state of the veins, and that there should be found something in the phenomena of acute and chronic inflammations of these vessels which shall also denote that peculiar susceptibility, and a predominance of their vital actions.

We had originally attempted a farther illustration of this part of our subject, by endeavouring to show that the "secondary abscesses" which supervene on phlebitis, are the result of sympathetic influences which prevail in the venous system, when one part is morbidly affected. Want of room has excluded most of our remarks. Some important light has been thrown upon the subject by Cruveilhier, (1) Douglass, (2) Roots, (3) and others, and more recently by our able Watson. (4) The secondary abscesses are generally ascribed by those who defend the doctrine of sympathy to inflammation of the capillary veins; and we may remark, that in the limited opportunities which we have enjoyed, we have found the larger veins in a state of preternatural dilatation; and there are some intimations by Dr. Douglass, that such had been his experience. (Pp. 248, 251, 279, &c. and Vol. I. p. 524.)

From the foregoing considerations, then, it is not difficult to understand how greatly the vital properties are developed in the venous system, and how extensively the principle of sympathy predominates in all parts of it; or, that its inflammations in every degree will be liable to produce corresponding derangements in remote parts of it. In the progress of our inquiry a great variety of facts will be introduced to sustain the foregoing conclusions.

(1) Anat. Patholog. liv. 12, p. 9. (2) on Phlebitis, &c. (3) On Phlebitis, &c.
(4) The American Jour. of Med. Sciences, vol. xxi. p. 58.

SECTION IX.

ANALOGIES SUPPLIED BY VARIX, HYPERTROPHY OF THE VEINS, ETC.

HAVING thus seen that the veins are remarkably organized, that they are highly endowed with "the tonic forces," that they are peculiarly susceptible of diffuse inflammation, and to its propagation by remote sympathy, we will now examine some common affections which are allied to venous congestion, and where various gradations of inflammation are found to exist. To this will succeed the consideration of active phlebitis. It will be our purpose in doing this to establish the fact, that the enlargement of the veins in all these affections depends essentially on inflammation of their coats. This conclusion will form another link in the chain of evidence, that the analogous phenomenon in venous congestion is owing to a similar cause.

"Uncertain marks, together with affinities, which others have not distinctly perceived, are very often the means by which another passes from the known to the unknown. He examines these marks and affinities, till the knowledge of simple and compound cases enables him to conclude on the resemblance of the whole, from the similitude of its parts." (1) (P. 300, and APPENDIX II. on *Analogy*.)

We begin with considering the pathology of varix, and hypertrophy of the veins. We would premise, however, that it appears to be compatible with a low state of venous inflammation, when not seated in an important organ, and independent of remote causes which act upon the constitution, that no continuous or remote sympathies shall be superinduced. This would appear not only upon general principles, but from daily observation. It is well shown by Mr. Mayo in his treatment of varix by caustic; (2) and particularly by Davat's, (3) Lallemande's, and Velpeau's mode of producing obliteration of the vein by the insertion of pins; as suggested to them by Mr. Philips' operation upon arteries by the needle, or, more probably, by Hippocrates, who says, *verum varicem ipsum alias atque alias compungere oportet, ubi oportunum esse visum fuerit.* (4) So, also, by Brachet's method of treating varicocele by pressure; and more

(1) Zimmermann, on Experience in Physic, vol. ii. p. 25.

(2) Outlines of Human Pathology, p. 433, &c.; and Lon. Med. Gaz. 1828. A method, however, which was formerly pursued. — *See Lieutaud's Synopsis, p.* 386.

(3) Trait. des Varices, &c. 1836 (4) De Ulceribus, ver. 255.

recently by Fricke's plan of passing a fine thread through dilated veins; or Delpech's (1) treatment of circocele; or Brodie's (2) plan of dividing varicose veins under the skin, the frequent safety of which has been particularly shown by Carmichæl. (3) In either case, circumscribed inflammation is indisputably produced. The principle will be seen to be very important to a full understanding of our subject, in its relation to the sympathetic results, continuous and remote, which may spring from different degrees and modifications of venous inflammation.

Another important reason will have been inferred from our last section, why sub-active inflammation in the veins of "non-vital" parts, or as aggravated in the foregoing manner, is often unattended by violent symptoms; viz. that the vital properties are less strongly pronounced in such veins than in those of the organic viscera; though certainly in the latter case much is due to the morbific influence of the entire organ. And yet there are peculiarities which render the superficial veins, and those of the extremities, liable to the severest forms of phlebitis; when the constitutional symptoms approximate those from congestions of the organic viscera.

In the hemorrhoides (4) we have an example where venous inflammation exists in a more active state than occurs in congestion; and where the latter affection may be intimately associated with it, as affecting other parts. We speak of the true hemorrhoidal varix; not of the tumours which have been distinguished from it by modern surgeons; though we doubt whether the distinction be always well founded. Here we may find in the venous tissue the plainest characteristics of inflammation. There is pain, heat, redness, throbbing, and, ultimately, alterations of structure. We witness in this affection, particularly, the progressive degrees of inflammation, from that which constitutes the most simple form of congestion to the full development of

(1) Bulletin Gén. Thérapeutique, Oct. 1831.

(2) Med. Chir. Trans. Lon. vol. vii. p. 202.

(3) Trans. of College of Physicians, vol. ii.

(4) Syme, after stating certain mechanical causes of hemorrhoids, which certainly act as excitants, admits with other intelligent observers, that "a *predisposition* to this disease frequently exists, since in some people it is induced much more readily than in others." These persons, also, "suffer from it at the age of puberty." (*a*) We have met with it, in several instances, among infants, affected with sub-acute muco-enteritis.

(*a*) On Diseases of the Rectum, p. 16.

active disease. In its mildest state, there is only that moderate enlargement of the veins which is the usual attendant of congestion, and there are no acute symptoms. But, as the inflammatory action advances to a certain limit, those phenomena increase in a corresponding ratio. If the varix have been connected with congestion of the liver, or of the upper tract of the mucous membrane, as either progresses, the phenomena of the other become aggravated. This may be effected by irritating cathartics, and the more so in proportion to their activity and the frequency of their repetition. There is then no pressure from fœces, and leeching is the most efficient remedy for the whole condition of the disease. (1) The venous tissue, also, becomes ultimately thickened, or the inner coat may be abraded, or otherwise deranged; and then the enlargement may obviously increase from these causes, and remain after the subsidence of the original lesion. That it is not owing to the pressure of the venous column appears, in part, from the foregoing considerations, and from others which will be set forth hereafter.

"It is rare," says Bichat, to find "dilatations (varices) in the superior mesenteric veins, splenic, gastro-epiploic, &c. whilst there is no part in which they exist more often than in the hemorrhoidal."

This, we apprehend, is owing to the peculiarity of the vital forces at the termination of the rectum.

"In Dr. Cheyne's cases of dysentery," says Dr. Lee, "it is highly probable the disease (phlegmasia dolens) commenced in the hemorrhoidal veins," &c.; as, also, "the cases of Dr. Tweedie, Graves, Stokes, and Macann." (2)

When hemorrhoids follow venous congestions of the liver, it is a matter of sympathy. Portal says, —

"The cutaneous veins of the inferior extremities swell, and become sometimes varicose from congestions of the liver, and that they are removed by cathartics." A learned reviewer, in quoting this passage, remarks, that "we cannot, for ourselves, comprehend how the *obstructed* circulation of the vena portarum should operate in determining a varicose state of the superficial veins of the legs." (3) But what are the facts?

"I was surprised," says Morgagni, "to find, that although the hemorrhoidal

(1) We often hear of attempts being made by leeches to recall a suppressed hemorrhoidal flux. Did the practice ever succeed, or is it founded on principle? See Delaroche's philosophical *Traité des Hemorrhoides*. He regards the effusion of blood as a secretion.

So great is the sympathy between the terminations of the mucous membrane, that we have been long familiar with instances where the smoking of tobacco has immediately developed an attack of the piles: — *See vol. i. p.* 61, *note.*

(2) Cyclop. of Prac. Med. Art. P. Dolens, p. 347.

(3) Eclectic Repertory and Analytic Rev. vol. vi. p. 50.

tumours contained a large quantity of grumous blood, the smallest vessels, only, communicated with them. It was apparent that some very small vessels had undergone this dilatation." (1)

If we look at the less complex forms of varicose veins, we shall find the same evidences of inflammation. But, before we examine this subject farther, it may be expedient to state the received doctrine in regard to the pathology of varix.

"The cause of varices," says Mr. Liston, "is obstruction to a free return of blood." (2)

Sir B. Brodie says that this is sometimes the case; but "in a majority of cases it appears to be a mere weakness in the coats of the veins, rendering them incapable of supporting the weight of the blood." (3)

"We possess," says Craigie, "no very precise facts on the exact change which takes place in the venous tunics; whether it be mere dilatation or injury of some kind, and rupture of the proper venous coats." (4)

And thus Meckel: "Varices mostly arise from mechanical obstacles which oppose the free course of the fluids." (5)

Mr. Hodgson says, "varix may depend on obstruction to the circulation, when the vein becomes dilated by the accumulation of blood. Sometimes it appears to arise from a preternatural weakness in the coats of the veins, as in those instances in which, without any evident cause, it exists in various parts of the same person." He also thinks there may be a rupture of the valves in some cases. (6)

Sir C. Bell attributes varix to "mechanical pressure; or, I should, perhaps, say hydraulic, since the distension is produced by the length of the column of blood." (7) (Pp. 251, 257.)

These quotations lead us to stop, also, for a moment, to consider farther the imputed mechanical cause; but we would first inquire whether in "those instances in which, without any evident cause, varix exists in various parts of the body," there be any other proof of a "preternatural weakness," and what, also, is the physiological import of this expression? (Pp. 153—154, 243, 260—261, and Sec. 15, 16.)

Again, pressure from ligatures, indurated fæces, &c. by producing accumulations of blood which excite irritation in veins that are predisposed to sub-active inflammation, appear to be sometimes exciting causes of varix. So, also, when an unusual volume of blood is determined upon particular veins by the obliteration of a principal trunk, they of course become mechanically distended.

(1) De Sed. &c. Epis. 32, a. 10. (2) Elements of Surgery, p. 96.
(3) Lectures in Lon. Med. Gaz. Oct. 1837, p. 185.
(4) Elem. of Gen. and Patholog. Anat. p. 132.
(5) Gen. and Patholog. Anat. vol. i. p. 148.
(6) On Diseases of the Arteries and Veins, p. 537.
(7) Lectures in Lon. Med. Gaz. vol. xiii. p. 427.

Their natural condition, however, in other respects, may be, and generally is for a certain time, unaltered. This is shown in the cases related by Lieutaud, and Manec. In many cases of obliteration of the vena cava we can perceive no physical difference betwixt the state of the veins concerned in the new circulation and that of varix in its early stages. And yet how rarely are any of the ultimate consequences of varix manifested in the former instance. In one case there is truly a mechanical obstruction operating, and yet the veins maintain a state of simple dilatation. Sometimes, it is true, a solitary vein puts on the distinctive characters of varix; but this only proves that mechanical distension of the veins is but little capable of producing that action on which their organic changes depend.

That pressure, or other obstructions, are not frequent causes of varix, or of those structural changes which take place in that affection, or in hypertrophy of the veins, and that the dilatation is mainly dependent on other causes, appears, also, to be evident from the fact, that these affections prevail only after adult age, and most in particular constitutions, and under particular circumstances of the constitution, and from their appearing sometimes in many parts of the same subject. It is also evident from the more frequent absence of organic changes when the veins in other instances become dilated, whether the dilatation arise from pressure or other causes. It appears from the constant exemption of varices in the lower extremities among the great proportion of those who are in the habitual use of garters; whilst others who have never employed them have suffered very large varices. It appears from the frequent dilatation and hypertrophy of the superficial veins of pregnant women after a certain age, whilst "it is very unusual in young females, even during repeated pregnancies:" and it appears that pressure is not an exciting cause in those cases, not only from the constitutional cause just stated, but from "the appearance of varices at the beginning of pregnancy," and long before the enlargement of the womb can impede the circulation; whilst "nothing is more unusual than a varicose or dilated state of the veins of the lower extremities in enlargements of the abdominal viscera." (1) It appears from an obviously constitutional fact, that children, with the exception of occasional hemorrhoids, are exempt from varix. (2) It appears

(1) Delpech, Précis des Maladies, Chir. t. iii.

(2) This has been known from early ages. Thus Hippocrates: "pueris vero

from the true disorganized varix having sometimes affected the jugular veins where no obstruction has been detected, and where the power of gravity equally contradicts the hypothesis of "relaxation."(1) It appears from pressing upon a varicose vein, and rubbing the blood out of it, when the current of blood follows the finger with the same velocity as in other veins.

Pressure, however, must necessarily increase disease when once begun in the veins; and such, also, will be more or less the tendency of gravity in the ascending vessels. The stimulus of distension may also produce active phlebitis.

What, then, are the ordinary causes of varix? A great proportion are consequent upon injuries, or arise during some inflammation of the skin, or in connection with some constitutional irritation. Here are seven consecutive cases by M. Davat.(2) In the 1st case, the knee was dislocated, when numerous and painful varices, (parsemées et nombreuses) immediately supervened. In the 2d, numerous varices upon the leg and thigh followed the kick of a horse. In the 3d, cause not ascertained. In the 4th, luxation at the ilio-femoral articulation was an immediate cause. In the 5th, the varices supervened during pregnancy, the woman having previously borne many children. In the 6th and 7th, causes unknown.

"An officer," says Mr. Hennen, "received a wound from a musket ball in 1814. The ball entered the right leg externally at about three inches below the head of the tibia. In 1818, the wound was perfectly cicatrized; but from the roots of the toes up to the crest of the ilium, the limb was completely varicose. It was very painful, *especially on a change of weather*." (3) — This case should be compared with that of Bressoles farther on.

In a case by Mr. Hutchinson, the patient had received a blow

herniæ accedunt, et *viris varices*." — *L. de Aere, etc. s. i. v.* 140. — Again: "Morbi hi ante pubertatem non fiunt, podagra, nephretis, *varix circa tibiam, hæmorrhois*," *&c.* — *Coacæ Prænot. s.* 3, *v.* 139 — 141. He everywhere manifests a familiarity with this affection, and ascribes it to causes of a vital nature. "Those that have bald heads are not liable to large varices;" — *Aph. s.* 6, *v.* 40, — but, "if the varices do not become large, they grow furious." — *De Morb. Pop. l.* 2, *s.* 5, *v.* 37. — "When varices *befall the mad, the madness is cured*." — *Aph.* 6, *v.* 25. — "They are brought on by dysentery," — *de Vict. Rat. in Morb. Acut.* § 4, *v.* 278. — and "are cured by long continued dysentery." — *de Articulis, s* 3, *v.* 6.

(1) Morgagni, Michaëlis, and others, relate cases in which pulsation, and fatal hemorrhage, attended this state of the veins, and where it was apparent that pressure had not existed as a remote cause. The hemorrhages to which varix is liable appear, also, to depend on ulceration of the vein, or on a morbid process by which the blood is excreted from the venous coats. See several cases by Haller, in El. Physiol. which reflect a good deal of light upon this subject.

(2) Trait. Curatif des Varices. (3) Principles of Military Surgery, p. 188.

upon the shin, of which active phlebitis was the consequence. These examples are not uncommon. Varix is not an unusual consequence of ulcers upon the lower extremities; though the latter are more frequently consecutive, when they sometimes superinduce the active form of phlebitis. Both conditions of disease, as we shall see farther on, are frequent results of ulceration of the mucous tissues. A patient has just presented himself for our advice, having a large and painful varix, extending from the left side of the ankle, up the thigh to the groin. It began twelve months ago, at the ankle, where it had been preceded by an obstinate superficial ulcer, which discharged only a watery matter. The ulcer has now been healed for many months, but the suffering has been uninterrupted. We referred this patient to Dr. J. K. Rodgers for the benefit of his surgical skill. An operation has been successfully performed by a double sub-cutaneous incision.

Analogous to the foregoing case, and with only a difference in the degrees of inflammation, is one of active phlebitis related by Dr. Lee.

"A small ulcer above the left internal malleolus gave rise to inflammation of the saphena and femoral, and probably of the iliac, veins; and the whole limb became affected with a hot, shining intumescence." (1)

We shall venture to state one case more of our own, in which erysipelas of the foot was the exciting cause. Simultaneously with the inflammation, the superficial veins of the leg, from the foot to the knee, became enlarged and painful. The usual pain has attended the affection during the four years of its existence. Leeches always afford relief. Such is the sensibility of the veins, that bandages cannot be worn. If it be true as supposed by M. Ribes,(2) and others, that inflammation of the capillary veins is the pathological cause of erysipelas, we may readily understand how the inflammation was propagated in the foregoing case to the larger veins. Nor is the sub-active incompatible with a more active form of inflammation in different parts of a continuous series of veins, as will be ultimately shown, — active phlebitis giving rise to varix. There is also, as will appear, a remarkable peculiarity in the venous tissue as it respects the extension of inflammation; being always diffuse, yet singularly and abruptly limited

(1) Cyclop. Prac. Med. p. 349.

(2) Mémoires de la Société Méd. d'Emulation, t. 8. p. 621. Also Revûe Méd. 1825, p. 13.

in "non-vital" parts, or suddenly passing from an active to a sub-acute state. (P. 337.) Mr. Farrall has related cases like our own, and remarks that, —

"Some will call the disease with which they have been affected erysipelas, or attribute it to accident; but they will generally agree in this, that they were at one period confined with inflammation and swelling of one or both legs, accompanied with more or less fever; that the swelling persisted after the fever was removed, and that on recovering the erect posture, the superficial veins progressively increased in size." (1)

The analogy, in the erysipelatous cases, with inflammation may be fully traced in the well known fact that the most violent phlebitis has originated in erysipelas.

"Sometimes," says Armstrong, "varicose veins seem to be the result of mercury; especially a varicose state of the vena portæ. A friend of mine has often found the vena portæ varicose, with a gray, granulated state of the liver, after the abuse of mercury." (2)

"I have known cases," says Sir B. Brodie, "in which the veins in both legs became varicose *immediately after* very hard walking." (3)

Various analogous causes of varix will be stated in the progress of our inquiry, from which it will become abundantly manifest that the hypothesis which respects their mechanical origin is without foundation.

The pathology of varix should be most successfully investigated by studying its diversified phenomena; and since they appear not to be susceptible of explanation upon any mechanical principle, not even the dilatation, let us inquire how far they supply a proof of inflammation as their cause. We will take, in the first place, a statement of the characteristics as lately given by Mr. Sanson in his lecture at the Trial by Concours at Paris, which appears to be one of the best; whilst the competitor is opposed to our conclusions. He premises the inquiry, —

"What is the state of the veins in varicose affections?" Then referring to a particular case, he says, "it is probable there has been no inflammation of the veins, since the patient has suffered no chill, fever, delirium, pain, or redness in the course of the vessels.'

Now, the absence of the foregoing symptoms does not contra-indicate a low, sub-active, state of inflammation, and the whole are sometimes wanting in acknowledged phlebitis. But what are the general facts?

(1) Dublin Journ. of Med. and Chem. Sciences, vol. ii. p. 218.

(2) Lectures, &c. vol. i. p. 101. — The morbid effects of this mineral are of an inflammatory nature.

(3) Lectures, in Lon. Med. Gaz. Oct. 1837, p. 185.

"In the first stage," says M. Sanson, "there is a simple dilatation of the veins, which are tense, and somewhat painful, especially when the patient is erect. When the veins are examined, they appear dryer than natural. At a later period the pain becomes more severe. The coats of the veins are thickened, and converted into tense, hard cords, and if divided across, they will be seen to have wholly lost their elasticity, and their mouths do not contract. In the third stage they become knotty and flexuous, much increased in thickness and length. Their dilatation is irregular, and the fibres of the middle coat form a hernia through the external one, as sometimes observed in the intestines. The cellular texture, which invests and connects the meshes of varicose veins, becomes, also, altered, being gorged and indurated. It presents a lardaceous appearance, and is very liable to inflammation and ulceration."

Do we hesitate in referring the foregoing organic changes to inflammation when they occur in other parts, especially if they be attended by the symptoms which are here said to characterize varix? But, if more active venous inflammation ensue, M. Sanson only recognises its existence then; and reversing the order of changes, he ascribes it to an extension of that disease from the adjacent cellular tissue. Does not, however, the progress of the phenomena show the origin of inflammation in the venous tissue, and are we not familiar with examples of phlebitis where the inflammation is extensively propagated to the cellular texture? Is not this phenomenon invariable when venous inflammation is artificially induced? "The inflammation of varices from wounds and ligatures is such," says M. Davat, "that it extends to the whole cellular tissue." (1)

"When a cluster of sub-cutaneous veins," says Dr. Craigie, "become varicose, they generally give rise to much pain, swelling and redness of the skin, and if not opposed by suitable treatment, may produce cutaneous inflammation terminating in a bad ulcer. The same process, nearly, may result from the inflammation round a *single* varicose trunk." (2)

The progressive relation between causes and effects, as stated in the last extract, is too obvious for comment. M. Gendrin, in speaking of the organic changes of varix, says,—

"When the dilatation has existed for some time, (depuis long-temps,) the internal tunic of the vein is folded longitudinally, and is rather thinned than thickened. The middle coat is thickened, its transverse fibres more distinct, but its longitudinal ones less numerous, its colour obscurely yellowish. The external coat increased in thickness, and confounded with the middle tunic." (3)

He then proceeds to describe the greater organic changes which take place at more advanced stages of the disease. He

(1) Trait. Curatif des Varices, &c. p. 16. (2) Op. cit. p. 133.

(3) Hist. Anatom. des Inflam. t. ii. p. 50.

does not, however, consider inflammation a primary cause of varix, but only a consequence.

"There is really no inflammation," he says, "till the varix has acquired considerable development," — "till there is a certain distension of the tunics." "The characters of chronic inflammation of varicose veins are opacity, thickening, a grayish red colour of the internal membrane, which is frequently ulcerated."

But these, and other organic changes, are merely the consequences of morbid action, and they may follow very remotely upon it. The beginning of the inflammatory action is only denoted by certain vital signs, and a dilatation of the vessel. Thence is it, as stated by M. Gendrin, that "the anatomical changes are in proportion to the dilatation of the vessel." The dilatation is a primary symptom, and occurs simultaneously with those vital signs which as clearly denote the pathology of the affection, as that pathological state is indispensable to the ulterior results. The contrary of all this is of the school of morbid anatomy. Indeed, the very term inflammation should be limited to that functional modification which precedes and produces the physical results, since they are the terminations of that condition; just as an abscess, or dropsical effusions, are other terminations. Inflammation has often wholly terminated when its consequences are most conspicuous.

M. Andral has divided varices into six species.

"In the first, there is simple dilatation unattended by any other alteration; either affecting the whole length of the vein, or existing only at intervals. This species of varix often *co-exists with the chronic hyperœmia of an organ.*"

In these cases it is probable that the morbid action has been propagated from the affected organ to the veins; and here it is very analogous to the state of the veins in venous congestion. Hemorrhoids, supervening on hepatic congestion, approaches an example. But we shall soon mention other cases, where the action may be traced in a more direct manner. In the remaining species of our author, with, perhaps, the exception of the second, we shall recognise such varieties as manifestly result from inflammation.

In the third species,—

"There is uniform dilatation of the veins, with increased thickness of the parietes. In the fourth, dilatation at intervals, with thickening of the parietes at the dilated points. In the fifth, dilatation with morbid septa, dividing the vessel into compartments. In the sixth, the same as in the fifth, with the addition that the parietes are drilled with a number of minute holes." (1)

(1) Précis Anat. Path. t. ii. p. 402. — This last division probably embraces that

To one or the other of the foregoing species, he refers all hemorrhoidal tumours; which, if depending on inflammation, and from the frequency of their occurrence, it might be expected would oftener exemplify the several varieties. In these several species we also recognise the successive stages of one affection; the various alterations depending on the duration and activity of inflammation. In the last stage, the "morbid septa," and the perforations take place, and it is only at this stage that the veins become knotted, and their coats greatly thickened. The knots have been often ascribed to the valves by the mechanical philosophers; but they are not only too numerous to admit of this supposition, but they are now more incapable of producing the phenomenon which they had failed to present at the early stages; nor has the truth of this conjecture ever been demonstrated in any partial degree.

Here we may advert, for a moment, to the analogy which reconciles any objection to an absence of structural change in the veins in venous congestion. Low degrees of inflammation constantly affect the serous membranes, (to which the inner membrane of the veins is allied, and in which we apprehend the morbid action always originates,) for a great length of time, without producing organic changes. Something, too, may be ascribed to the modifications of inflammation as determined by the nature of the remote causes, &c., all of which, too, has an important bearing upon the symptoms. For these reasons there is rarely suppuration in varix, until the action be exalted to that of violent phlebitis. In varix, the morbid action is only gradually propagated to the other coats, and the principal alterations in structure supervene only after a long duration of the disease.

But our records are beginning to abound with structural lesions of the venous tissue in ordinary congestions of the veins. This we have already shown, in part, and other examples will occur in the progress of our investigation. These are distributed about for the purpose of illustrating specific points of inquiry; and, with this view, we shall here supply an example which connects the pathology of venous congestion directly with that of varix, whilst it shows the entire independence of varix of any obstruction to the circulation. We select it from a work by M. Andral, on account of his luminous analysis of varix, and to show his

variety of disorganization, in which the blood spurts from the veins, as in some cases of hemorrhoids.

particular conclusions as to individual cases. The symptoms were ultimately such as are constantly attendant on bad forms of venous congestion of the brain.

"Veins ramifying over the net work of the pia mater, covering the cerebral convolutions, were very much *dilated at intervals. These were genuine varices.* Their parietes, soft and friable, were torn and reduced to a kind of pulp by the least force. A *layer of coagulated blood*, at least six lines in thickness, covered the *entire upper surface* of the right hemisphere. One of the large varicose veins, which traversed the pia mater on the right, was *perforated.* It presented a large orifice with irregular jagged edges." "This alteration *partly* existed for a *very long time* before *any cerebral* symptom developed itself."

Now for the pathology: "The *dilatation* of the veins of the pia mater, and the softening of their walls, were *certainly* the effects of *chronic disease.*" (1)

We suppose, then, that in all similar cases of varix, the "*dilatation*" and lesions of structure must be equally "the effects of chronic disease;" call it either "hyperæmia" or inflammation. And who will doubt that that "part" of the alteration which had "existed for a very long time" was merely a state of venous congestion, and that inflammation of the venous tissue being the true pathological cause, gradually advanced in particular sections of the vein till it resulted in disorganization? It should be also observed as a frequent characteristic of venous congestion, especially in its early stages, that, in the foregoing case, "the *alteration partly* existed for a *long time* before any cerebral symptom developed itself," whilst there was at last a development of the most formidable and characteristic. (Pp. 139, 219.) A single instance of the foregoing nature is as good as a thousand, and settles important fundamental principles. (Pp. 267, 300, 337, and Vol. I. p. 298.)

When the changes of structure, &c., which prevail in varix, affect other parts, but where there had been no other appreciable evidence of inflammation, they are ascribed by the best pathologists to that mode of disease; even such as are similar to thickening and obliteration of varix. Such, for example, is the case in obliterations of a hernial sac, when produced by the slight pressure of a truss, as described by Mr. Hunter. (P. 319.) So, also, in respect to innumerable cases of adhesive inflammations; and, again, as in tubercle. (2) (APPENDIX V.)

(1) Andral's Medical Clinic on Diseases of the Brain, p. 135. *tr.*

(2) Although Dr. Hope is disposed to admit that the "researches of the present day have justly bred a *philosophical doubt* on the tenet of the ancients, that all accidental productions are the effects of inflammation," (*a*) he thinks that "arteritis may

(*a*) On Diseases of the Heart, &c. p. 165.

We may also notice the frequency with which active inflammation has been set up in varicose veins by ligatures, and even by their section. "The division of varicose veins by the knife, practised by modern surgeons," says Dr. Lee, "is so hazardous a practice, that it has been almost universally abandoned." (1) The operations by compression, and ligature, are wholly condemned by Davat. (2) The Medico-Chirurgical Review, in commenting upon the different modes of operating, remarks, that "in practice they have proved to be much more hazardous than their supporters would wish us to believe. We should be very

be known by certain anatomical characters of inflammation, such as *thickening*, *swelling*, and *puffiness*, of the inner membrane, *effusion of lymph*, on either its free or adherent surface;" &c. without the trouble of ascertaining whether the attendant redness be owing to increased vascularity, or to an imbibition of the colouring matter of the blood, or to its increased fluidity before death. (*a*) Again, reasoning upon the same principle in respect to the causes of morbid depositions in the coats of arteries, he justly concludes that "analogical evidence, derived from other membranes, leads to the belief that chronic inflammation is, in most instances, at least, the main agent in the production of these depositions." (*b*) Bertin expresses the same opinions. (*c*) This, however, is one of the most feeble analogies. Bichat says that osseous depositions are found in seven-tenths of subjects after the age of 60. (*d*) Cooper considers them only morbid in youth. (*e*)

"The best way to determine, beyond doubt, the anatomical characters of acute inflammation of the arteries," says M. Gendrin, "is to produce this inflammation, and to study the alterations," &c. (*f*) And so others. This, however, would only show the consequences of inflammation artificially induced; not according to its natural forms, when it is so variously modified in its symptoms and results by the predisposing and other causes. Even in its artificial forms, those phenomena will be modified by the nature of the *exciting* causes, whether it be a simple mechanical irritant, an acid, an alkali, a morbid poison, &c. If we regard the chronic forms of inflammation, to which the arteries are mostly liable, we see more distinctly the inaptitude of the experiment, and the limited views in pathology of which it is indicative. It is probably impossible to establish, by artificial means, any of the varieties of chronic inflammation which nature is so often instituting in limited parts of the arterial tissues, and which lead to aneurismal tumours, a fragile and inelastic state of the tumours, to fibrinous, steatomatous, cartilaginous, calcareous, and other depositions, &c. As well might we attempt to establish smallpox by the matter of a common phlegmon. (Vol. i. p. 610, *note*.)

If we survey the wide range of inflammation, we observe so great a variety in the phenomena, and the ultimate results, there will appear a manifest difficulty in reasoning, according to our author's precision, from artificial to natural diseases. Art can never embrace the various causes, and perhaps no one of them, which operate naturally in determining the modifications of any generic disease.

(1) Cyclop. of Prac. Med. Art. Varix.

(2) Trait. des Var., &c., 1836.

(*a*) In Cyclopædia of Prac. Med. *Arteritis*.
(*b*) Diseases of the Heart, p. 162.
(*c*) Diseases of the Heart, &c. p. 47, et. seq.
(*d*) Gen. Anat. vol. i. p. 314.
(*e*) Philos. Trans.
(*f*) Hist. Anatom. des Inflam. t. ii. p. 9.

loth to meddle much with veins." (1) And thus the London Medical Gazette:—

"Mr. Cooper, rather an apologist than otherwise, for the operation observes: 'The case calls our attention to the question, under what circumstances is the attempt to cure varicose veins of the leg justifiable by any means attended with a risk of exciting this formidable disorder?'"

Upon which the reviewer remarks:—

"Perhaps Mr. Cooper's advice might be extended to not performing the operation at all in *any* constitution." (2)

Even the external air is supposed to develope active inflammation in a wounded varix; and this consideration led Davat to recommend the operation of puncture.

"The action of the air," he says, "upon the venous tissue appears to be incontestable. Brodie, dreading it, modified the process of cutting adopted by the English. Dancé did not doubt the influence of the air upon the open orifice. Lisfranc, taught by many observations, thus expresses himself. 'I have often remarked, that at the very moment of the section, the air rushes into the vein, and a *horrible inflammation*, a severe phlebitis, becoming general, destroys the patient in twenty-four hours.'" (3) (See p. 334.)

Why does all this happen? Are the same results apt to attend the veins from similar operations, and exposure to the air, in their normal state? And when violent phlebitis supervenes on "venesection," do we not find the clearest evidenee of extensive venous congestion in some of the important viscera? Why does pressure upon a varicose vein unite its opposite sides, when it fails of this effect upon the healthy veins?

At other times the inflammation consequent on division, &c. is less severe, and there takes place an increased enlargement of the vessels as the principal effect; and "it is evident," says Mr. Hodgson, "that the disease in the branches sometimes increases after the obliteration of a varicose trunk." (4)

Varicose veins of the lower extremities not unfrequently pass into the active form of phlebitis. Two cases of this nature are recorded by Dr. Lee, parturition having been the exciting cause in one case, although the uterus appears to have had no direct agency. In this case the subject

"Had suffered much from a varicose state of the veins of the lower extremities, and in two days after her confinement, the acute inflammation supervened in the veins of both limbs." In the other case, "a varicose state of the veins of the leg had existed for some time." Ultimately, "the saphena vein became gradually hard and exquisitely painful. Limb hot and colourless. Veins

(1) Vol. xxx. p. 150. (2) Oct. 25, 1839.
(3) Op. cit. p. 29. (4) On Diseases of the Arteries and Veins, p. 563.

around the ankle can now be felt, indurated and knotted. Exquisite pain on pressure along the saphena vein." (1)

"In a patient of the British Lying-in Hospital, who had suffered much from varicose veins in the latter months of gestation, inflammation of the saphena veins of both lower extremities came on two days after delivery, with most severe constitutional symptoms." (2)

A case is related in Cooper's and Travers' Essays, where most violent symptoms attended a case of spontaneous obliteration of varix of the saphena and spermatic veins. In the north of England Med. and Surg. Jour. for 1830, is a case by Dr. Knight, which well exemplifies active phlebitis supervening spontaneously on a varicose state of the lower extremities. Similar facts will appear in the sequel.

Dr. Good remarks that varicose veins are often exceedingly irritable, and almost always add in a high degree to the irritability of diseased parts in their vicinity. (3) This is universally known. There is frequently, too, much constitutional disturbance; but not of that kind which attends venous congestion, for the reason, in part, that the latter disease affects the organic viscera. Mr. Hodgson says that "the pain is often excruciating." (4) Sir C. Bell, who adopts the mechanical theory, remarks that, —

"The pain becomes sometimes excessively severe, whilst the enlargement is taking place in the thigh; and an acute attack of inflammation attends it, marked by a broad firm cake in the integuments over the vein, sometimes as large as your hand. I may hint to you that I have repeatedly seen the pain, in the *early stage of the distension* of the veins, on the inside of the knee and of the tibia, mistaken for pain in the bone. When the varicose condition of the limb has advanced, there is a disposition to *throw out* coagulable lymph." (5)

The long catalogue of cases assure us that pain is the principle inconvenience attending varices, and which induces the sufferer to incur the hazard of an operation. Let us lay our hand indiscriminately upon an author. It is M. Davat. (6) In the seven cases of simple varix upon which he performed his operation of puncturing, every one was attended with great pain. In some of the cases it was aggravated by atmospheric changes. Caius Marius, who had varicose swellings in both legs, had one of them divided, without any expression of suffering: but, he refused to have the operation repeated upon the other, "as it was a remedy

(1) On some of the Most Important Diseases of Females, pp. 170, 172.
(2) Cyclopædia of Prac. Med. Lon. Art. Phleg. Dolens, p. 348.
(3) Study of Med. Art. Varix. (4) Op. cit. p. 540.
(5) Lectures, in Lon. Med. Gaz. vol. xiii. p. 427.
(6) Trait. Curatif des Varices.

too dearly bought by such exquisite torture." (1) Seneca, (2) and Celsus, (3) allude to the great suffering attending the operation. Even the hypertrophied veins of pregnant females are generally painful; though they complain but little because they consider the evil irremediable by art, are patient of suffering, and depend on nature for relief. Febrile action is often a consequence. Mr. Farrall mentions a case where "a lady is always obliged to lie on a sofa during pregnancy an account of varicose swellings of the veins." (4)

"Whatever may be the cause of circocele," says a reviewer, "the symptoms are not confined to a *simple distension*, increased by heat, exercise, &c.; but the patient experiences excessive pain along the cord, which is so severe as sometimes to give rise to the idea of a neuralgic affection." (5)

Instances are recorded in which castration has not removed the suffering. We have no doubt, however, from the great frequency of the affection, and from its occurring mainly on the left side, that it forms a rare exception in which the pressure of the venous column, along with the concurring effect of straining at stool, is the common exciting cause, through the stimulus of distension, of that morbid state of the vein which follows in numerous instances. At other times, however, there is a simple state of mechanical distension, and then are wanting the characteristic signs of varix.

That the pain and soreness are owing to inflammation is inferable from all analogy, when associated with the other signs, and from the multiplied instances in which simple distension of vessels is not productive of suffering, whether the distension be suddenly produced, or in a gradual manner; whilst they denote the intensity of the vital forces with which the veins are endowed.

We may, also, find some analogies in the results of particular remedies, especially local bloodletting. Hippocrates advises bleeding in varices, and even making small incisions in such as are liable to bleed profusely. (6) Avicenna says, "juvamentum quidem in phlebotomia sciaticæ est magnum in dolere sciæ; et similiter in podagra, *in varice*, et in elephantia." (7) Petit extols the practice of bleeding varices, and says, that by this means and the recumbent posture, he has not only removed the most obstinate ulcers which have depended upon them, but has redu-

(1) Plutarch Apothegm. t. ii. p. 202. (2) Epist. 78. (3) L. 7. c. 31.
(4) Dublin Journ. Med. and Chem. Sciences, vol. ii. p. 223.
(5) Boston Med. Magazine, vol. ii. p. 763.
(6) De Ulceribus, v. 56; de Med. v. 63. (7) L. i. Fen. 4. c. 20, p. 84.

ced permanently the dimensions of the vessels. (1) We have been long in the habit of treating circocele with the most gratifying success, by leeching, cold effusions, and the recumbent posture. M. Petit states a case in which bloodletting reduced the vein to its original size. (2)

Petit, (3) and Boyer, (4) were in the habit of taking two or three pounds of blood from varices without producing the least weakness. The latter states, also, that "copious hemorrhages may take place spontaneously from ruptured varices without debilitating the patient in proportion to the loss of blood." Why is this so? We think the answer may be found in our remarks on bloodletting; where we have endeavoured to show that it is the tendency of inflammation to resist the impression from the loss of blood. This law, however, is more or less modified in inflammations of the venous tissue, as we have also attempted to show, and as will appear farther hereafter.

Do not the foregoing considerations lead to the conclusion, that active phlebitis may be more or less counteracted in operations for varix, by previous leeching along the whole course of the vein? And do not our facts and illustrations account for the difference in the results of operations in different hands? (5)

We have reserved, finally, for examination, the fact that varicose veins are liable to spontaneous obliteration, and to other changes which distinguish active phlebitis.

"Sometimes," says Hodgson," the coagulum accumulates within them to such an extent as completely to obliterate the canal of the dilated vessel. I have seen four cases in which this event terminated in the spontaneous cure of varices. In these instances, it is probable that the coagulum accumulated till it completely filled the varix, and the upper portion of the vein communicating with it. The blood, being unable to pass forward, coagulated in the vessel to a considerable extent; this coagulum was gradually absorbed; as its absorption advanced, the coats of the vein contracted; the vein was ultimately obliterated, and the blood was conveyed through collateral branches." (6)

But, there is no evidence given, in these cases, of any primary obstructing cause, or of the existence of any coagulum. The whole philosophy is hypothetical, whilst analogy and facts to which we have just adverted, prompt the conclusion that, as in common phlebitis, adhesive inflammation had supervened spon-

(1) Trait. des Malad. Chirurg. t. ii. p. 53. (2) Ibid. (3) Ibid. (4) Op. Chir.
(5) See Cooper's & Travers' Surgery, on Varix. (6) Op. cit. pp. 542, 544.

taneously. (1) Would not the coagulum be more likely to be permanent in some of the cases, than to be absorbed in all?

But, it is important to remark, that the coagulum found in varices is no evidence of an original obstruction to the circulation, or that it had been even languid in the primary stages of the disease. We shall endeavour to make this still farther evident in the sequel. At present we shall only state a pathological fact:—

"Whilst the dilatation is uniform through all the points of the vein," says Cruveilhier, "the blood circulates freely. But, if a point in the circumference of the vein become altered in structure, and it yield, as in varices, the blood is here retained." (2)

This is one source of coagula. As these particular alterations multiply, coagula form one after another, till the circulation may become entirely obstructed in consequence. But then the coagulum is more or less permanent. We do not believe, however, that the foregoing *rationale* is often the true one. Are not the coagula generally organized? So say the anatomists. It is at this stage of varix that disorganizing inflammation takes place, and when it may be most engaged in the production of coagulable lymph. Low degrees of inflammation constantly produce this result in the cellular tissue, between which and the venous there is a strong resemblance in regard to the products of that disease. When the deposition of lymph has proceeded so far as to diminish the area of the vessel below its natural volume, it then becomes a cause of obstruction, and the blood may "stagnate."

If the formation of coagula take place in the veins according to our philosophy, it is important that it should be duly considered; since the fact is indicative of inflammation, and serves as an analogical ground of induction as to other formations. (Pp. 251, 257.)

"Coagula," says M. Andral, "present considerable variety in the firmness of their adhesion to the parietes of the veins, in their physical characters, and in their organization. They live precisely as the veins in which they are form-

(1) The "hardness of the veins" arising from inflammation alone is liable to be mistaken for the presence of coagulum. In one case "the varicose veins were connected with an intractable ulcer near the ankle. A large varix in the course of the saphena major, a little below the middle of the thigh, became *suddenly enlarged without any evident cause*, and was extremely painful. In a few hours it became hard," &c. (p. 537). Pressure was ultimately applied, and the tumour gradually obliterated. Here the ulcer had evidently operated in superinducing a state of active inflammation.

(2) Path. Anat. Malad. des Veines, liv. 16.

ed, and like every other part endowed with life, keep up a constant process of nutrition and secretion, and are liable to become diseased." "Sometimes they contain liquid or clotted blood, or otherwise altered; again the central matter is purulent." (1) They may undergo cartilaginous, or osseous, and, according to Velpeau, and others, encephaloid, degenerations.

Now as to the organization of these coagula; for here, also, morbid anatomy is in conflict with the laws of physiology.

"If we examine," says M. Andral, "whence this coagulum derives its vitality, we find *it cannot partake of the common life of the rest of the body*, since it very often merely touches the surrounding tissues, without being in any manner continuous with them. We must therefore admit that these polypiform concretions, or *polypi*, as they are called, may possess a proper vitality, *by means of organs they have created themselves*." (2)

When we consider the organization, the vital properties and actions, of these formations, and that they are liable to the same inflammation and its products that occur in the natural parts of the body, is not the analogy too irresistible to allow the conclusions in the preceding extract? Whence do they derive their perpetual supply of blood, unless it be transmitted through vessels that are continous with the aorta? Or, shall we resort to the hypothesis of physical imbibition of blood from the contents of the veins? And yet another more inconsistent, that they are nourished by a species of blood which is known to be fatal to all other parts? If other proofs be wanting, the attachment of these formations to the venous parietes, which generally exists, is another important step in the demonstration. Sometimes they are loosely, at other times firmly, connected. The degree and extent of their attachment appear to depend on the activity of disease in the venous coats. In simple congestion, where they are also found, (though we believe generally a post-mortem result,) their connection with the veins is commonly slight; in varix it is greater; in phlebitis it is greatest. Thence, also, they possess corresponding degrees of organization. For like reasons they are more or less easily detached; and there are three obvious modes in which they may be separated, and found loose in the veins. It may be, by the constant attrition of blood; by the violence used in dissections; by the contraction, obliteration, and absorption of their vessels of attachment, after inflammation shall have terminated; or it may be from a concurrence of these several causes. (3) A remarkable exemplification of the detachment

(1) Patholog. Anat. vol. ii. p. 257; vol. i. p. 394. (2) Ibid. vol. i. p. 394.

(3) Therefore it appears that the "argument" employed by Dr. Hope to show

of analogous formations is seen in the tubular masses of lymph which are frequently ejected from the bowels after the termina-

that polypi of the heart do not depend on inflammation cannot be held as "conclusive," viz. "were they so formed, they would, from the first, be adherent; which is not the case. The comparison of facts clearly proves, in the first instance, that they are free, and that it is only the older and more perfectly organized which become adherent." (*a*)

Our author, therefore, supposes that "the more perfectly organized," as well as the imperfectly organized polypi, create for themselves, in the first place, an independent organization, and afterwards, in the first instance, attach themselves to the living tissues. But why this reservation in favour of "the more perfectly organized?" We think the interrogatory is answered in our text. We may quote Dr. Hodgkin upon this subject, whose opinion is here valuable, since he believes in spontaneous generation, (p. 124,) although we see in the following conclusion a proof against it. "I am sceptical," he says, "as to the production of new and independent vessels, except in the case of an embryo; and I believe that in the organization of new parts, the vessels are always prolongations of those which were prëexistent." (*b*) This may be abundantly defended on the ground that the new organization and actions often partake of those of the part from which the new substance is effused, whether it be secreted lymph or extravasated blood. It has been well remarked that "the vessels seem to have more of the polypus in them than any other part of the body." (*c*)

Formations of simple lymph, and what may be more accurately called "concretions," are often taking place after death. These, of course, are detached, and we have known them to have been mistaken for the organized masses. But that analogy which Dr. Hope so justly employs, at other times, on the obscure results of the laws of vitality, teaches us, at least, that their organization cannot be independent of the agency of organized matter. And since, also, the violent and perpetual action of the heart excludes the belief that coagula may form in the blood, lose their vitality, and then, by mechanical irritation, attach themselves through the adhesive process of inflammation, the irresistible conclusion is that they are originally a secreted product. Or, whenever the result of any mechanical impediment to the circulation, and they subsequently take on the changes which are peculiar to organized matter, as the formation of pus, &c., the coagula had established an adhesive inflammation with the vein at an early stage of their formation. Magendie considers the opinion that polypi of the heart are formed antecedently to death as exploded. (*d*) It had been denied by Senac, Pasta, Morgagni, Lieutaud, and others. This shows their great rarity, though a few undoubted examples have been reported by Bouillaud, (*e*) Andral, (*f*) and some others. Burns found a perfectly organized concretion of the size of a hen's egg, upon which were some superficial ossifications. Meckel (*g*) says they are composed of lymph, thrown out by inflammation. And so Kreysig, and Bertine. (*h*) Hughes has given a table of 62 examples. (*i*) The disproportion of cases observed by individuals is too great. Thus, Bouillaud reports 6; Legroux 8; and Dr. Hughes 6; whilst Lænnec, Hodgson, &c. but 1 or 2 each. It is important to remark, however, that they were generally attached to some part of the heart;

(*a*) Diseases of the Heart, &c. p. 508. — And so, Andral. — *Path. Anat. vol. i. p.* 394 — (*b*) Lectures on the Morb. Anat. of the Serous and Mucous Membranes, vol. i. p. 354. — (*c*) Editor of Mr. Hunter's Lectures on Surgery, p. 51.— (*d*) Note to Bichat. Recherch. Phys. p. 297. — (*e*) Archiv. Gén. de Méd. t. v. p. 95 — 101, & t. ii. — (*f*) Patholog. Anat. — (*g*) Gen. & Path. Anat. vol. ii. b. 4, c. 6. — (*h*) Diseases of the Heart, &c. p. 443. — (*i*) In Guy's Hospital Reports, April, 1839.

tion of muco-enteritis. The structure of course remains, in all the cases, after the attachments are severed ; and it may be difficult to say when the last vascular communication was interrupted. A clot of blood may become organized. But then it is an indispensable requisite that it shall be from the beginning in contact with abraded vessels. We shall not farther argue the question than in the preceding note as to whether the vessels are propagated into the consolidated blood, or whether, as surmised by M. Andral, and even by Mr. Hunter, "the coagulum, has power, *under necessary circumstances*, to form vessels in, and of itself." (1) The latter we had, perhaps, sufficiently controverted in our "Vital Powers." We also hold as fundamental, that the process of organization begins before the blood has lost its vitality, and that the latter contingency probably happens soon after its extravasation from the vessels on which its vitality depends. (2) If this be an exception to Harvey's principle, *omne vivum ex ovo*, it is within the rule of O'Kerr, *omne vivum e*

but this may be apparently the case with the mere coagulum which forms after death. Mr. Gulliver thinks them more common than is generally supposed. (*a*)

The following illustrations put the whole of this question in its proper aspect. "For many years a female had suffered the usual symptoms of dilatation of the heart. The dilated left ventricle was filled with a fleshy substance which was attached in two places of the lining membrane. These attachments, after projecting from the walls of the heart, united and formed an oval body about fourteen lines in length. Three roundish blood-vessels were continued from the heart into the rami of the polypus. They were injected with mercury, which penetrated the ramifications." (*b*) In another case, "a mass of coagulum filled the inferior cava and iliacs. In one or two points it appeared as if a kind of suppuration had been going on within it; and *here* its sides were *still more closely* attached to the inside of the veins." (*c*) Or thus: M. Andral found the pleura studded with numerous knobs; some of which were only clots of blood; others, coagula of older date, and of various hues from red to a dull white. But they were all attached to the pleura by an organized vascular tissue. The same appearances were also observed upon the peritoneum. (*d*)

Carswell has always found the vascular organization of a coagulum of fibrin, whether in an artery or vein, to have been derived from the vessel. He has never been able to discover an original and independent formation of blood-vessels in the fibrinous substance. (*e*) M. Bertin, "after an attentive examination," concludes that "polypiform substances depend on inflammation of the vessels," and that "their organization takes place in the same manner as that of the false membranes." (*f*)

(1) On the Blood, &c. p. 92; & Med. Commentaries, vol. ii. p. 200.

(2) Blundell has endeavoured to show that the blood loses its vitality in 30 to 60 seconds after its effusion from the vessels. (*g*) Some physiologists maintain that coagulation is a proof that the blood has lost its vitality. (*h*)

(*a*) Lon. Med. Chir. Trans. 1839, p. 149. — (*b*) Bulletin des Sciences Méd. Sept. 1828. — (*c*) Bright's Med. Reports, vol. ii. p. 64. — (*d*) Précis de Pathol. t. ii. p. 692. — (*e*) Illustrations of Elementary Forms of Disease, fas. 11. — (*f*) On Diseases of the Heart, &c. pp. 442, 443. — (*g*) Med. Chir. Trans. vol. ix. — (*h*) Ibid. vol. xvi. p. 293.

vivo;(1) a principle which the materialists have endeavoured to subvert by scrutinizing the habits of the lowest orders of plants; but where, from the greater simplicity of organization, its truth is exhibited with the force of mathematical demonstration. We firmly believe with Mr. Hunter, that "all the water in the world will not make a dead seed grow," and that the doctrine of spontaneous generation leads directly to atheism. (See *Appendix on Spontaneous Generation*, p. 123.) But, in referring to the seed, it should not be neglected, that it possesses within itself a power of originating actions, whilst the blood is merely the material on which other parts operate. (See *Vital Powers*, and *Humoral Pathology*.)

"In repeating those experiments of M. Gendrin," says Mr. Gulliver, "in which a seton is passed through a clot of blood, either in or out of the vessels, I could only twice succeed, after numerous trials, in producing anything like pus in the substance of the coagulum." (2)

There was no organization, and therefore we infer that in the two instances of our author's success, there could have been nothing produced like purulent matter. Indeed, our author admits that, "*the matter proved to be nothing more than softened fibrine;*" a mere chemical change which has no analogies in the living organism. (P. 181—204.) These experiments are among the great encumbrances on physiology. (3) (P. 190, *note.*)

(1) Dictionnaire, &c. t. 7. p. 329.—In respect to the production of the infusoria, the true but rare hydatid, intestinal worms, &c. it has never been shown that they do not proceed, originally, from a germ. Dutrochet expounds the whole mystery as to the production of animalcula from "invisible germs." (*a*) Ehrenberg, who adopts Harvey's doctrine, has demonstrated the dependence of fungi and mould upon real germs or seeds. (*b*) Amici supposed that the white byssus was the result of a spontaneous generation in the sap of the vine. From this he argues that wood is formed by the tendency of sap to a spontaneous organization. But Dutrochet proved by experiment that the byssus springs from seeds,—*ut cit.*—Spallanzani had done the same in respect to the dependence of animalcula upon eggs; which was afterwards confirmed by Wrisberg, by a different process. (P. 95, *note.*)

There is nothing hypothetical in the induction that the germs of insects may be greatly altered in their habit, though not in organization, by new modes of existence, as within other animals; and what was but an almost invisible object, in its native abode, may enlarge into a hydatid or an intestinal worm. Even man himself is affected in his stature, &c. by climate, and other animals of high organization far more remarkably by domestication, &c. It is asserted that the ligula and bothriocephalus solidus obtain their genital organs only after they enter the intestines of birds. But this is against fundamental laws.

(2) Lon. Medico. Chir. Trans. 1839, p 150.

(3) The Medico-Chirurgical Review, in noticing Mr. Gulliver's article, from which

(*a*) Annales des Sciences Nat. Jan. 1834.—(*b*) Nov. Act. Nat. Cur. t. x; & Schilling, in Kastner Archiv. t. x. p. 429.

The doctrine of which we have been speaking would ascribe a higher degree of vitality to loose coagula formed in the veins than belongs even to the most highly organized matter; for if such organized part were introduced into a vein at the moment of its excision, it would no more maintain a circulation than if immersed in a vessel of water. But, the objection which we have stated as to the properties of venous blood is conclusive against the hypothesis of an independent life. "Sanguis arteriosus solus est qui nutrit partes, venosus autem nunquam, residuus est enim a nutritione." (1) How very rarely, indeed, does blood become organized when extravasated into the cellular tissue, whilst in immediate and permanent contact with the vessels from which it is effused, and that blood and those vessels belonging to the arterial system. On the other hand, we constantly witness the organization of recently secreted lymph, and expect it as an almost certain consequence. When coagula of blood apparently become organized, it is probable, in most instances, that the true substance is secreted lymph, of which the coagula have been the remote cause by exciting inflammation in the contiguous parts, whilst the coagula themselves undergo an absorption; or, whatever may remain of them will become involved, as a foreign substance, in the new formation, and will be ultimately absorbed by this new agent. (2) (P. 190, *note.*)

What are the appearances in the arteries when inflammation is excited by ligatures? Here a permanent obstruction to the

the foregoing extract is made, observes, that,—

"We would simply remark that Mr. Gulliver's researches on the presence of pus in the blood are far from being established. *There are grave doubts entertained of their correctness.*" (*a*)

Such doubts we had taken the liberty of expressing; and we are now glad to find that they are not likely to add another laurel to the humoral pathology, but prove another "mirage" of the microscope. (See our vol. i. pp. 388, 524, 686, *note*, 716.) Will any one have the goodness to repeat Dr. Percy's experiments as to the specific attraction of the brain for alcohol? (Vol. I. p. 577, *note.*)

(1) Ramazzani Opera Med. et Phys. p. 495. See Bichat on "black blood;" and Müller's Elem. Physiol. vol. i. p. 149.

(2) Whilst some observers have looked on and witnessed the process of organization in secreted lymph, seen it completed almost at a single inspection, Stoll informs us that it is so gradual that he had rarely been able to discover the appearance of organization before the eighth day, and Villermé never till the twenty-first. The latter states that the first appearance of the new vessels is in those parts of a false membrane which are nearest to the seat of the greatest inflammation, or in parts which are the thickest. Yet does the process doubtless begin as soon as the lymph is secreted.

(*a*) Jan. 7, 1840, p. 51.

circulation is constituted, and the escape of blood is not facilitated, as in the veins, by anastomosing branches. Organized coagula, on the common hypothesis, should therefore be constant and extensive. But if the formations depend on inflammation, they should be very circumscribed, and limited to the seat of disease. And is it not so?

In consequence of the greater disposition of the venous tissue to inflammation, and the peculiarities of its properties, we constantly meet with a variety of products which never appear in the arteries. This consideration is, *a priori*, in favour of our conclusion that they are not generated in the blood. Some of them are irresistibly ascribed to inflammation; but others, again, are supposed to depend on spontaneous coagulations of blood. This is affirmed of phlebolites by Carswell, Burrows, (1) Lee, (2) Tiedemann, Otto, Errhman, and others; and as their formation in varicose veins tends to illustrate our subject, as well as other principles in physiology which it is our object to consider, we shall devote a few words to this phenomenon. The first thing which strikes us, is the *concentric* layers of lymph, and of carbonate and phosphate of lime, (3) and that there are no analogies to support the conclusion that coagulated blood may spontaneously undergo such an extraordinary transmutation, whilst the processes of life supply them in abundance. This might be sufficient. But Boujalsky has found them directly connected with the vein. Tiedemann found them attached to the inner coat by a very thin transparent membranous covering, and has seen in some of the varicose dilatations soft fibrinous masses adhering to the inner tunic, which were evidently the nuclei of beginning concretions. The origin of all others is thus rendered obvious, since the analogy is conclusive; and the "soft fibrinous masses" refer the process to inflammation. (4) Dr. Lee, in a case of ulcerated mucous

(1) Burrows supposes them to be indiscriminately the result of "stagnation," and of "slight inflammation of the vein," and that coagula take on the independent office of forming the concentric layers of lymph and chalk. (*a*)

(2) Lee says, "in what manner the blood becomes coagulated in the veins so as to give origin to the formation of phlebolites, is not so easy to determine." Blood may coagulate about them; but the reason of this is sufficiently obvious.

(3) According to Jahn, Gmelin, Prout, and Kemp, they are composed of the foregoing substances; and, as Mr. Kemp states, in about the proportions that exist in the bones.

(4) Bertin, in speaking of chronic arteritis, remarks that "the yellow points, cartilaginous lamina, calcareous and lining incrustations of the aorta, seem to us to be nothing more than a series of changes through which passes, successively, the mat-

(*a*) Crounean Lectures, 1835, lec. 2.

membrane of the rectum found several phlebolites in the hæmorrhoidal and vaginal veins. Some of them were invested with concentric layers of fibrin. And, although these bodies had become detached, they were in the immediate vicinity of parts of the veins that were *disorganized* by inflammation. (1) That inflammation is the cause of phlebolites is inferable, also, from the veins in which they occur being almost always in a varicose state, whether the veins of the spermatic cord, the venous plexuses of the womb or vagina, or of the bladder, or the prostate gland, or testicles, or rectum, or spleen, or the lower extremities, &c.; whilst this peculiarity operates as a proof of the dependence of varix in all those parts upon inflammation. If phlebolites be only simple and independent conversions of coagula into lamellated chalk, why do we never meet with them in other parts when extravasations of blood have been long retained, as in the brain? Why are they always confined to the veins; where, indeed, according to Tiedemann, Otto, Boujalsky, Lobstein, Columbus, Tulpius, Errhman, Walter, Carswell, Bartholin, Hodgson, Langstaff, Holberton, Cloquet, Beclard, Meckel, Andral, Dupuytren, Tilerier, Bouillaud, Burrows, Lee, and others, they are found almost universally? Whence comes the carbonate of lime? From a clot of blood? Never; but organized structures are full of exact analogies which supply the true interpretation. It should be also considered that secreted lymph is extremely prone to take on some new and unexpected action.

ter secreted by the effect of inflammation. They are infinitely varied, perhaps becoming sometimes detached, and forming those kind of *stones* which the ancient authors are said to have met with." "Analogy, reason, and positive experiments on living animals, concur to prove that they are one of the terminations of inflammation. This truth has been clearly and ingeniously developed in a memoir which Dr. Rayer published in the 1st volume of the Archiv, Gén. de Méd." (*a*) And so Dr. Hope: "Changes of this kind in other parts are, by general consent, attributed to chronic inflammation." (*b*) And so Dr. Hodgkin; (*c*) who adopts what is called the "exclusive doctrine" as to the general dependence of new formations upon inflammation. We may also refer to the analogy, regarding phlebolites, which is supplied by concretions that form within the joints of rheumatic subjects. It is obvious that there is no analogy between the formation of phlebolites and urinary calculi. Or, what shows the whole philosophy of this matter is a case by Cruwell, who "found under the valves of the pulmonary artery a hollow, globular body, having its parietes partly cartilaginous and partly osseous, whilst an extensive ossification pervaded the right side of the heart." (*d*)

(1) Cyclopædia of Prac. Med. Lon. Art. Varix.

(*a*) Diseases of the Heart, &c. pp. 69, 71. (*b*) Diseases of the Heart, &c. pp. 162, 163.
(*c*) Lectures on the Serous and Mucous Membranes, p. 60, &c.
(*d*) De Cordis et Vasorum Osteog. p. 25, 1765.

Andral has known tubercles to form in it within two weeks after an attack of inflammation. No argument can be drawn against our doctrine from the indisposition of the venous tissue to become ossified. It does not show that its secreted lymph may not acquire the ossific action, in virtue of peculiar modifications of the vital properties which are imparted by the veins: whilst this is another proof of the peculiar vital endowments of the venous tissue.

If we are right in our construction of the pathology of varix, the reason becomes obvious why this affection is so rare in children; and we derive from the philosophy of the fact another proof of the validity of our ground. Their *superficial* veins are less developed, less matured in their vital forces, than at after age. Bichat notices this fact.

"During the first years of life," he says, "the veins have a real inferiority as it respects the arteries. This inferiority continues during the whole time of growth. Of this you may be satisfied by examining the external veins. They are never as evident, or as much developed in children as in an adult." (1)

What anatomy thus teaches we infer more particularly from the exemption of the superficial veins from varix, &c. during the period of childhood. Bichat, however, was often too general in his statements, but which may be comonly found qualified in other places, as in the instance now under consideration. The foregoing principle is not true of the veins of particular organs, especially of the brain, where they are greatly developed. It is for this reason, the brain is especially subject to venous congestions during childhood. Here it is said by Bichat "the proportion of the cerebral veins over the others is gradually lost as we advance in age, because the brain does not continue to predominate so much in nutrition;" (2) and, as might be supposed, therefore, there is a corresponding final cause in a diminution of their tonic forces. Their susceptibility to disease consequently lessens, and *simple* cerebral congestions become more and more rare as we move on in life. Pathology, in this instance, borrows a strong light from physiology. Still, when causes operate upon the system at large, and modify the vital state of the whole organization, as in congestive fevers, there is great liability to venous disease of the brain, and the law is shown to be mainly applicable to the

(1) Gen. Anat. &c. vol. i. p. 427. (2) Ut cit.

simple conditions of venous congestion. Bichat is doubtless right, in a general sense, in saying that,

"At the period of puberty, and towards the end of growth in height, the veins partake of the general plethora, which seems to manifest itself, and which is, as we have seen, the source of many diseases." (1)

A difference, also, at the two divisions of life exists in relation to the predisposing causes. Gastro-enteritis constantly develops cerebral congestion in childhood, especially in infancy when the tonic forces of the brain are in most active operation. (2) Not so, however, at adult age; whilst, from other causes, that modification of venous congestion which results in cerebral hemorrhage is almost peculiar to this period. (P. 335.)

The foregoing will be seen to be important considerations in arriving at a knowledge of the various modifications of venous inflammation, as manifested in ordinary phlebitis, varix, venous congestions, &c.; whilst other remarkable peculiarities of the venous tissue, as will be shown by certain abrupt limitations of inflammation, or by the sudden transition of a severe form of the disease into milder degrees in contiguous parts, or by the differences which arise from the different modes of operating upon varix, (Vol. I. p. 610, *note*,) will aid our understanding of the varieties which are produced by different morbific causes, and of the propagation of inflammation, in one case, from the capillary veins to their trunks, whilst, in another, it does not extend beyond the capillaries themselves.

When disorganization and loss of contractility take place, although the inflammatory action subside, the enlargement will remain, and the circulation continue unimpaired. In a case by Dr. Carswell, an eruption of blood took place from the varix, in consequence of ulceration, and of which the patient died in about ten minutes. The walls of the vein were found to be much thicker than those of an artery of the same size, and considerable pressure was necessary to approximate their internal surface. (3) Authors have described ruptures of varicose veins, from which torrents of blood have flowed in consequence. Here, we apprehend, that the rupture was only an ulceration of the venous parietes, and the immense loss of blood was owing to the loss of contractility consequent on disorganization.

(1) Ut cit.

(2) This is especially the reason why general bloodletting is preferable in these cases to leeching or cupping. (See vol. i. p. 338.)

(3) Illustrations of the Elementary Forms of Disease, Fascic. vi.

But, although the ultimate, and common lesion of structure show the nature of the pathological cause, it is only a sequel. There is always a period when no disorganization, no obstruction, no relaxation, exists; and the essential inquiry relates, therefore, to the primary cause of the dilatation, upon which, also, all the other results depend. Again, the venous coats may be variously affected in their contractile property by various degrees of structural lesion, or by interstitial deposition of serous fluids, so that, being slight, they shall sometimes obey this principle when gravitation favours, or cold operates, &c. The greatest vicissitudes are seen to occur in respect to the progressive results of the pathological cause. The action often remains a long time without change, or, again, it abates, to be again renewed. The predisposition is apt to be permanent, even when disorganization has ensued, and the inflammatory action has terminated. This is shown by the fatal results, in cases of this apparent nature, which have attended the different modes of radical cure.

An enlargement of the epigastric veins occurs, as we have said, in pregnancy without any apparent cause. We must look for the source of it in the principle of sympathy. That a predisposition to venous derangements is more or less determined by that condition is obvious from the frequent cases of phlebitis which supervene on parturition, and from the venous congestions to which puerperal females are liable. As to the epigastric veins, besides the objections we have mentioned, gravitation is opposed to a simple mechanical accumulation of blood, and other veins are natural, as those of the leg, where we should sooner look for the operation of this principle were it constituted by the gravid uterus. But the tissues of that organ are in a state of irritation, which may be propagated by sympathy to the epigastric veins, whilst the latter return to their natural state as soon as the irritation ceases. Particular organs most readily sympathize with each other under certain remote influences. And so we shall have seen it to be with the venous system of particular parts. Here, too, the thickening of the venous coats excludes the hypothesis that the dilatation is owing to simple distension or relaxation. This, however, is an instance in which we apprehend inflammatory action exists in its lowest degree, or consists in that state of irritation which precedes inflammation.

Van Swieten says, that he has known the hypertrophied veins of pregnant women to acquire the size of the thumb. He has

seen them ruptured, as in varix, — the hemorrhage having been "very profuse, and followed by an ulcer exceedingly difficult to cure." (1) There is yet a difference between venous hypertrophy and varix, which denotes different modifications of action.

We now enter upon a consideration of that branch of our analogies, to which we have just adverted, hypertrophy of the veins; which is so closely allied to varix, that it is regarded by many as identical with it. And here we premise, as in all other cases, the phenomena must be taken collectively. The remote causes, the vital and structural lesions, &c. must be studied in their proper connection, and no one considered abstractedly from the others. If it be not allowed, for instance, that morbid *thickening* of the tissues is, per se, a proof of the antecedent existence of inflammation, we maintain, when other characteristic phenomena of that disease have often preceded, that this result must be taken as conclusive, even by those who are most disposed to make a distinct disease of every modification of structure. In considering, therefore, this particular phenomenon, it must not only be associated with all the other antecedent circumstances; but, in the following discussion, these circumstances must be constantly compared with what we have already said of varix, and shall hereafter state in regard to phlebitis. We are, therefore, in no respect inclined to hold morbid anatomy to that consistency which would be justified by the recent pretensions of the science.

Hypertrophy of the veins is, also, another condition which illustrates the modifications to which inflammation is liable as affecting the venous tissue, and, in this respect is instrumental in aiding our conceptions of those varieties which distinguish venous congestion.

Venous hypertrophy appears to arise from different exciting causes. It is sometimes owing to an obstruction to the course of the venous blood, which gives rise to this affection rather than to its kindred, varix. But since, in numerous cases, great obstructions have existed without producing this phenomenon, it is manifest that in the instances in which it takes place, it results from some modified action which is developed in predisposed parts by the stimulus of distension. But, in a far greater number of cases no such original obstruction has existed, but the

(1) Commentaires on Boerhaave, &c. vol. xiii. s. 1301, p. 355.

action has been excited by an injury, or by chronic inflammation of some part with which the veins are connected, or has been the result of remote sympathy. As the hypertrophy advances and the veins become convoluted, there is then a partial obstruction constituted by this condition, and the affection, in consequence, advances more rapidly.

The pathology of hypertrophy will be best illustrated by an example of the disease. We shall select a remarkable one from a work by Cruveilhier, on account of the minuteness of the dissection, in which the dilatation of the veins had no original dependence on obstruction, and where the causes and the morbid anatomy are clearly indicative of inflammation.

"Joseph Bressoles, aged 48 years, an old soldier, entered the hospital St. Antoine, on account of a disease which presented all the appearance of scirrhus of the pylorus; there being, also, an extraordinary dilatation of the veins of the abdominal integuments. These veins, resembling large leeches, crept in a zigzag manner under the skin, and were turned upon each other like the convolutions of the brain. This appearance extended from the pubis to the umbilicus, where there was a voluminous convolution as large as a moderate sized orange, having the appearance of an umbilical hernia. From this collection departed two veins, which were less flexuous and less voluminous than the preceding. These veins could be followed as far as the xyphoid appendix, where, from becoming deep seated, they were lost. The tumours formed by the union of the dilated veins, resembled the head of the medusa of which M. Séverin speaks. Its colour was violet; consistence soft; and it yielded to compression. A light *bruissement* was heard through the stethescope.

"The following is the history of the case. In 1813, Bressoles was taken prisoner by the Hungarians, who, to avenge the death of one of their comrades, gave him a blow on the belly with the butt-end of a musket, and left him for dead. Being carried to the hospital, he felt for a long time a pain in the abdomen, and did not go out till the end of six months.

"In 1814, after his return to France, on applying his hand to his abdomen, he discovered this enlargement of the veins. They were *then* voluminous, but soft and indolent. Their colour was bluish. He attributed their formation to the fatigue of a long journey on foot.

"Restored to private life, he devoted himself to commercial business, and lost the whole of his gain. In 1818 he was obliged to seek for a livelihood by turning a crank during the whole day. The occupation was laborious, and the veins became more enlarged. A physician being consulted, he was told that nothing could be done but to change his pursuit, which he accordingly did. Since 1826 the enlargement had been stationary. In 1827 the gastric symptoms were manifested. The patient died on the 14th of January, 1833, with all the symptoms of scirrhus of the pylorus.

"*Examination of the body.* — Hypertrophy of the mucous, fibrous, and muscular tissues of the pylorus, and the neighbouring parts of the stomach. Considerable contraction of the pyloric orifice.

"The subcutaneous veins of the abdomen were injected with a coloured preparation. The injection did not penetrate into that portion of the dilated veins which ran from the umbilicus to the sternum. The iliac and pelvic veins, and the ascending vena cava, were *natural.* The veins of the lower extremities were neither varicose nor dilated.

"The umbilical vein, and the vena portæ, received the injection. The portal vein was *very voluminous.* The ductus venosus obliterated.

"The liver very small, but healthy. The spleen was nearly as large as the liver, being in a state of hypertrophy and induration. Its surface was scattered over with cartilaginous plates.

"The dilated veins were situated between the skin and a very thin membranous fascia, which separated them from the sheath of the recti muscles. This membrane was evidently the superficial fascia of the abdomen. The skin was raised to a level with each circumvolution of the veins. It formed a sort of a cyst, moving freely, without adherence to the veins, as if a serous tissue were interposed between the vein and the skin. Some of the distinct convolutions adhered to the skin by a very delicate cellular tissue. Fibrous and indurated prolongations of the skin to the superficial fascia separated the vascular convolutions. The veins, which were greatly convoluted, formed an uninterrupted series, and they were thickened to a level with the concavity of each circumvolution, and were thinned to a level with the convexity.

"There were two very distinct systems of the circumvolutions; one on the right, the other to the left. The right was constituted by a single vein; the left, by two veins which met below in a single trunk. There was *no communication* between the circumvolutions of the right and those of the left side. Each communicated with the epigastric vein, across the rectus muscle. There was one very large communication at one point; and yet the epigastric vein presented its natural size, nor had the injection penetrated the epigastric vein. But the syringe employed was not a good one.

"At the umbilical region there were two very large tumours, in juxta-position, and very projecting. The tumours were situated immediately below the umbilical cicatrix. Each tumour was formed by a single vein folded upon itself." (1)

The points to be considered in the foregoing case, as relating to our subject, are

1st. The veins which were the seat of the affection. These were, mainly, the subcutaneous, which, in their natural state, are very small, and empty themselves into the internal saphena. Their circulation was, therefore, promoted by the force of gravity; and, in this respect, accumulations of blood are prevented here as in the jugular veins. — 2d. The pervious state of the umbilical vein, and the communication of the dilated veins with it. — 3d. The communication of these veins with the epigastric and the portal veins. — 4th. Their great tortuosity, and the alter-

(1) Cruveilhier's Anatomie Pathologique, livrais. 16.

ed state of their parietes, and of the adjacent parts to which the inflammatory action had been propagated from the veins.—5th. The voluminous state of the portal veins, the liver being preternaturally small.—6th. The natural state of the saphena, iliac, and pelvic veins, and of the vena cava, and veins of the lower extremities.—7th. That the subcutaneous veins were alike on both sides, and communicated only at the umbilical region.—8th. The diseased state of the stomach and spleen, and that no other than inflammatory action could have produced the pain, which was early felt for a long time in the abdomen, and, subsequently, "all the phenomena of cancer of the pylorus."—9th. The violence inflicted and the nature of the subsequent exciting causes of disease.—10th. That there was no conceivable obstacle to the circulation in the subcutaneous veins; but, on the contrary, that there were unusual facilities, besides the force of gravity, from their communication with the umbilical, portal, and epigastric veins.—11th. That the convoluted state of the vessels did not contribute to their dilatation; since, after it had reached its maximum, the dilatation remained stationary for a period of six years.—12th. The important fact, that there was the same alteration of structure in the stomach and spleen as in the diseased veins.

13th. Was not the affection of the veins and of the internal viscera produced simultaneously by the blow; or, rather, was not that of the former propagated along their veins to the vena portæ, and thence through the umbilical vein to the more dilated ones; or may not sympathetic influences have operated? Was not the excessive toil the subsequent cause of the increased enlargement of the veins, and how did it operate? 14th. Can the dilated state of the veins be satisfactorily ascribed to any other cause than the diseased condition of their parietes? The whole case will be fully illustrated when we come to the subject of ordinary phlebitis. The example by Mr. Anderson, followed by extensive varix, will be much in point; as, also, is that of simple varix by Hennen, at page 342.

It is worthy of remark, that in the foregoing case neither the epigastric nor saphena veins participated in the affection; though there is reason to think that the umbilical did. The latter fact, however, is not necessary to our interpretation of the pathology, since the affection of the subcutaneous vein, may be as readily explained through the principle of remote sympathy with the

internal viscera, as the analogous affections in pregnant women. But, we may here point out an analogy between the exemption of the epigastric and saphena veins, and what happens in active phlebitis; where, according to Dr. Lee, —

"In some women affected with crural phlebitis, the inflammation of the femoral vein has appeared to be suddenly arrested where the trunk of the saphena enters it, and the inflammation has extended along the superficial veins to the leg and foot." (1)

Arnott's observations led him to lay down the principle, that "the inflammatory changes are limited by the current of blood; where a trunk is concerned, the boundary being the entrance of a branch; and where a branch is concerned, the boundary being the junction of this with a trunk." (2)

A close analogy often exists, in the foregoing respect, between varix, venous hypertrophy, and active phlebitis. It is a coincidence, in itself, which appears to imply analogous pathological causes.

Finally, the subcutaneous abdominal veins appear to be peculiarly liable to that mode of diseased action which results in hypertrophy, and its attendant enlargement of their areas. This is denoted by its frequent occurrence in pregnant women; whilst its exclusive limitation to these vessels, and its absence in a greater proportion of cases, as well as other reasons hitherto assigned, evince its dependence not only on some peculiar condition of the vital state of the uterus, but a peculiar susceptibility of these veins in their relation to the gravid uterus. And does not the subsequent reduction of the volume of the veins depend, in the same way, on a corresponding sympathy with the change which ensues in the uterus after parturition; that organ exerting, throughout, a specific influence on these vessels according to the variations in its condition? Or, is the reduction of the veins owing simply to the removal of the exciting cause? We think not; for, when hypertrophy occurs in other cases, whether of the veins or of other tissues, it is permanent. The same fact is shown in active inflammation of the saphena vein, as will appear hereafter. The saphena may be obliterated; but, is it a sound conclusion that the attendant hypertrophy of the subcutaneous veins depends on that obliteration? The contrary is rendered obvious by the fact that in many cases the morbid dilatation alone exists in both series of vessels. Both are more or less simultaneous in their origin; and the inflammation, where most strongly pronounced,

(1) Op cit. (2) In Med. Chir. Trans. Lon. vol. xv. p. 46.

has not attained that degree, nor are the depositions those, which are followed by contraction, or obliteration of the vessel. Besides, there are instances recorded of hypertrophy of the superficial veins of the abdomen, on both sides, where only one saphena vein was affected. In these cases, there is no room for doubt that the hypertrophy on one side depends upon the active inflammation of the corresponding saphena; whilst the true physiologist will see from this coincidence, and analogous facts, that the same condition on the opposite side has resulted from sympathetic influence. — (See p. 149; and Vol. I. pp. 134, 158, 160, 204, 467, 478, 567, 570, *text and notes*.)

If facts be allowed as much weight as the graved uterus, the exemption of the veins of the lower extremities should alone settle the question against the mechanical hypothesis.

No argument can be drawn in opposition to the foregoing facts, nor to the dependence of abnormal thickening of parts upon inflammatory action, from the general increase of the impregnated uterus. One is a morbid, the other a natural process. The interstitial depositions are wholly unlike in the two cases; a fact which seems not to have been at all considered by those who borrow analogies from natural processes to embrace the foregoing results of inflammation. One is disorganization; the other growth. And yet such is the tendency of nature to restore the integrity of parts, that, where great disorganization has happened, as in hepatization of the lungs, the disorganizing deposit is often removed after inflammation has terminated.

In carcinomatous, and some other inflammations, (pp. 317, 331,) where there may be less blood circulating in the part than in many instances of the common form of inflammation, we often find the superficial veins enlarged far beyond the capacity that would be required by any extraordinary determination of blood. This is frequently true of all disorganizing inflammations which have no natural termination. Dr. Hodgkin, in defining the characters of malignant disease of all kinds, including malignant ulcers, states, that "another character is, the enlargement of the veins in the *neighbourhood* of the affected part, though this symptom is not at all times present." (1) The veins become involved like the other tissues, and a low state of inflammatory action is gradually propagated to the larger trunks. In the brain, this condition of the veins is particularly liable to attend organic

(1) Lectures on the Morb. Anat. of the Serous and Mucous Membranes, p. 267.

derangements when they proceed from common inflammation, whether active or sub-active. Dr. Monro remarks that, "a distension of the veins of the pia mater is the most common morbid appearance in various organic diseases of the head." (1) This peculiar disposition of the veins of particular organs to be thus affected grows out of a principle which we have already examined in part. Again, also, "hypertrophy of the veins, which is distinct from varix," says M. Cruveilhier, "is seen in all the cases where there is a large morbid growth of an organ." (2)

"The last circumstance," says Dr. Carswell, in speaking of carcinoma, "to which I shall allude, is the development of the subcutaneous venous system; sometimes so conspicuous when the disease affects the breast in the form of a tumour, or any other external part where the skin is capable of considerable extension."

Dr. C. ascribes this affection of the veins to the pressure of the tumour, and "not to any special influence exercised by the disease." This, too, is the opinion of M. Cruveilhier, and of pathologists generally; whilst the "extensibility of the skin" appears to favour the mechanical doctrine. But we have the observation of critical observers, that there is no obstruction to the circulation in the enlarged veins in carcinomatous affections, — that "the motion of the blood is as rapid there as in the other veins." (3) Enlargement of the veins frequently takes place while the tumour is small and loose. And why is it not as frequent and remarkable in tumours of a simple nature? Sir C. Bell, as we have said, had seen more than twenty cases of painful swelling of the upper extremities from cancer of the mammæ, which were evidently instances of phlebitis. (4)

In the circuitous circulation, depending on the obstruction of a large vein, there takes place only a simple dilatation of the veins. What, then, should constitute so great a difference in the results of this species of obstruction, and that which is supposed to arise from the pressure of tumours? How can we explain, on the principle of pressure, the case which immediately follows the remarks of Dr. Carswell just quoted? It is beautifully displayed in a plate, which represents a carcinomatous ulcer of the stomach of about two inches square. Two coronary veins, stretching across the organ, are enlarged to the diameter of nearly three-

(1) Morbid Anat. of the Brain, p. 46. (2) Anat. Patholog. Livrais. 16.

(3) Bichat's Gen. Anat. vol. i. p. 430.

(4) See Cyclopædia of Prac. Med. Lon. Art. Phlegmasiæ Dolens, p. 349; and Dr. Lee's Observations on some of the most Important Diseases of Women, p. 174.

eighths of an inch. The branches bear a corresponding size, those being largest which proceed from the ulcer. The plate appears to be an impressive illustration of the dependence of the dilatation of the veins upon inflammation. This case is wholly parallel in respect to any mechanical cause with the one of varix by Mr. Hennen, (p. 342,) and another, of hypertrophy, by M. Cruveilhier, (p. 366.) Similar, also, to the foregoing case, is one of common ulceration of the stomach, described by Andral, who speaks of the veins as an instance of chronic phlebitis, and which is the only instance in which we have known this term to have been employed.

"L'estomac était envahi par une large ulcération," "on voyait de grosses veines ramper en grand nombre autour de cette ulcération." (1) — Some of the veins were altered in structure. Perhaps the greatest number of cases of crural phlebitis in men have originated in ulcers of the mucous membrane of the large intestines, whilst ulceration of the uterus is a frequent cause of phlegmasia dolens. We may also state that the researches of M. Cruveilhier have led him to the opinion that the primitive seat of cancer is in the capillary venous system. (2) This circumstance is rendered the more important to our views by the subsequent observations of other pathologists. Dr. Carswell, in his "Illustrations," states that the extremities of the ducts in hepatic carcinomia "are dilated into the form of pyriform sacs of various sizes." Dr. Hake, in pursuing the inquiry, remarks that the varicose ducts are coloured by a plexus of varicose capillaries from the portal vein, which also occur in a more varicose state within the ducts. The structure of the ducts is entirely vascular, consisting also of a plexus of varicose capillary veins; and when a section is made, the whole substance appears to be composed of these varicose capillaries, resembling a honeycomb. He concludes, as we have seen of M. Cruveilhier, that the essential pathology consists in the foregoing condition of the capillary veins, in which he also includes, in a lesser degree, a varicose condition of the capillary arteries. (3) But, we apprehend, that the real pathology lies still deeper, and must be looked for in a specific form of inflammation.

"Dr. Graves has shown that a varicose state of the superficial thoracic veins may occur from cancerous degeneration of the lung itself." (4) Here, again, we see the principle of sympathy in operation. (Pp. 248, 279, 283, 290, 333, 351, 369.)

Of the same nature is a case described by Laennec, "who examined the body of a man who died of cancer of the rectum, and he found the iliac veins inflamed and obstructed." (5)

In these several instances, which are only given as examples of many others of the same nature, the various degrees of inflammatory action are illustrated.

(1) Clinique Medicale, t. ii. p. 108. (2) Op. cit. Livrais. 4, p. 3, 1829.
(3) Treatise on Varicose Capillaries, &c. p. 7, &c. 1839.
(4) Brit. & Foreign Med. Rev. vol. iv. p. 292.
(5) Cyclopædia of Prac. Med. Art. Phleg. Dolens, p. 346.

We have said that there is generally but little blood, less than in health, circulating in carcinomatous tumours, and it is obvious, therefore, that less is returned by the veins. With the exception of the medullary sarcoma, Dr. Carswell remarks that the blood-vessels are often indistinguishable in carcinoma, and when new formed vessels take place, that they are often in a varicose state. As the disease advances, it frequently results in the worst lesions of active phlebitis. The veins, indeed, often become obliterated; to which circumstance Dr. Carswell sometimes ascribes the termination of carcinoma in mortification, (1) — this being in conformity with the mechanical hypothesis of that affection. (Pp. 173, 180, *notes.*)

Again, the substance of carcinoma is often found within the veins, which we are disposed to regard as a product of these vessels; especially as Dr. Carswell, and others, have often found it attached to their parietes. "Minute blood-vessels pass from the one into the other, and are often very numerous, and remarkably conspicuous in the cribriform matter." (2) One case of this nature must settle the philosophy of the whole. A great variety of the carcinomatous formations are found in the large veins, "the lardaceous, mammary, medullary, and the hematodic, — all in the same trunk."

It appears to be incompatible with all analogy to suppose that these various products originate in the blood, as supposed by Dr. Carswell and others; (3) especially since their existence in the solids is obviously the result of vascular action. But we have sufficiently considered the merits of this doctrine in our Humoral Pathology, particularly in the 14th Section. There is also an equal difficulty in accounting for the manner in which those products become attached by membrane and vessels to the veins, unless we suppose their deposition by inflammatory action in the venous parietes. Indeed, Dr. Carswell states that — "We cannot limit the disease to any one tissue." That these products

(1) Illustrations, &c. ut cit. Fascic. 3. (2) Ibid. Fascic. 2.

(3) Dr. Carswell thinks, also, that, "as *a morbid constituent* of the blood, we can take *no cognizance* of the existence of tubercles, otherwise than through the medium of the secretions, or until that fluid has ceased to circulate. Then the tuberculous matter is seen to separate from the serum, fibrin, and colouring matter," &c. (*a*) Is there any proof of this? Here secretion, also, is regarded as a mere mechanical process, a mere filtration. We refer to these facts that they may be connected with our consideration of the relative merits of the anatomical and Hippocratic schools.

(*a*) Illustrations, &c. ut cit. Art. *Tubercle*; and his Art. on Tubercle in Cyclopædia of Prac. Med.

are not formed by the capillary veins, as supposed by some, is evident from the manner of their attachment to the larger. True, this matter is more frequently unconnected with the parietes of the veins. But as in the case of organized "concretions," it may have become detached; or, like simple lymph, have been deposited from the venous tissue, without becoming organized. It is also particularly worthy of remark that this peculiar substance is found in the veins only, not in the arteries, and generally only in the vicinity of the affected parts. If generated in the blood, (if it be worth while to argue this doctrine any farther,) should it not appear as well in the vena cava, and other remote vessels, —in all vessels indiscriminately, including the arterial? If the deposit be sometimes found remote from the main seat of disease, we have the principle of sympathy, as we have already endeavoured to show, to explain the phenomenon. The carcinomatous disposition, too, as we shall soon see, and as is well known, is apt to pervade the system.

Since the foregoing, in relation to Dr. Carswell's opinion of the pathology of carcinoma, and venous depositions, was transmitted to London, as stated at the beginning of this essay, Dr. Hodgkin's excellent work, on the morbid anatomy of the serous and mucous membranes, has reached this country; and we find in it the same comments upon Dr. Carswell's doctrine that we had expressed ourselves. We shall quote some of his remarks for the benefit of this coincidence of opinions.

"It is evident," says Dr. Hodgkin, "that the material (carcinomatous) so found (in the veins) is not *in transitu* to the spot at which the bulk of the tumour exists." "Is this appearance, then, to be regarded as a sufficient proof that there exists an essential and general change in the blood in the system, such as to give rise to the development of malignant tumours in one or more parts of the body? I conceive not." "How is it that the peccant humour confines its mischief to one spot, and performs the part of good and healthy blood everywhere else throughout the body?"—(Vol. I. *Humoral Pathology.*)

But the most material part of the coincidence of our views, and which is especially interesting to our inquiry, relates to the generation of carcinomatous matter in the veins.

"The existence of a material," says Dr. H. "in some respects resembling that of the tumour in the interior of the vessels is, I believe, nothing more than one of the effects of this change, (inflammation.) Its more frequent occurrence in the veins favours this idea, since they are more disposed to inflammation than other vessels; and as they allow of its extension along their course from the original seat of the affection, we need have no difficulty in accounting for the existence of the morbidly produced material through a considerable extent of the vessel."

"The presence of cerebriform matter in either of these sets of vessels, (veins and lymphatics,) is no proof that it has been absorbed by them. On the contrary, it seems far more likely that it had been actually secreted by their own internal surfaces, since the very consistence of this matter seems to show that it could not have been transmitted through these vessels, like the blood and lymph which they are destined to convey." Our author also adverts to a similar phenomenon in "scrofulous *inflammation*." (1)

We do not feel that an apology is necessary for the particularity with which we dwell upon this, and analogous subjects. The facts are prolific of information as to the great inquiry before us. As one of their bearings, it is obvious that when we connect them with the enlargements of the veins in which carcinomatous matter is found, it certainly may be strongly applied in the way of analogy to certain other enlarged states of the veins, where no carcinoma exists, for the purpose of showing that this phenomenon, in venous congestions, depends upon inflammation of the veins.

Another hypothesis is associated by some writers with that of the spontaneous formation of this matter, which is, that the matter is sometimes absorbed by the venous radicles. In a particular case, it is said that, —

"Professor Cruveilhier, by whom the dissection was made, regarded the disease as having its point of departure in the uterus, thinking that the cancerous deposit was taken up by the veins and deposited in other organs, precisely as, according to this pathologist, pus is transported to remote organs in uterine phlebitis." (2)

This conclusion is founded upon two other hypotheses, one of which regards the absorbing power of the veins, the other, the deposition of pus by these vessels in remote parts. The analogy, therefore, would be feeble, at best; but is rendered abortive by the want of a proper foundation for the premises, and is opposed by facts and physiological principles. As is well remarked by the writer last quoted, the predisposition to cancer is often con-

(1) Pp. 290, 350, — 352.

(2) Dr. Swett, in New-York Med. and Surg. Journ., July, 1839, p. 34 — 36.

We had intended to have stated the opinion of Hunter in connection with our remarks upon pus, as appear in vol. i. pp. 388, 523, 540. "And in the case of veins," he says, "there is great reason to believe, that, after all the bad symptoms are removed, suppuration is still going on, as we find it so in a sore. Pus may, therefore, still pass into the constitution from the veins, and yet the hectic may not be produced, which would certainly be the case if those bad symptoms were occasioned by the matter getting into the circulation." (*a*) Thus do we constantly return from artificial results to the phenomena of nature.

(*a*) On the Blood, &c., p. 501.

stitutional; and, it may therefore, as it doubtless was in the foregoing case, be developed in many parts of the body.

The doctrines as to the formation of carcinomatous matter are as various as those in relation to pus. Dr. Burrows believes that coagula of blood change into carcinomatous matter. He relates a case on the authority of Dr. Pardoe, where —

"A woman was affected with gangrene of the foot and lower part of the leg, quickly extending to the knee, when the limb was amputated. Half an inch above the division of the vena cava, and extending into the iliac vein, a semi-coagulated brain-like substance was found. The presence of the substance in the veins was considered by Dr. Pardoe as rather obscure, unless, as he supposed, it was the result of inflammation of the vessels of the gangrenous extremity." — Dr. Burrows rejects this construction, and considers the substance "the result of spontaneous degeneration of the fibrinous part of the coagulated blood." (')

We have introduced this case on account of the singularity of the supposed presence of carcinomatous matter in the veins, when there existed a state only of common inflammation upon which the phenomenon depended. For this, as well as for other reasons, it cannot be that the matter was carcinomatous; whilst it is also stated to have "resembled a mixture of *pus* and *blood*." This fact, if no other, establishes its dependence on common venous inflammation. In all the recorded cases which we have examined, the carcinomatous matter within the veins has been accompanied by a condition of disease in other parts distinguished by analogous formations. It is upon this fact, indeed, that the hypothesis of absorption has been in part founded. But, our conclusions here, as in the case of propagation of inflammation from common ulcers, &c. to the veins, appear to be sustained by analogy, by what is known of the liability of the veins to inflammatory action, and to its various modifications as denoted by their products, by the constitutional nature of cancerous affections, and by the coincidence of the supposed morbid action in other parts. From the great disposition in the veins, also, to throw out morbid secretions when only moderately inflamed, it is no evidence against us that their structure is not impaired by specific actions. See APPENDIX VI. *on Melanosis.*

If, then, it appear that inflammation exists in varix, hypertrophy, the venous enlargements which attend carcinomatous affections, &c. in the absence of other apparent causes of the dilatation of the veins, we must conclude that it depends upon that state of

(1) Crounean Lectures, 1835, lec 2.

disease. This will form a strong ground of analogy in our farther inquiry into the analogous phenomenon in venous congestions. We shall have farther proof, also, that different orders of veins are endowed with different kinds and degrees of susceptibility to inflammation. That, the disease is variously modified according to the natural modifications of the vital properties of the different series, and according to the nature of the remote causes, and the species of inflammation, or other disease, which may affect other tissues with which the morbid veins are connected. That, the superficial veins are mainly the seat of varix and hypertrophy; the deeper seated, but such as belong to parts that are not concerned in organic life, are most liable to phlebitis; whilst those of the great vital organs are alone especially disposed to congestion. From their varied situations and their particular relations to other parts, the different series of veins are not only distinguished by peculiar modifications of the vital powers, but are exposed to the agency of causes specifically different; those of the surface more to the causes of common inflammation, and those of the parenchymatous organs, as in congestion, to such specific ones as derange the viscera with which they are connected.

We claim not the merit of originality for the particular views, which we have also more or less expressed in our essay on the vital powers, as to the modification of these powers according to the tissue which they animate, or their alterations in disease according to the nature of the morbific causes. They belong to John Hunter, who deduced in conformity with them his important doctrine of generic and specific inflammations.

"Inflammation," he says, "will in general be in proportion to the exciting cause, in which we may include the mischief done the constitution and the nature of the part;" but, especially, does he dwell upon the variously modifying influences of the remote causes. (1)

The great French Philosopher, Bichat, probably arriving at all his conclusions by inductive philosophy, adopted Hunter's views as to the modifications of the vital properties in the various tissues, and carried them out to a full demonstration. (Pp. 297, 317, 319, 321, 333, 335.) The doctrine of the variously modifying effects of morbific causes belongs emphatically to Hippocrates. Of these several principles we shall have made an extensive use in relation to venous congestion. What Hunter and Bichat had so well perfected, has been ably applied by Dr.

(1) On the Blood, and Inflammation, p. 255. et seq.

Davies in his "New Pathological Views." The cöoperation of this philosopher is too valuable to pass without enlisting him in our present objects. We shall therefore subjoin in a note his general conclusions. (1)

What we have said in the present section in relation to the veins, is also applicable to that very analogous tissue, the lymphatic vessels, where they proceed from carcinomatous tumours, ulcers, &c. Breschet (2) has ascertained that their dimensions vary, like the veins, according to the changes which take place in the organs with which they are connected, the enlargement sometimes surpassing the size of the thoracic duct. And, what is especially to our purpose, they become enlarged in scirrhus, and in suppurating parts. In the former case it cannot be from the absorption of any redundant matter. Here, then, analogy is reciprocal in its light, as to the constitution, and peculiar tendencies of the veins and absorbents to common pathological conditions. Breschet relates a case of varicose absorbents of vast extent, and quite analogous to the case of venous hypertrophy by Cruveilhier, (p. 366.) The vessels were painful and manifestly diseased; being also large enough to admit the tube of a kitchen bellows.

We have constant proof of a great susceptibility of the lymphatics to inflammation from various causes, and of its diffuse nature, as in the venous tissue. As in the latter, also, it undergoes every variety of degree and modification. This subject being important in its analogical bearings upon the pathology of venous congestion, we shall resume its consideration in a more appropriate place.

(1) He concludes, "1. That the most prominent characters of inflammation are dependent upon the tissue affected. 2. That every variety of inflammation, — in other words, that inflammation of each tissue, — is modified considerably according to the nature of the cause producing it. 3. That every variety of the disease is also modified according to the condition of the tissue affected. 4. That every variety is likewise modified according to the general condition of the system; which, of course, embraces the condition of each tissue. 5. That the same cause may produce more than one variety of the disease, by acting upon more than one tissue. 6. That the vessels of all the tissues are liable to be affected somewhat differently, and in a different degree, by the same cause." (*a*)

Upon these principles rests the whole philosophy of Disease and its curative means. *See Essay on the Humoral Pathology.*

(2) Le Système Lymphatique, &c. 1836.

(*a*) Selections in Pathology and Surgery, exhibiting New Pathological Views, &c. Part I, 1839.

SECTION X.

THE VEINS POSSESS THE POWER OF DILATING ACTIVELY.

It will be seen that the present, like the following section, has an important connection with the philosophy of the operation of the loss of blood, as well as with the subject immediately before us; whilst the true aspect of the question now to be considered is another great step in our system of demonstrating the essential independence of organic life of physical laws.

It appears to be probable that the dilatation of the veins, in varix, hypertrophy, phlebitis, and congestion, is *primarily* of an active, and not of a passive, nature. In the progress of disease, disorganization contributes, more or less, its mechanical effect. It is certainly possible, however, that the dilatation may depend on that diminution of consistence which would follow certain degrees of inflammation in the venous tissue. The modified action may result, at once, in an impaired state of vital affinity, by which power the molecules of the venous coats are held in aggregation; and, any interstitial fluid effused would contribute to the same result. But, we state this merely as a possibility; whilst the constant change of diameter in the veins, where no such contingencies exist, is *prima facie* in favour of our doctrine of active dilatation. The rapid effect of bloodletting, also, in producing a contraction of the vessels in venous congestion, could not be explained on the mechanical principle, whilst it is strongly corroborative of the vital one. The former construction, however, will in no respect affect our theory of venous congestion; but, physiology is deeply interested in the true interpretation, and the treatment of venous congestion will be more or less influenced by the exact pathology of the disease.

An inquiry is naturally suggested, whether the veins are endowed with muscular power; and, since this question has a special bearing upon the vital endowments of the veins, is intimately connected with the philosophy of venous congestion, and with important physiological principles, we shall examine it with some particularity. It is also important to our next inquiry as to the powers which circulate the blood, and which is likewise intimately connected with a proper understanding of venous con-

gestion. By this investigation, we also advance an important object of our whole work, — that of illustrating the powers of life.

Hunter, Bichat, Beclard, Meckel, Tiedemann, Breschet, and others, speak very familiarly of a middle fibrous coat. Tiedemann says, —

"The veins contract on their contents by virtue of the elasticity and contractility of their fibrous coat, in which they resemble the arteries." (1)

Bichat was able to observe this coat in nearly all the veins. He was doubtful as to its possessing "much motion." He thought that the data which we possess are not sufficient to determine this question. (2)

"One of the first effects of hypertrophy of the middle membrane of the veins," says Andral, "is to render it distinctly visible in a number of veins where it naturally exists in such a rudimentary state as to be imperceptible. In those places where it is usually visible, the *longitudinal* fibres are rendered much more distinct, and there is never seen any trace of *circular* fibres;" and he thinks he has seen it present the appearance of muscular tissue. (3)

Some other anatomists have not been equally successful in detecting these fibres; but, their observations are only of a negative kind, and can, therefore, have no weight against the direct proof. It is sufficient, however, for our purpose that the veins possess the power of dilating and contracting; and, that these changes are of such constant occurrence as to denote the principle for which we contend. Bichat abandoned an inquiry into the relative areas of the venous and arterial systems, because the contents of the veins were every instant subject to change. Their middle coat, about which so much difference of opinion has existed, we infer from that circumstance, possesses a peculiar structure. Bichat thought it "*essentially different from that of all the other tissues, having its peculiar properties, life and organization.*" (4) And, although it do not obey the stimulus of electricity, like the common muscular fibre, as stated by many observers, (5) it may yet derive an action from its vital properties when left to their natural operation, or when exposed to other stimuli. Who shall say, that this tissue of peculiar organization may not be highly susceptible of contraction and dilatation when its organic properties are impressed through the power of sympathy with the capillary arteries, and by its

(1) Comparative Physiology, vol. i. p. 149.
(2) Gen. Anat. vol. i. p. 401.
(3) Patholog. Anat. vol. ii. p. 253.
(4) Gen. Anat. vol. i. p. 401.
(5) See Craigie's General and Patholog. Anat. p. 114.

natural stimulus, the blood? (1) Or, that, when altered by disease, the principle of dilatation shall not be in the same permanent operation as during the continued application of heat or other stimulants? Has it been in any way demonstrated that a part must be muscular to perform the function of motion? It certainly does not follow, because mobility is a property of muscle, that it cannot belong to another tissue. The fact can only be established by showing its absence everywhere else. We grant the full import of analogy; but here it can only operate in relation to what is strictly muscular, and since also, as we shall see, it has been fully shown that other structures manifest the function of motion.

In respect to the conclusion drawn from the effects of electricity, and of other irritants, we may remark that their results are very various when their action is directed upon different muscular fibres. This arises from the peculiar modifications of the vital powers with which different series of the veins are endowed, (pp. 329, 334, 362, 377;) being analogous to the varied results in different orders of veins, when morbific causes operate. The proof from electricity may be also shown to be fallacious by referring to its action upon the mimosa sensitiva, where, by parity of reason, it should establish the existence of muscular fibre. (2) If the negative evidence be admissible, the positive should be more so. Sœmering, however, says, when chemical stimulants are applied to the trunks of veins their contraction is stronger than that of the arteries. (3) Haller, (4) produced contraction by touching them with sulphuric acid; and Verschuir, (5) Hast-

(1) This association between the properties of a part and their natural excitants, and their repulsion of other causes, is particularly exemplified in the influence of light, exclusively, in exciting motion in the leaves and petals of plants, in the iris, &c.

(2) Cavallo, (*a*) Ingenhous, (*b*) Delametharie, (*c*) Dandriani, (*d*) Ledru, (*e*) Marum, (*f*) produced contractions of the leaves of the mimosa sensitiva by the electric fluid. Percival, (*g*) and Ritter, (*h*) proved that its contractions were owing to its stimulating properties, and not to the agitation of the shock. Giulio, (*i*) the same by the galvanic fluid. And so of many other vital stimuli. Percival (*j*) says, that "the leaves instantly close when the vapor of volatile alkali, or the fumes of burning sulphur, are applied to them.

(3) De Corp. Human. t. v. p. 327. (4) Op. Min. p. 375.

(5) De Arteriarum et Venar. Vi Irritabili.

(*a*) Vollstandigue Abhandlung, &c. 1797, p. 319. — (*b*) Rosier, in Journ. de Physique, t. xxvii. p. 62. — (*c*) Ibid. t. xxx. p. 26. — (*d*) Ibid. t. xxvii. p. 468. — (*e*) Ibid. t. viii. p. 395. — (*f*) Expériences faites par le Moyen de la Machine électrique, &c. 1795, p. 160. — (*g*) Mem. of the Philos. Society of Manchester, vol. ii. p. 124. — (*h*) Denkschriften der Munchner Akad. 1810, p. 245. — (*i*) Journ. de Phys. t. lvii. p. 460. — (*j*) Op cit.

ings,(1) by a variety of stimulants. Meckel says, "the large trunks which possess very apparent fibres are *particularly irritable.*"(2)

If a variety of foreign agents, therefore, will excite contraction of the veins, it establishes conclusively their vital tonicity, and implies, as we shall show when we speak of the general circulation, a corresponding power of dilating. The fact, indeed, of the veins being so sensitive to the action of foreign stimuli is the highest possible evidence of their profound endowment with the vital properties, (Sec. 8,) and that their power of mobility, or of dilating and contracting, must be readily influenced by the varying proportions of their natural stimulus; whilst we infer, *a priori*, that they must be especially liable to morbific influences. But we might have safely taken their natural manifestations, since we find throughout the animal body, that fibres apparently the same are specifically affected by different stimulants. It appears to be an illegitimate conclusion, that parts which are excited into action by their natural agents, and influenced by principles which depend on their relation to other parts, should equally obey all foreign stimulants. It is contrary to all the facts which relate to living matter.

"We are bound to admit in animals," says Tiedemann, "as many kinds of excitability as they have different parts; therefore, an excitability of the cellular tissue, of the muscular, of the nervous, of the bones, of the fibrous organs, &c. Each organ, every part, the different coats of vessels, &c. have each their peculiar and specific susceptibility to excitations, according to the differences of structure communicated by the act of formation, and are excited to manifestations of activity by peculiar kinds of stimulants." (3) And so Hunter, Bichat, &c. (Pp. 377, 378.)

None doubts that blood stimulates the heart, and is the immediate cause of its action; but will it stimulate all other muscular tissues? Verschuir,(4) and Aldini, could not always produce muscular contractions of the heart and bladder by electricity; and Bichat,(5) Sœmering,(6) and Berhends, could not excite the heart with galvanism. Humboldt, Fowler, Pfaff, Wedemeyer, but very feebly. Zimmermann(7) often failed of exciting the

(1) On Inflam. of Mucous Membrane of Lungs; *Introduction.*
(2) Gen. and Patholog. Anat. vol. i. p. 128.
(3) Comparative Physiology, vol. i. p. 389.
(4) Op cit. exp. 22. (5) Recherches Phys. sur la Vie, &c. p. 492.
(6) Op cit. (7) De Irritabilitate. — Thus might we go on illustrating our subject. Krimer has alone observed the contraction of the muscular fibres of the trachea; whilst Wedemeyer could not excite them by galvanism, although it

muscles by different stimuli. The lacteals will not absorb bile, &c., (Vol. I. pp. 549, 575, 605—608, 525;) urine excites the bladder by distension alone, whilst it excoriates all other parts; and light produces motion in the iris through the indirect medium of the retina, the brain, and the third pair of nerves; whilst it is insensible to the knife. When one pupil contracts, from this cause, its fellow is affected by a corresponding movement. (1) Here is the principle of sympathy, which we shall see in operation in the veins as it respects their relation to the arteries.

"I have employed galvanism," says Bichat, "and I am convinced that it has very little, almost no power, in putting into action muscular contraction in organic life, whilst it is the most powerful agent in animal life." And again, "admitting the fibres of the veins to be muscular, since they are only *longitudinal*, it would be very difficult to observe the effect of irritants applied to them, though it might be real." (2)

"The structure which produces muscular action," says a learned writer, "varies so much in different animals, that we are at a loss to conceive how the effects should have the least similarity; and, it is in some cases only from witnessing the actions that we can consider the parts as muscular; since, in nothing else do they bear a resemblance to the muscular structure in the most perfect animals with which we are best acquainted." (3)

produced contractions of the bronchi of three-fourths of a line in diameter, till their cavity was nearly obliterated. (*a*) Bichat, Kleim, Valli, Volta, doubted whether the motions of the heart can be affected by electricity. Müller says that galvanism has no effect on the thoracic duct; and Tiedemann and Gmelin found it insensible to chemical irritants; and yet it is well known that the absorbents possess a strong vital contractility. Electricity and all other agents had failed upon the small arteries, till Schwann (*b*) effected their contraction by cold. It is also worthy of remark that he represents the capillaries as possessing circular fibres, like the arteries; though this was a microscopical discovery.

(1) In some animals, as in fishes, the iris is immovable.

(2) Gen. Anat. vol. i. p. 415; vol. iii. p. 24; and his Researches on Life and Death.

Nevertheless, Müller, and others, adhere to the test of electricity. Galvanism, and other irritants, will produce no contraction of the stomach when applied to its nerves, but may excite powerful contractions by contact with its mucous surface, whilst it is the reverse with the muscles of animal life. Again, powerful impressions are transmitted from remote parts to the stomach, by way of the brain, and even by the mind. It was by neglecting the consideration, that, for important purposes nature has suited the irritability of certain parts to particular stimuli only, Magendie, (*c*) and many others, have concluded from their experiments with galvanism, acids, &c. that the arteries possess no irritability. Wrisberg, and others, have contended that the iris is not muscular, because it is not excited by the direct action of light, whilst others represent it as composed of two sets of fibres.

(3) Crounean Lecture, in Philos. Trans. 1795, part 1, p. 203.

(*a*) Muller's Physiol. vol. i. p. 347. (*b*) Muller Archiv. 1836.

(*c*) See Bichat Rech. sur la Vie, &c. p. 155, *note*.

M. M. Roux and Laennec think it has been shown experimentally that the lungs are capable of an "independent, active expansion." (1) Müller says,—

"The *efferent* ducts of glands are lined by a mucous membrane, which has on its exterior an extremely thin layer of *muscular* substance. The existence of muscular fibres cannot, it is true, be demonstrated anatomically; but *physiological observations* place it beyond dispute; the efferent ducts of most glands have the power of contracting when irritated." (2) — (See our APPENDIXES on ENDOSMOSE, &c., and on the MICROSCOPE, Vol. I.)

"Nothing is more uncertain," says Bichat, "than the rule which is commonly adopted for pronouncing upon the muscularity of any doubtful part; for the rule consists *in ascertaining whether such part does or does not contract under the action of stimuli.* It is thus, that a muscular tunic is admitted in the arteries, although their organization differs entirely from that of the muscles; it is thus, the womb is pronounced to be fleshy, however foreign to such structure; it is thus, a muscular texture is admitted in the iris, and other parts, although no such structure be observable there." (3) — (See APPENDIX on ANALOGY.)

And here is Mr. Hunter's excellent logic, which we may be excused for quoting, since it is said that these authors are "almost forgotten." (4)

"We have said that animal matter is so constructed as to be endowed with a power of self-motion, as in muscles; and as we can hardly conceive any part of an animal entirely passive or free from motion, since all parts grow and are nourished, we must suppose this *muscular structure very universal* in an animal body, though this power has been considered principally in regard to muscles, properly so called, whose actions are plainly visible to the eye." (5)

"There is a set of animals," says Sœmering, "entirely composed of cellular and mucous tissue, in which, even by the microscope, neither muscular fibres, nor anything analogous to them, are discovered, and which, nevertheless, are capable of moving. Of this kind are infusoria, polypi, most radaria, and some entozoa." (6)

The monades, and animals composed apparently of a simple mucous matter, are seen moving occasionally. And, if we descend even to the vegetable kingdom, we shall find that "the

(1) Laennec on Diseases of the Chest, &c. p. 415. — See, also, Dr. Williams in Edin. Med. and Surg. Journ. vol. xix., p. 353. — It should be stated, however, that Laennec considers the structure of the bronchi muscular. In Resseien's splendid work, published in 1822, the muscular fibres are delineated. Late observers, as Trousseau and Belloc, deny the independent action of the lungs. (*a*)

(2) Elements of Physiol. vol. i. p. 475.

(3) Researches on Life and Death, c. 7, p. 116.

(4) See Brit. and Foreign Med. Rev. July, 1839, p. 186.

(5) Lectures on the Principles of Surgery, p. 38.

(6) Sœmering's Comparative Anat. p. 304.

(*a*) Trait. Pratique de la Phthisie Laryngee, &c.

spontaneous movements of plants are almost as readily observed as their living principle." (1)

"If the slightest impression be made upon one of the leaves of the averrhoa carambola, not only that leaf but all the neighbouring ones, and frequently some of the distant ones, *will contract themselves by sympathy.*" (2)

The phenomena of habit, also, in respect to the properties we are considering, are seen in the vegetable as in the animal kingdom. The mimosa pudica, for instance, ceases to contract in obedience to the stimulus of the wind after it has been accustomed to it. The power of mobility is lost by the exclusion of light; and when a plant is suddenly destroyed, the phenomenon cannot be reproduced. The principle is, therefore, a vital one. All other motions in plants must be referred, upon the same ground, to a like principle; and here we find the animal and vegetable kingdoms connected by the closest affinities. (Vol. I pp. 24, 594.) And since the sensible manifestations of motion in vegetables are very numerous, we may well infer, without the aid of analogies from the higher kingdom, that mobility is more or less the source of the various phenomena in vegetable life. Capillary attraction will not explain a single phenomenon, not even the simple motion of the sap, since this function has peculiarities to which that law alone is perfectly inadequate. (Vol. I. p. 593—596.) Eminent physiologists, as Don, Barbieri, Hedwig, Saussure, Brugmanns, Carradori, Townson, Hales, &c. infer from experiments that the sap is moved by a vital action of the vessels. (3) (P. 75—78.)

We see, by the foregoing illustrations, how different tissues are endowed with similar properties. Irritability is common to all. Mobility belongs equally to animals and vegetables; and the more we investigate the subject, the more we are inclined to the belief that sympathy may exist where we may be unable to appreciate a nervous system, however this power may be a principal characteristic of the brain and nerves. (See Vol. I. pp. 157, 474, 571.) Where the nervous system is present, the organic powers are mysteriously associated in all textures with the nervous influence. (See *ut supra.*)

"In the present state of our knowledge," Dr. Bostock thinks that, "con-

(1) Sir J. E. Smith's Introduction to Botany, p. 22.

(2) Girtanner, on the Laws of Irritability.

(3) Dr. Hales states that the sap of the vine circulates in the spring, "with a force eight times greater than the blood in the crural artery of a fallow doe." (*a*)

(*a*) Vegetable Staticks exp 36, p 114.

tractility ought to be regarded as the unknown cause of known effects; a quality attached to a particular species of matter possessed of properties peculiar to itself, and which we are not able to refer to any general principle." (1) (Pp. 103, 105, 117; Vol. I. pp. 75, 77.) Girtanner thinks "the irritable or contractile fibre is improperly called muscular." (2)

Going back to a consideration of the muscularity, or perhaps rather the mobile tissue, of the veins, it is observed by Mr. Bell that, "to say the venæ cavæ are not muscular merely because they are not red nor fleshy, is very ignorant." (3) The spirit of this remark is of extensive application, and should lead us to infer the properties of organization rather from its phenomena than its structure. Adopting the common philosophy, what should be our conclusions as to the ligaments, nerves, brain, all of which are fibrous? Do they manifest motion? "We know nothing of their properties that is characteristic, except only the negative ones of those of the muscular texture." (4) The only test, therefore, of muscularity, in a generic sense, is the contraction which follows the application of stimuli, or, more properly, the natural stimulus of the part whether physical or moral. In every system of admitted muscularity, there are very remarkable peculiarities appertaining to each; especially as it respects their vital forces, and their adaptation to stimuli.

If the veins, therefore, possess the active principle of contractility, and it be in virtue of this property that they diminish their diameters, it is the dictate of all reason that they equally possess a power of dilatation, without which contractility would defeat its own final cause. The contractions necessarily suppose alternate dilatations. We shall see this principle powerfully operative in the heart, and analogy suggests that it is not only cöextensive with the vessels which are propagated from it, but refers the coincident motions to a common cause. The function of elongation possessed by vermicular animals (5) not only shows the cöexistence of antagonizing forces, if we may call them such, in the same muscle, but it conveys the lesson that we can assign no limits to nature where she has specific purposes to answer. That the property of elongating does not belong to the compound muscles may be inferred from its uselessness; and, by parity of reason it should exist where there is no antagonist.

(1) Elements of Physiol. vol. i. p. 174. (2) On the Laws of Irritability.
(3) On the Mechanism of the Heart, &c. (4) Bichat's Gen. Anat. vol. ii. p. 267.
(5) Although the leech has longitudinal and circular fibres, it would seem that the former must contribute to the elongation of the animal.

But, what is especially important to us, if the fact must be established by experiment, Hastings proved that veins not only contract under certain stimulants, but that they are *dilated* by others. "A saturated solution of muriate of ammonia was applied to a vein, where a dilatation of the vein and the capillaries connected with it came on. In five minutes, by the application of spirit of wine, the vessel contracted." (1) This, and some analogous experiments, have been allowed to slumber, whilst we are offered a profusion of negative results to prove that the veins and arteries have no vital action. (2) But, it is manifest that one affirmative demonstration cannot be superseded by negative results. (P. 25.) It also illustrates another important fact which is indicated by various natural phenomena, viz. the predominance of continuous sympathy in the veins, so that when one part is roused to dilatation, the action is propagated extensively. This is not only important in a pathological sense, but, as we shall see, in respect to venous and even arterial circulation. Mr. Hunter remarks,—

"It is curious to observe how vessels become enlarged upon any irritation; not only the arteries, but the veins; and, *not only the smaller branches, but the larger trunks.* This was evident in the following case. I applied a caustic to the ball of the great toe of a patient every other day, for more than a month, and, after *each* application, the surrounding parts put on a blush, and all the veins *on the top of the feet*, as well as *up the leg*, immediately began to swell, and became large and full. This was so remarkable that the patient watched for this effect, on the days on which the caustic was applied, from its happening only on those days." (3)

The applicability of these results to our doctrine of venous congestion, in explaining the analogous phenomenon, is sufficiently obvious. Nor should it be overlooked that the veins when dilated by the foregoing causes were more or less permanent.

More than a year after the substance of the foregoing remarks was transmitted to London, the following observation was made by Professor Graves, which increases our confidence in the truth of our views.

(1) On Inflammation of the Mucous Membrane of the Lungs, p. 58. Also his Experimental Inquiry of Inflammation, table 2d. exp. 35, 38, 39, 40, 41, &c.

(2) Berzelius, (*a*) and some others, say there is no fibrin in the middle coat of the arteries, and thence conclude that they have no muscular action. Here we have an induction from the laboratory, which is also fully carried out by Müller. (*b*) Magendie, (*c*) and others, consider "the middle coat of the veins as consisting chiefly of fibrin."

(3) On the Blood, &c. p. 158.

(*a*) On Animal Chemistry. — (*b*) Physiol. vol. i. p. 202. — (*c*) Précis. Physiol. p. 215.

"I have already spoken," says Prof. G. "of the dilatation of the arteries and veins of inflamed parts, as being produced by something very different from mere distension, and that it is not of a passive but an active nature." He supposes the veins, as well as the arteries to "dilate actively." (1)

Dr. Thomson obtained exactly the same results as those of Hastings, from different irritants when applied to the arteries.

"The most *singular phenomenon*," he says, "which presented itself in the experiments with the solution of salt, (hydrochlorate of soda) was, undoubtedly, the *dilatation* of the artery and its branches; whilst directly the *reverse* of that took place from the application of volatile alkali." (2)

So, also, Dr. Hastings, who states that, "an increase of dilatation and contraction in the exposed part of the vessel, (artery,) whilst the stimulus was applied, was not an uncommon occurrence." By transfixing a *vein* in a frog's foot with a needle, "the blood moved with as much celerity in an *opposite direction* towards the veins of the opposite toe." (3)

Müller, who is disposed to deny the active dilatation and elongation of all muscular fibres, ultimately supplies a fact as if in proof of his error. After showing satisfactorily that the mucous coat of efferent ducts of glands has on its exterior an invisible "layer of muscular substance," (Vol. I. p. 688,) states that —

"He has observed in a bird, just killed, contractions of the ductus choledochus occurring at regular intervals in pauses of several minutes, the tube *dilating* again in the intervals; and, what was *remarkable*, the contractions took place in an *ascending* direction." (4)

We shall soon endeavour to show that venous circulation is mainly dependent on the absorbent power of the heart, and that the contractile and dilative power of the veins is chiefly designed for another purpose. But, in the glandular ducts, the intestines, &c. the final cause of the foregoing principles is that of moving forward their contents. For this reason, it is always in operation, and obeys the impression of various stimuli. Being however, otherwise in the veins, contraction and dilatation are here mainly determined by the stimulus of sympathy with the arterial capillaries, and by the stimulus of distension, —contraction and dilatation taking place according to the varying conditions of the arterial capillaries, and the quantities of blood transmitted.

When Cruveilhier, and others, injected irritating substances into the veins, in a direction from the heart towards the extremities, the veins became, immediately, greatly enlarged. Did the

(1) On Inflammation, &c. in Lon. Med. Gazette, July. 1838, p. 605.
(2) On Inflammation, p. 75–89. (3) Ut cit.
(4) Elements of Physiol. vol. i. p. 476.

stimuli produce a state of passive relaxation; if this be intelligible in respect to living tissues?

Hunter, and others, have long since shown that the vena cava pulsates in the higher animals. Nysten, Wedeymer, Müller, have proved the same, extensively, of warm and cold blooded animals. Müller has seen a "regular pulsation in the veins of young warm blooded animals;" and, "the venous trunks of the frogs continue to contract even after the removal of the heart and auricle." (1) Fleurens has seen regular contractions of the large veins in the abdomen. More recently Dr. Allison has performed some ingenious experiments upon the "Venous Pulse."

"It appears," he says, "that the alternate contraction and dilatation of the great veins of the heart must exert a considerable influence in promoting the circulation of the blood." (2)

We have no doubt of the accuracy of this induction, as it respects the veins in the vicinity of the heart. But our present object is to say, that the foregoing law, modified according to its final cause, probably pervades the whole venous system. This principle, being established, clearly subserves the functions of contraction and dilatation which adapt the smaller veins to their varying quantities of blood, and explains their permanent dilatation when their vital forces are modified as in venous inflammation and congestion.

The fibres of the veins being longitudinal, as we have now seen according to the best observers, and none of them circular, (3) are most favourably arranged for extensive and uniform operation, so indispensable to the sudden and varying proportions of blood transmitted from the arteries. They are not designed, as in the arteries, to propel the current of blood by a consecutive series of contractions; but mainly to enlarge the diameters of the vessels, and to embrace firmly the moving column of that fluid. Mr. Hunter, however, believed "that the veins have considerable power in carrying on the circulation." (4)

The important fact should be also considered, that filaments

(1) Müller's El. of Physiol. vol. i. p. 204. Nevertheless, it should be stated that Müller can see no reason for extending the foregoing facts, in the way of analogy, &c. to other veins; but relying on the old doctrine as to the failure of galvanism to excite contractions, he denies here, as in the case of the arteries (p. 150,) vital contractility to the other veins.

(2) In American Journ. of Med. Science, Feb. 1839, p. 306.

(3) Meckel's Gen. and Patholog. Anat. vol. i. p. 127.

(4) On the Blood, &c. p. 185.

of the sympathetic nerve are every where distributed to the veins. By their anatomical structure, therefore, the veins are obviously capable of the functions which we suppose them to perform, susceptible of disease, and liable to receive and to communicate impressions through the principle of sympathy. Thus, too, we see how an impression made upon the sensitive capillaries of the veins may, by sympathy, produce an instantaneous disposition to a state of contraction or dilatation over a great extent of the vessels. This may be illustrated by referring to the universal contraction of the polypus, or worm, when any part is pricked with a pin, or, to a similar phenomenon in the leaves and stems of the averrhoa carambola.

If we be right respecting the final cause of the middle coat of the veins, we should expect its absence whenever it might be incapable of the imputed functions. Accordingly we find that, "the sinuses of the brain, and the veins of the bones are almost entirely constituted by the inner membrane;" (1) and they are, if we receive the statements of Bichat, and Beclard, and some others, the only veins which are destitute of the fibrous coat. Here we see the safety of reasoning from final causes, where they are obvious, and how analogy must apparently yield in the organization of nature, where it conflicts with Design. For the foregoing reason, too, the fibrous coat abounds most in the vena cava, and so on in ratios corresponding to the size, situation, and special functions of the veins; just as we find the longitudinal fibres of the large intestines exceeding those of the small, those of the rectum exceeding those of the others, &c. In the last instance, indeed, there is not a little analogy with the vena cava as to its office of aiding especially in the projection of the blood into the right cavities of the heart. Again, in the organic muscles, generally, the fibres interlace each other, because they have compound motions to perform; but, in the veins the motions being simple, so also are the fibres. Without any reference to final causes, Mr. Hunter supposed that,—

"The muscular powers are much greater in what may be called the ascending veins, than either the descending or horizontal; for, if we look at the back of our hand, and compare their size in a warm day, or before a fire, and in a cold day, they hardly appear to be the same veins." (2)

This is borne out by the Philosophy of Design. Where the principle of gravity is generally operating in favour of the cir-

(1) Bichat's Gen. Anat. (2) Hunter on the Blood, &c. p. 180.

culation, we should infer, a priori, that the contractile power would be less than where gravity counteracts the circulation. Thus we find that the jugular veins are comparatively passive tubes. They become distended when gravitation is reversed; and, this law is here partially and beautifully substituted for that of contractility, since, were the contractile power as great here as in inferior parts of the body, the descent of blood from the brain would have been more or less embarrassed. For the same reason, the vital forces, generally, are less strongly marked in the descending veins, as we infer from their comparative exemption from varix, &c. This remark, however, is not intended to apply to the veins of the encephalon, where, it is manifest, the power of gravity does not operate as in those of the neck, and where, from their connection with the nervous centre, we may suppose their vital forces to be strongly pronounced.

We may also say, that the quantity of blood in the veins of the lower extremities is about the same in the erect as in the horizontal posture; though, as we shall see, a more important cause must be assigned for this phenomenon. Mr. Hunter also supposed the veins to possess an active power of dilating and contracting.

"Their muscular power," he says, "adapts them to the various circumstances which require the area to be within the middle state, and assists the blood in its motion towards the heart." (1)

"The muscular fibres of the veins," says J. Wilson, "have, in cases of great loss of blood, and in animals bled slowly to death, contracted those vessels so as very nearly to have obliterated their cavities." (2)

We have witnessed this phenomenon in the veins of the brain, after a rapid destruction of the animal by loss of blood. (3) It is worthy of remark, however, as illustrating the vital nature of the function, that the power of contracting and dilating is destroyed by stretching the veins.

The veins, therefore, possessing longitudinal fibres, are fitted for rapid and uniform operation over an extensive tract. This action, whether with or without fibres, implies a predominence of continuous sympathy; and, this constitutional principle, which is designed for an important function, and depending, of course, upon peculiarities of organization and vital modifications, is the probable cause of the susceptibility of the veins to inflammation,

(1) Ibid. p. 181. (2) Wilson on the Blood, &c. p. 76.

(3) See our paper on the Organic Functions of the Brain, in Med. Chir. Rev. Lon. April, 1834, p. 433.

and analogous changes, and the foundation of their rapid extension. (1)

Will it be said that the veins in the natural circulation, are mechanically dilated by the ingress of blood, or, that their swelling in the extremities from the application of heat is a proof of simple relaxation?

"Cooks," says Sir B. Brodie, "are very subject to varicose veins. Why? If you put one hand into warm water, and the other into cold, you know that the veins of the former become dilated, and that those of the latter will contract." (2)

This illustration is, in itself, a contradiction of the intended philosophy of our author, since cooks are not subject to varicose affections of the upper extremities, which are alone, though constantly, exposed to "warm water." Indeed, in this lecture from which we quote, Sir B. speaks, on account of its great rarity, of two instances of varix of the upper extremities, which he had witnessed, and one of these was the consequence of an obliteration of the venous trunks, and the other was occasioned by the pressure of a tumour.

(1) M. Velpeau attributes the liability of veins to inflammation, to the circumstance of their carrying the recrementitious parts of the body. (*a*) This is a new step in the humoral pathology. Hitherto, the deterioration of the blood has been supposed to depend, at least, upon foreign agents, primarily; not upon the introduction of the molecular parts of the healthy system into the circulation by a natural process.

As this is probably the last time we shall advert to the humoral pathology, we have concluded to notice in this place what we designedly omitted in our Supplement. It is a case of hydrophobia, which we supposed would be sufficiently embraced in what we have said in our first volume pp. 489 — 503, 537 — 539, 716. We find the case, however, in circulation, and being apparently opposed to our construction, it is entitled to a respectful consideration. "Two sheep were bitten by a dog labouring under hydrophobia." About six weeks afterwards both sheep were attacked with the malady, and were both killed. One of them had two lambs, the other, one. Their offspring shared the same fate soon after the death of the old ones, and it is supposed that they imbibed the poison from the mother's milk, as in the celebrated case of the *Indian-hachy*, (vol. i. p. 537.) It is, therefore, considered a "Peculiarity in the Transmission of Hydrophobia." The ground of the inference is, that "not the slightest *scar* could be discovered "upon the lambs," and "they were removed from the ewes a month before the latter became affected." (*b*) As to the first reason, we refer to what we have already said in respect to the morbific effects of poisons where the recent pricks of a knife could not be detected by the microscope, (vol. i. p. 489;) whilst a *scar* may be invisible where a wound had been obvious, especially when concealed by a fleece of wool. The second reason embraces its own objection, since, if the lambs "were removed from the ewes a month before the latter became affected," it is quite obvious, even upon the humoral doctrine, that they were removed a month before the blood or secretions of the dam had become affected. (Vol. i. p. 533 — 536.)

(2) Clinical Lect. in Lon. Med. Gaz. Oct. 1837, p. 185.

(*a*) On Diseases of the Lymphatic System. (*b*) Lon. Med Gaz. Oct 25, 1839.

Again, if Sir Benjamin's philosophy be correct, should we not meet with this affection amongst glass blowers, and other artizans who are constantly exposed to intense degrees of heat? The operatives at the great iron-works of Bowling, Low-Moor, &c. are often exposed to a heat of 130° — 150° Fh.; yet these men are not only healthy, but, in no respect liable to varix, or other "relaxations" of the veins. Ramazzini thinks they must be unusually subject to disease, — "non possunt quin gravibus noxis afficiantur." Upon which Tackrah remarks, that in this, as in many other instances, Ramazzini's conclusions in respect to the effects of the arts "are rather from *a priori* reasoning than from actual observation and large examination." "The same disposition to error," he continues, "I have repeatedly detected in myself;" and who has not?

It appears to us, that there is a great error in applying the effect of heat upon inanimate matter to vital parts, which are well known to resist the laws of caloric as they are manifested in the inorganic world. It is a part of the common mistake of confounding the physical with the vital laws. We relax dry, dead matter by soaking it in warm water. The effect is purely mechanical. Dry heat, however, contracts and dries it the more, whilst it will produce as great an expansion of the veins as warm water. It is manifest, therefore, that the phenomena in the two cases depend upon utterly different principles; and, there is no conceivable mode of explaining the dilatation of the veins, but by supposing an action of dilatation, through what are known as the powers of irritability and mobility. It would, however, be absurd to say that we act upon the irritability of dry leather. The *rationale* of the vital example appears to us to be this. Heat stimulates the vital properties of the arteries, and increases their action. More blood is thus transmitted to the veins. We may, therefore, rationally look for some corresponding properties in the veins by which their capacity shall be increased. We think it must be admitted that they possess a principle of sympathy, which is designed to establish a harmony of action betwixt the two great systems of vessels. We believe that we are warranted in proposing this doctrine by all that is known in relation to the veins, to their circulation, and by the whole science of physiology. This consent of parts, indeed, appears to be very strong in the veins, where the provision is so important to the varying changes in the arteries; (Sec. 11, and Vol. I. p. 157 —

161,) is coincident with what we have shown of the high endowment of the venous tissue with the vital properties; (Sec. 8,) and, it seems to us that the action of the veins depends mainly upon it; and that it will account for the failure of some experimenters to excite them by electricity, &c.

Here, too, we have a probable reason why the veins collapse when divided, since their sympathetic relation with the arteries is thus extinguished. The veins, indeed, appear to be not less susceptible of action from the stimulus of sympathy than the iris itself;(1) whose phenomena so clearly demonstrate the operation of that principle in developing sensible motion, show us that there is no limit to the modifications of the vital properties in structures apparently alike, and that the modifications of a common disease may be partly determined by these peculiarities.

Experiments having clearly established active dilatations of the veins, the result must be carried to the interpretation of the same phenomena when taking place naturally. We may also say, that all experiments upon the veins when this property is impaired, or abolished, prove that the venous tissue has less elasticity, than the arterial, whilst the dilatations of the former are incomparably greater in the natural states of each. The veins, indeed, actually collapse, when they are divided across; a phenomena of no little import. And, since the whole of this investigation is designed to illustrate our doctrine of venous congestion, we may say here, in respect to the morbid dilatation

(1) Parallel illustrations are important, where great principles are concerned. The iris, which is more abundantly supplied with nerves than any other part of the body, loses its contractile power when its sympathetic relations with the retina are interrupted by paralysis of the latter organ; and, it is again restored when those relations are rëestablished. (Vol. i. pp. 157 — 161, 474 — 480, 568 — 575.) According to Sir C. Bell, and others, the iris does not contract when divided in certain operations upon the eye. We see something analogous to the foregoing, (and not less applicable to what has been urged against the vital actions of the veins from the failure of electricity to excite them,) in the vegetable kingdom. Dufay, (*a*) Duhamel, (*b*) (not "the celebrated," vol. i. p. 715,) Ritter, (*c*) Decandolle, (*d*) and especially Dutrochet, (*e*) have ascertained that the mobility of the sensitive plant depends on the continued influence of light; and that, when withdrawn from its action, the movements cannot be excited by the usual irritants, but readmission of light restores the natural irritability.

(*a*) Obs sur la Sensitive, in Mém de l'Acad. de Paris, 1736, p. 87. — (*b*) Physique des Arbres, art. Sensit. — (*c*) In Gehlen's Jour. fur Chimie, &c. t. vi. p. 470. — (*d*) Expériences relatives à l'Influence de la Lumière sur quelques Vegetaux, in Mém. l'Institut. t. i. p. 328. — (*e*) Obs. sur les Mouvemènts de la Sensitive, in Rech. Anatom. et Physiol. sur la Struct. des Animaux et des Vegetaux, t. ii. 1824.

of the veins, that it appears probable, as will be more fully set forth hereafter, that the first morbific impression is upon their capillaries; and the action established here, whether directly by the morbific agents, or by some primary disease in other tissues of the affected organ, is propagated along the trunks by continuous sympathy. It is upon the capillary veins, also, that the capillary arteries exert the first impression when variations take place in the quantities of blood transmitted. The same principle which determines the extension of disease and a consequent dilatation of the veins in the former case, operates in the latter, with the stimulus of an increasing quantity of blood, in developing extensively the natural action of dilatation. (1)

It now becomes apparent, why, if the enlargement of the veins in venous congestion depend on an active dilatation of the veins, that dilatation is permanent, and is not followed by corresponding contractions, as is more frequently happening in the natural

(1) "The subcutaneous veins," says Dr. Wardrop, "perform the office of a receptaculum, by receiving any superfluous quantity of blood which cannot be admitted into the pulmonary vessels." "When the lungs become congested, and can no longer admit into their vessels any additional quantity of blood, not only is the systemic blood driven into those arteries which are not influenced by muscular compression, but we observe that the venous blood, which cannot find access within the thoracic cavity, stagnates and becomes accumulated in the veins. No more striking example can be given of this office of the superficial veins, than their distended appearance beneath the delicate skin of a race horse after a severe gallop." (*a*)

We notice this statement on account of its high authority, and the frequency with which it occurs in authors. We believe, however, that the phenomenon is owing, wholly, to the cause which we have just assigned in the text, and to muscular compression; not at all to the mechanical one surmised by Dr. Wardrop, and others. In the first place, it is a mere assumption to say that the lungs are in a state of congestion; and were they so, it would not impede, as we shall endeavour to show, the free transit of blood. We have seen too, already, (p. 248—257,) on high authorities, that nothing short of change of structure in the lungs, tumours, or some analogous cause, can constitute an obstruction to the pulmonary circulation.

But, this is not all. If pulmonary congestion actually exist, and be the cause of the accumulation of blood in the subcutaneous veins, the following conditions must happen. The blood, in the cases supposed, must first accumulate about the right cavities of the heart. Upon this, the pulmonary injection will depend. So, also, must the distension of the subcutaneous veins depend upon the same cause. But, we have already shown, that, according to a law in hydrostatics, if there be a remora of blood into the subcutaneous veins from an accumulation about the right cavities of the heart, the same remora must be coextensive with every other part within its greatest range. But, since no such remora occurs, the distension of the subcutaneous veins clearly depends on a wholly different cause. Precisely the same conditions must arise, if the supposed congestion begin in the lungs.

(*a*) On the Nature and Treat. of Diseases of the Heart, Part I, p. 52, 1838.

state of these vessels, and constantly in the heart and arteries. It grows out of the constitutional endowment of the veins with the power of dilating actively, and of maintaining that state of dilatation, for an indefinite time, to suit the emergencies of the circulation. When special morbific causes operate, they so influence that principle, as we have seen in experiments, (p. 387,) that it is held in permanent operation, till the vital forces are brought back to their natural state. But it is otherwise with the heart and arteries. They have constant alternations of contraction and dilatation, since their actions are designed to carry on the circulation, whilst, as we shall endeavour to show, the action of the veins is mainly intended to advance, upon another principle, the final cause of the former, as it respects the circulation. But, in venous congestion, it should be observed, that there has not existed that cotemporaneous afflux of blood to the smaller arteries, upon which the dilatation of the veins depends in their natural state; and for the relief of which the dilatation takes place. It is not, therefore, the natural cause which operates, whether in respect to vital action or mechanical distension, since there is no arterial plethora to relieve, and no increased transmission of blood. Since, also, as we have endeavoured to show, the accumulation does not depend on relaxation of the veins, or on mechanical causes, some disease must affect the coats of the veins, by which they are brought into a state of active dilatation.

Why do not the veins contract, like the arteries, when they are divided? For the plain reason that the vital properties are not alike, and, therefore, do not obey the same influences. The two systems of vessels are constituted for very different functions. The arteries possess a power of acting that qualifies them for aiding the heart in maintaining the momentum of blood. The suction power, or pressure of the atmosphere, on which venous circulation mainly depends, as we shall endeavour to show, is so fully adequate, that it requires not the aid of venous action; and the veins, therefore, are endowed with the power of dilating and contracting that shall adapt them to varying quantities of transmitted blood. This principle is maintained in operation by sympathy with the arterial capillaries; and, therefore, whenever this relation is destroyed by dividing a vein, the power of contracting and dilating disappears with it, (1) just as the iris refuses

(1) It is said that, "when a portion of varicose saphena is excised, it immediately contracts so as scarcely to admit the passage of a probe. The inner coat of the vein

to contract when the retina becomes insensible. The very fact, indeed, which we are considering is a full proof of our principle. It appears to us that nothing can surpass this beautiful, harmonious, and never ceasing consent of parts. Active power is also given to the arteries; since, were the whole force necessary, even to arterial circulation, bestowed upon the heart, the aorta itself would be either stretched or ruptured in its curvature. Whilst, on the contrary, atmospheric pressure is equal to the exigencies of venous circulation, it does not endanger the venous tissue, since its pressure is everywhere alike.

It is readily seen, therefore, why different phenomena are manifested where common experiments are made upon different tissues possessing the function of sensible motion.

From what we have now said, we infer that the final cause of the power of mobility in the veins is chiefly designed to subserve the arterial system, whilst it establishes a *conatus* in the veins to contract on their contents, and in this way to promote the return of blood to the heart; being ever liable, however, to those changes which attend antagonizing forces. Did not a consent of action with the arteries, depending on the principle of sympathy, exist in the veins, the vital contractility, and the elastic property of their coats, must be overcome by every increased quantity of blood that may be transmitted to them, or, in other words, by the action of the capillary arteries. This is utterly repugnant to that unity of design which prevails in the organized system, and would be leaving an important function to a fortuitous and even inadequate provision. Is it probable, too, that the phenomena which appertain to one class of vessels are of a vital nature, and those of the other, resulting in a connected series, mechanical? The laws which govern living are so unlike, and even opposed to, those which operate on dead matter, it would seem that this general principle should discourage the belief in mechanical distension.

is then found corrugated into longitudinal folds, while the external is considerably thickened" (*a*) The phenomenon is owing to a morbid condition, which had established the dilatation, and to the new impression upon the vital properties of the part by the excision. Tho experiment shows the active nature of the dilatation, and its great independence of disorganization when that consequence has ensued to a certain extent. As the latter advances, the vein may assume the condition of a rigid cylinder.

(*a*) Edin. Med. & Surg. Jour. vol. xxv. p. 238, *note*.

SECTION XI.

THE POWERS WHICH CIRCULATE THE BLOOD.

"The vascular circulation is effected chiefly by mechanical action; the capillary and arcolar circulation, by vital or dynamic forces. The phenomena belonging to the two kinds of circulation have no analogy, either as connected with physiology, pathology, or therapeutics. They must be studied separately, and the reciprocal influences, and individual peculiarites of each be determined. We shall hereafter have occasion to show, that in pathology and therapeutics, the most vital errors have been committed, and are daily perpetrated from confounding the two, and regarding the circulation as a unity, to which the same general principles are applicable in settling the diagnosis of disease, and the operation of curative means." — S. JACKSON. *Principles of Medicine, &c. p.* 484. 1832.

It is important to our propositions to consider the powers which circulate the blood, — especially the causes of venous circulation. These causes disprove, in no small degree, the doctrines of obstruction and stagnation of the venous current. The true philosophy of the circulation of the blood has, also, an important bearing upon that of the operation of the loss of blood, and should be connected with our essay upon that subject. We accomplish in this section, also, the farther great object of demonstrating the independence of the processes of organic life of the physical powers.

The experiments of Hales had early satisfied many philosophers that the action of the left ventricle of the heart is wholly inadequate even to arterial circulation; whilst Harvey propounded an unobjectionable cause for venous circulation in the suction power of the right auricle. To this we may add the theory of Bichat, Philip, Hastings, &c. (1) which regards the action of the

(1) Who suppose that the blood in the capillary system is beyond the influence of the heart, where it circulates by the tonic power of the capillaries, and that the entire venous column is raised by this capillary action. — *See Bichat's Gen. Anat. vol. ii. p.* 34.

"On the other hand," says M. Magendie, "my opinion is, that the capillaries have nothing to do with the matter, and that they must be completely stripped of the pretended functions with which they have been invested." (*a*) We do not see, however, that he assigns any good reason for this opinion; no better than on former occasions. (See vol. i. pp. 397, 398, 511, 514, 518, 697.) We cannot understand how the experiments of Poiscuille, if we allow them to have their intended weight, conflict with the imputed agency of the capillary system in venous circulation. Indeed, we think their tendency is just the reverse; though, for various reasons which we have assigned, we have not much confidence in these artificial results. Nevertheless, in a theoret-

(*a*) Lectures on the Blood, p. 112, tr.

extreme capillary arteries; and also the constant *conatus* of the veins to contract on their contents. There appears to have been a common error in endeavouring to explain venous circulation by a single cause; whereas it seems to depend on the concurrence of several causes,—all acting together in beautiful harmony. (1) Nevertheless, the power of derivation, or suction, or atmospheric pressure, is the principal agent.

We assume that the active dilatation of the heart is sufficiently established, although it is unimportant to our specific purpose whether it be the result of vital action or of mere elasticity. Nevertheless, as it is still a controverted question, and nearly concerns our position as to some great principles in physiology, and especially to supply an analogy that shall bear upon the doctrine of active dilatation of the blood-vessels, it may be well to examine the proof in favour of the physical construction.

Müller thinks it has been fully proved by Oesterreicher, that the dilatation of the heart is not an active movement. His experiment consisted in placing a weight upon the heart of a frog, when it appeared that the weight was raised by the contraction of the organ, which became flat when it dilated. (2) We can well believe that the observer was perfectly accurate as to the elevation of the weight when the heart contracted; and, indeed, from the nature of the experiment we see not how it could have been otherwise. It appears that "the weight was so heavy as to press the heart *flat*." Surely, then, when the organ contracted it would be in the direction of one or both axes, by which a swelling up must necessarily have taken place, and a correspon-

ical sense it would subserve our purposes better to assign the whole function of venous circulation to the suction power of the heart.

The British and Foreign Medical Review, (*a*) in noticing the foregoing declaration of Magendie, remarks that, he "tosses to the wind all opinions opposed to his own as a whimsical assemblage of absurd suppositions." The Reviewer adds, "is it not strange to find M. Magendie afterwards freely admitting that the various local changes which take place in the circulation, independently of the action of the heart, depend on something peculiar — something which has not, hitherto at least, come within the domain of physics? These changes, he supposes are produced by the influence of innervation; and he adduces the well known fact, that when a part is deprived of its supply of nervous energy, the circulation is immediately disturbed, or even suspended. May not M. Magendie's unknown something consist in the *vital properties* of the extreme blood-vessels which he regards with so much contempt."

(1) All these agencies, as will be seen by our references, were pointed out by Mr. Hunter in different parts of his work on the blood.

(2) El. of Physiol. vol. i. p. 173.

(*a*) Oct. 1839, p. 327.

ding elevation of the weight. But, on the other hand, when the active expansion occurred, it would happen in lateral directions from the pressure of the weight, which would consequently descend. This corresponds exactly with what is stated; for, when the heart dilated, it became *flat*. It will be seen, too, that it is admitted that dilatations took place; and how does the experiment show that these movements were not more the result of an active principle than of the pressure of the weight? Least of all, how does this unnatural experiment show, in the slightest degree, that the dilatations which occur when the heart is not encumbered by weights, are not purely of a vital and active nature? (Vol. I. p. 511, *note*.) It appears to us that the experiment proves more than was intended, as we shall soon see of some others.

Müller shows conclusively, that "the cause of the heart's contraction must be in *some way* connected with the organization of the heart," and is more or less independent of the stimulus of the blood. (1) This is all we know of it, except that it is determined by vital properties. And since the contractions and dilatations are so intimately related to each other that one could not take place without the other, it would seem to follow, *a priori*, that the cause which determines the contractions governs, also, the dilatations. As to the want of analogies among voluntary muscles, we have already endeavoured to remove that objection, (Sec. 10;) and we may once more say that it appears wholly contradictory to Unity of Design, and all that is known of organic actions, to suppose that the act of contraction is vital and the associated rhythmical act of dilatation, physical. This blending of forces so utterly distinct has naturally led to conflicting opinions as to the particular allotment of one or the other to the alternate actions of the heart; Lower, and others, maintaining that the *systole* is the passive state, and the *diastole* the violent one.

If the power of contracting were antagonised by a mechanical one, they must bear the relation to each other of a compressing and a resisting force; and, therefore, agreeably to a law in mechanics, the heart must restore itself with a force equal to that with which it is compressed; the supposition being that the elasticity is perfect. Is it probable that so great a degree of elasticity, if it possess any of a truly physical nature, can appertain

(1) Ibid. p. 188.

to a muscle which is so largely composed of fluids? Is there any parallel in the phenomena of inorganic matter, of analogous composition; taking as the measure the power with which the heart performs its contractions? Nor can it be supposed that any condensation of the substance of the heart can be produced by the vis insita, which is in vain attempted by art. (1) Is there any evidence other than the dilatation of the heart which should lead us to ascribe a greater degree of elasticity to that organ than to the voluntary muscles? When contraction ceases in the muscles of animal life, they are restored by their antagonists. The active principle would, therefore, be useless; although Bichat thinks that they are sometimes "the seat of true active dilatation." (2) Meckel affirms of the muscles generally, that "they possess an active power of elongation," and that "many facts are explained in a forced and unsatisfactory manner, if we suppose that elasticity alone contributes to extension." (3) It is also a radical objection to the doctrine of elasticity that it would be an unexampled principle in organic actions, and an unphilosophical introduction of a force to explain a phenomenon whose antecedent is admitted to depend upon the vital powers.

On the other hand, if the dilatation of the heart be supposed to depend on a vital principle, we have a power that coincides with the principle of contractility, and which is analogous to the law in mechanics, by which a body possessing perfect elasticity restores itself with a force equal to that with which it is compressed. That such a force actually exists, appears from the experiments of Pechlin, (4) since confirmed by Magendie, and others. The latter "has often seen the heart in animals, recently dead, raise a weight of twenty pounds." "It is impossible by any manual power to prevent the dilatation of the heart in a living animal however small the organ." (5) Pechlin and Bichat assert the same thing; and here the experiment which Müller considered so conclusive against us is quite to our present purpose. But, Bichat found that the "force is incomparably greater in the phenomena of life than in our experiments;" and he

(1) Dr. Goddard, in 1669, (a) was supposed to have settled the question as to condensation, by the subsidence of water in a vessel when the muscles of the arm were contracted. But this was owing to the expulsion of blood from the veins, &c.

(2) General Anatomy, vol. iii. p. 54. (3) Gen. and Patholog. Anat. vol. i. p. 265.

(4) De Corde. (5) Précis, &c. p. 329.

(a) Register of the Royal Society, vol. iv. p. 95.

shows, as was done by Pechlin, that the heart dilates with as much force as it contracts, and, what is very material, if the heart be irritated when at rest, it frequently *dilates before it contracts.*

"I have many times observed," says Bichat, "at the instant in which I irritated the heart with the point of the scalpel, that a *dilatation* was the *first* consequence of it, and that *contraction* was only *consequent* to it." His researches led him to conclude that "the dilatation of the organic muscles is a phenomenon as vital as their contraction." (1)

"It appears to me," says Dr. Davies, "an abuse of terms to say that that is a passive relaxation which a corresponding force cannot prevent." (2)

And here is a strong illustration from Lord Bacon: "Equidem meminimus ipsi vidisse hominis cor, qui evisceratus erat, (supplicii genere apud nos versus proditores recepto) quod in ignem de more injectum *saltabat in altum*, primo *ad sesquipedem*, et deinde gradatim ad minus; *durante spatio* (ut meminimus) septem aut octo minutarum." (3)

And here, too, we may refer to the dried heart, Vol. I. p. 17. Can it be that those hearts dilated in virtue of any elastic property? Try it, by compression, on a dried bladder. Yet may even this experiment fail, because "we cannot comprehend the nature of the vital forces."

"The contractile power of the heart," says Dr. Philip, "is one of the chief sources of the circulation of the blood; yet, such is the love of singularity, that even this position has been denied." (4)

What is the exciting cause which determines the muscular action of dilatation? Mainly the stimulus which arises from the state of contraction, and partly that

"Necessity," as expressed by Mr. Hunter, "which is demanded by the constitution for its alternate contraction and dilatation." "This we see to be the case whenever a constant supply, or some kind of aid, is wanted in consequence of some kind of action. We have thus the stimulus for respiration," &c. (5)

Dr. Philip has the same idea. (6) This, however, can scarcely be considered an explanation, nor is Bichat's any better, who calls it "a kind of instinct;" though we are more content with his philosophy at another time, when he says, "the heart dilates itself, because it has in itself the cause of its dilatation." (7)

"Sic curiosus vero requisiveris," says Haller, who bestowed great labour upon this inquiry, "cur a stimulo magis cor, quam alius musculus moveatur, nihil respondebunt alii, nisi phenomenon; alii hypothesin offerent." "Varios,

(1) Gen. Anat. vol. iii. pp. 48, 53, 54.
(2) Lectures on Diseases of the Lungs and Heart, p. 241.
(3) Hist. Vitæ et Mortis, in Op. t. iii. p. 374.
(4) On the nature of Sleep and Death, p. 70. (5) On the Blood, p. 149.
(6) Essay on the Means of Preserving Health, and other works.
(7) Gen. Anat. vol. i. pp. 336, 365.

et subtiles quidem, hypotheses physiologi proposuerunt." "Si hypotheses nos deseruerint, non est, quod valde queramur, summa enim hic simplicitas est." (1)

As to the real essence of the principle, all that we know of it is, that "*the Lord God formed man of the dust of the ground, and breathed into his nostrils the breath of life, and man became a living soul.*" He was no longer "dust of the earth," and no longer governed by physical laws; but, being now "made after the image of God," his very materiality bore an approximation in its moral and vital attributes to that pure Spirit Who had impressed His Own Features upon it, and Who farther exemplified the Nature of the "Image," and its affinity to the living principle, in the corporeal frame of His only Son.

It has been objected to the theory of vital suction, (for which we are mostly indebted to Wilson (2) and Carson,) that the motion of the venous blood would not be in an uninterrupted stream; that it would move much more quickly during the diastole, than the systole of the auricle. (3) But this difficulty is at once removed by connecting with the suction power of the heart the projecting power of the capillary vessels; which urge on their contents in an equal stream, as shown by Mr. *Hunter*. (4) Nor can we doubt that something of this nature is contributed by the action of the capillary veins. The larger series being so highly endowed with the principle of adapting themselves to their varying contents, it is an obvious conclusion that this function is more strongly pronounced in their capillaries, where the vital forces are in a state of energy which corresponds with the propelling motions of the capillary arteries, and which are constantly influenced, through the law of sympathy, by the varying states of the latter. In the natural condition of the circulation, also, it can hardly be doubted that the suction power of the heart is extended into those capillary arteries which communicate with the capillary veins: especially if it be true, as is contended by some of the best medical philosophers, that the lymphatic circulation is wholly determined by derivation. (5) And whilst such

(1) Element. Physiol. t. i. pp. 488, 504. (2) Wilson's Inquiry, &c., *passim*.
(3) Hastings' Exper. Inquiry, p. 9; and others.
(4) On the Blood, &c., pp. 185, 186. We use the Ed. of 1794.
(5) Armstrong says, "it is the chief if not the sole cause of the circulation in the

an extension of the derivative influence operates as a direct power, it must aid as an excitant to capillary action. (1)

There is another cause of the equal current of venous blood, which was considered by Mr. Hunter fully adequate to explain the phenomenon.

"A secondary vein," he says, "or one that is a third or fourth in order of size, cannot have a pulsation, because it has more than one cause acting upon it; for such a vein is receiving the impulse of the heart at very different times, owing to the larger trunk receiving blood by a number of smaller veins that come from a variety of parts. So that, if the trunk was to receive it by starts from the smaller veins, it would only be a tremor or confused pulsation. This is a reason why this cause could produce none in the secondary veins." (2)

And here is one of the rare instances of organic functions where a mechanical illustration may be introduced. If we continue to fill a sponge by repeated projections of water, the fluid will run out in a uniform stream. Or, we have, perhaps, a better illustration in a tube divided into many branches that again

absorbent system." (*a*) The experiments by Carus (*b*) and Autenrieth (*c*) of tying the thoracic duct only proves that there is a certain power operating from behind; but it also shows by analogy, that a similar power appertains to the capillary arteries which has no connection with the propelling power of the heart, and the analogy might be carried with propriety to the circulation of sap; whilst we thus lay a foundation for a doctrine of the dependence of inflammation, &c., upon vital actions of the capillary vessels. The experiments, however, prove nothing against the cöoperating power of suction, which is everywhere exerted upon an obstructed vessel through indirect channels.

Dr. Graves, and others, suppose that the functions of the lymphatics are, in certain respects, identical with that of the veins; the former being destined to return "white blood." (*d*) This opinion was prevalent till the time of William Hunter, who arranged the lymphatics and lacteals into one common system of absorbent vessels. A close affinity with the venous system is also shown by the apparently analogous endowments of the lymphatics, as evinced especially by their susceptibility to diffuse inflammation. And that they are also more or less subject to the laws which determine venous circulation is shown by the difficulty of assigning others; by their valvular structure; by the termination of lymphatics in the veins; by the absence of all jet, as demonstrated by Sœmering and Magendie, (*e*) — however their slow circulation may appear to contradict the theory. This doctrine being admitted, it would establish a special dependence of venous circulation upon the same cause.

(1) It has been strangely supposed that the suction power of the heart would close the orifices of the capillaries. But here their independent action is overlooked, as well as the constant supply from the heart.

(2) On the Blood, p. 186. — Dr. Armstrong, adopting the suction theory, explains the phenomenon of venous jet, in another way, but on Hunter's principle, — on Continued Fever, in his works, p. 177.

(*a*) On Continued Fever. — (*b*) Meckel's Archiv. t. iv. p. 420. — (*c*) Physiol. t. ii. p. 115. — (*d*) Lecture on the Function of the Lymphatic System. — (*e*) Précis Elem. de Physiol. t. ii. p. 224.

unite; when, by abrupt injections at one end, the fluid will issue from the other in a continued and equal stream. (1)

Other objections have been made to the doctrine on which we partially depend for some important conclusions relating to venous congestion; and, since they are but few, we shall notice the whole. Dr. Philip has stated one which is more ingenious than "insurmountable." Thus, —

"An insurmountable, and surely most evident objection to this hypothesis presents itself. What is the effect of an exhausting syringe adapted to a tube filled with a fluid? If the fluid must rise before it can enter the syringe, after the first portion has entered it, either the fluid continues to rise, or the sides of the tube begin to collapse. If the sides of the tube are firm enough to resist a pressure equal to that of the column of fluid to be raised, it rises; if not, the tube collapses. Could the sides of the veins resist a pressure equal to that of the fluid to be raised, even if they were not pressed upon by the surrounding parts? Nay, so small is the elastic power, that they collapse by their own weight as soon as emptied. Yet it is through these tubes that a column of blood of many feet is to be raised by suction! Besides, the elastic power of the ventricle can have no operation in the veins, the auricle being interposed between them, and contracting during the diastole of the ventricle." (2)

Dr. Arnott has also stated the same objection, (3) prefaced by the following remark: —

"Such a doctrine could not be proposed or entertained for a moment by a person understanding the principle of a common household pump; and, that it has been published and tolerated by certain professional men in the present time, will remain a proof to posterity of the deficiency, as regards fundamental science or natural philosophy, now existing in the ordinary medical education." (4)

We do not complain of this, as we espouse the doctrine whilst the penalty is impending. The theory is entertained by such hewers of philosophy, as Harvey, (5) Haller, (6) John Hunter, (7)

(1) See Wilson on the Blood, p. 187.

(2) Medico-Chir. Trans. Lon. vol. xii. p. 399. Also, his Inquiry into the Nature of Sleep and Death, p. 67; and Philos. Trans. 1831.

(3) So, also, Dr. Bishop, (*a*) Dr. Cross, (*b*) and others. It is considered "insurmountable" by all objectors.

(4) Elements of Physics, &c. vol. i. p. 451. (5) Ut cit.

(6) Element. Physiol. t. ii. pp. 325, 215, 328. (*c*) (7) On the Blood, p. 185, &c.

(*a*) American Med. Recorder, vol. xii. p. 55. — (*b*) Ibid. vol. xi. p. 322.

(*c*) Haller says, "deinde sanguinem in auriculam dextram, tanquam in *vacuum castellum* approperare, ne id quidem videtur absque specie veri dici;" &c. *Again*, "hujus derivationis tanta vis est, ut contra ponderis resistentiam, contra legitimum iter sanguis undique ab omnibus venis, in locum laxantum quocunque sub angulo apertis, aut per alios ramos cum eo consentientibus, se in vulnus conjiciat, maximoque cum impetu de rima venæ effluat." *See farther on.*

It is remarkable that Harvey should have hit, as he hints, upon the true philosophy of the return of venous blood. But this he may have borrowed from Servetus, who, following up the suggestions of Galen, ascertained the true pulmonary circulation, and expressed his belief that the blood is drawn from the lungs into the left ventricle of the heart, by the suction power of that organ. (*aa*)

(*aa*) De Trinitate; and de Restitut. Christianismi.

Huxham,(1) Blumenbach,(2) Armstrong,(3) Rudiger,(4) Platner, Wilson,(5) Carson,(6) Tiedemann,(7) Doelinger, Schubarth, Good,(8) Stromeyer,(9) Laennec,(10) Wardrop,(11) Müller,(12) Wedemeyer, Guenther, and others of the same stamp. We shall also endeavour to show in a note below, that Dr. Arnott must be content to be ranked with the foregoing worthies.(13) But, this we will say in behalf of the illustrious men with whom the doctrine of derivation originated, that it appears to us that a great oversight of physiological principles has been made in the foregoing citations. The organized machine, with all its complex apparatus, endowed with powers which are utterly unknown in the world of mere physics, and all conspiring together for harmonious results, is compared to "a household pump,"! (Vol. I. pp. 215, 290, and *notes*.) This is a great evil with which physiology struggles, and which with chemistry and the microscope renders it, in the language of Bichat, "a frivolous romance of the age." (Vol. I. p. 710.) The only connection which the circulation of the blood has with physics consists in its motion through the vessels, and the pressure of the atmospheric air. Every agent, with the exception of the last, is of a vital nature, and even the operation of that is wholly directed by vital actions.

(1) Obs. de Aere et Morb. Epidem. *Prolegom.* p. viii. — (2) Physiology — (3) On Continued Fever, in his works, p. 176. — (4) De Reditu Sang. mechanico Per Vices. 1704. — (5) Ut cit. — (6) Ut cit. — (7) Physiol. — (8) Study of Medicine vol. ii. p. 26. — (9) On the Production of Sensation by Motions, *tr.* — (10) On Diseases of the Chest, &c. p. 564. — (11) On Diseases of the Heart, p. 42. — (12) Elem. Physiol. vol. i. p. 220.

(13) Let us see what Dr. Arnott thinks of the folly of this hypothesis when he has a favourite one to support. "The head," he says, "may be considered as an air-tight vessel or cavity of bone, containing chiefly the brain and blood, and having openings which admit blood-vessels leading to and from the heart. The *atmospheric pressure*, therefore, always keeps the head *full*, as it keeps the top of a *syphon* full," "how-much-soever the quantity of blood may vary in the body generally." The *syphon* is always in operation. The principle assumed in this case is the same as that which Dr. A. condemns in the other. But, we hear nothing of the "eel-skin," or collapse of the vessels deseending from the head. The syphon is as good in one case as in the other; and the heart is as much the "top of a syphon" in one case as the head is in the other, and atmospheric pressure is equally assumed as the agent in the latter. The cavities of the heart are always as full as the cranium; and did the ventricles of the brain dilate and contract like those of the heart, or "the substance of the brain sensibly change in bulk," Dr. Arnott would then contend that the blood would rush up the jugular veins to supply the vacuum.

It is from no disposition to act the critic that we make these comments, or others of a similar import. We have utterly different objects in view; and what may seem to refer to a specific point is generally intended to have a more extensive application.

In the foregoing statement by Dr. Philip several important circumstances are to be observed. The action of the capillary arteries in forcing the blood into the veins, which Dr. Philip considers the cause of venous circulation, and that of the capillary veins, are here overlooked. This, therefore, is a circumstance which destroys the parallel between the mechanical experiment and the principle which is supposed to mainly determine venous circulation. No sooner does the venous blood begin to ascend, than its place is supplied by an injection from the capillary arteries. Here then, is an *injecting* as well as "exhausting syringe." It has been objected to Carson's, or rather Hunter's, idea of an associated "forcing pump," that the admission of its existence supersedes the necessity of an aspiratory action of the heart. But this is merely an assumption, whilst it is surrounded by obvious difficulties. The injecting force, however, being considered adequate to the venous circulation by those who object to the principle of derivation, the whole objection which is deduced from the operation of a "household pump," necessarily falls. The tendency of the centripetal force of the heart to produce a collapse of the venous parietes will be accurately counterbalanced by the injecting power; and thus the vessels will be maintained in that state of equilibrium which enables their fibrous coat to perform its important functions. (1) It is with the veins as with the arteries; at each dilatation "of the ventricle (cavities) the blood experiences suddenly a general motion that is felt at the extremities." "It is like the blow on the timber, that is felt at one end at the same time that it is received at the opposite." There is no progressive motion, no "wave," in either system of vessels. (2)

Now, all that we have just said is exactly demonstrated to be true by Dr. Philip himself, by an experiment which was intend-

(1) Dr. Carson, in replying to this objection of Dr. Philip, remarks, that "if the water is to be raised through the same medium, that is, through water, then a yielding vessel is sufficient; for the pressure on the internal surface of the tube is equal to that upon its external surface. The pumping of the water through the tube could not, in this case, cause a collapse in the sides of the vessels, if these vessels were once dilated by the fluid in which they are immersed." He then supposes that the "animal fabric may, so far as regards the point in discussion, be considered a vessel filled with blood," in which the veins are immersed, &c. (*a*) But this explanation is defeated by the fact, that when a vein is emptied of its blood by pressure along the arm, as soon as the pressure is removed the vein fills again.

(2) Bichat's Gen. Anat. vol. i. pp. 345, 346, 432.

(*a*) Edin. Med. and Surg. Journ. 1824, p. 267.

ed to show that venous circulation depends upon the vis a tergo of the arterial capillaries. Thus, —

"In the newly dead rabbit, in which the circulation was maintained by artificial respiration, the jugular vein was laid bare for about an inch and a half; a ligature was then placed behind the part of the vessel nearest to the head, and the animal was so placed that the vein was brought into the perpendicular position, the head of the animal being undermost, so that it was necessary for the vein, in carrying the blood to the heart, to convey it perpendicularly against its gravity. The ligature was suddenly tightened, whilst Mr. Cutler and myself observed the vessel. The blood in the part of the vein between the ligature and the heart was *instantly and completely expelled;* the vessel itself wholly collapsed," (because no longer supplied, and from the violence inflicted.) "In the mean time, on the other side of the ligature, the vein had become gorged with blood." (1)

It will be here seen that the blood was "instantly and completely expelled" from the vein *against the force of gravity*, whilst in a corresponding experiment upon an artery, where the blood was moving with greater velocity, it was only partially and more slowly expelled after tying the ligature. (2)

As to that portion of the vein outside the ligature becoming "gorged with blood," this should happen upon the theory of derivation; since the suction power of the heart should divert the blood into that portion of the vein, (the tendency to a vacuum always existing,) through the inosculating ones. The vessel, however, may have been filled by the projecting power, which may be supposed equal to the elevation of so short a column, without proving that it could raise it six inches higher. The experiment, therefore, establishes nothing but the suction power of the heart, which Dr. Philip was aiming to disprove.

The accumulation of blood below a ligature upon veins has been especially presented as an objection to the doctrine of derivation by Dr. Hastings. The blood will also flow in a continued stream if the inferior portion of the vein be punctured. But this should certainly follow upon our own premises, or it would prove them false. The vacuum, or its tendency, which had existed in the vein, still remains, and the blood will continue to rise as in the syphon, and to make its escape from the orifice, even without the supposed agency of the heart, through the inosculating veins. The resistance of the atmospheric pressure at the orifice is overcome by the injecting power of the capillary

(1) Inquiry into the Nature of Sleep and Death, p. 73; and Philos. Trans. 1831.
(2) Ibid.

arteries. A slight amount of this power, far less than is admitted to exist, is sufficient to overcome the equilibrium of atmospheric pressure. It is also evident, from the constant *conatus* of the veins to contract on their contents, (coinciding, however, with the action of dilatation which is instituted by the sympathetic influence that is propagated open them by the capillary arteries in their increased determinations of blood, p. 397,) and since the tendency to contraction is not readily counteracted but by the operation of the vital cause, the influence of the ligature will not be felt by the excluded and anastomosing vessels till a certain distension has taken place in the vein whose circulation is obstructed. It will be then only that the unobstructed vessels will permit a greater volume of blood to pass; and whilst, therefore, from this as well as the foregoing causes, an orifice exists in the lower portion of the obstructed vein, no redundancy of blood will pass the unobstructed. Nevertheless, the discharge of blood from the orifice is slight and has but little force, whilst this is a conclusive proof that there is but a slight momentum imparted by the vis a tergo. For this reason, as is verified by experiment, the injury of the vein ultimately deranges its relations to the circulation, and the blood disappears from the inferior portion, whilst the rapidity of its disappearance depends on the number and size of the anastomosing branches. Where all the veins are obstructed, the momentum which attends the stream is greater from the concentration of a greater force of the capillary arteries.(1) When a single vein is obstructed, and no orifice is made, it follows from the injury inflicted, as well as from what we have said of the inosculating vessels, that their tendency to contraction will be gradually overcome by the stimulus of the increased determination of blood, and that the tumour below the ligature will subside.

(1) Thus, also, we answer the only two objections of the Edinburgh reviewer, (*a*) (which were ma le to what is called Dr. Barry's hypothesis, but which apply equally to the theory which we advocate,) that when a vein is divided, the blood continues to flow from the lower orifice till syncope or death takes place; and, secondly, that in venesection the blood escapes from the vein below the ligature. The suction power is exerted through various communicating vessels; whilst, also, nothing is more probable than that the action of the capillary arteries, would be adequate to maintain, for a certain time, a circulation in the lower portion of veins. The question is not whether the injecting power can maintain a current of blood through a

(*a*) Edinburgh Journ. of Med. Science, vol. ii. p. 463.

Nor are the veins in their natural state liable to the "collapse" which is supposed by Dr. Philip, nor is the simile of "a piece

short extent of the venous system; but, whether it can carry it on to the heart, and with the same velocity.

Here is another experiment similar to the foregoing, and intended for the same purpose, but equally confirming the doctrine of derivation. It is the celebrated one by Magendie, (*a*) Poiseuille, (*a*) Cross, (*b*) and others, who tied a ligature round the thigh of a dog, excepting the crural artery. A ligature was also thrown round a vein. This vein was opened, and the blood flowed out in a stream. The artery was now compressed, and the blood gradually ceased flowing from the vein, which, however, remained full through its whole length. When the pressure was removed from the artery, the blood again flowed from the vein. But whenever the artery was compressed, the blood only gradually ceased flowing from the vein, whilst the vessel ultimately remained *full* and the artery *empty*. Here, then, it is evident that the vein was filled by some other power than that of the heart. That power was the action of the capillaries; and, unless the ligature around the limb was tighter than is probable, the suction power of the heart contributed to the result. For the reason, also, mentioned in our comment in the text, the blood should have started from the vein whenever the ligature upon the artery was loosened; since the tendency to a vacuum being established in the vein, the renewed supply of blood would be moved along by any cause that should disturb the equilibrium of atmospheric pressure at the orifice and along the vein. A slight cause operating at the base of the venous column would be sufficient for this purpose.

It would seem, however, that little dependence can be placed upon an experiment where so much violence is inflicted. (Vol. i. p. 507—514.) Nevertheless, it has crept into many of our best works on physiology, and appears in Morgan's late treatise on surgery. (*c*) It is the only proof of any importance, upon which Müller rests his affirmation that, "it is *irrefragibly* proved that the motion of the blood through the capillaries is effected *solely by the action of the heart*." (*d*) And yet Müller says of another and less objectionable experiment by Magendie, that "the circumstance of an animal not being susceptible of other impressions immediately after the division of so large a nerve as the fifth, proves nothing more than that it had suffered a *serious injury*." (*e*)

We are led, also, to consider another parallel difficulty. "If a turgid vein in the hand is compressed," says Dr. Elliotson, "it will not become empty above, as it should if suction from one or all of the three sources mentioned were considerable." (*f*) Perhaps it is sufficiently shown by Dr. Philips' experiment (p. 408,) that there are some unexplained causes operating here, which may denote the importance of attending to all modifying causes. It is probable, in the first place, from the entire disappearance of the blood from the same part of the vein in Philip's experiment, that, in the case of the hand, the *temporary* detention is owing in a measure to the free inosculation of vessels. Again, in all the cases, we might look for a near equilibrium, and, consequently, a partial stagnation of blood in the superior portion of a compressed vessel, excepting as it may be moved forward by the contractile power of the vein. The blood in all the other veins is partially carried forward by the ingress from the capillary arteries; and the momentum thus imparted, however small, by cOoperating with the suction power of the heart, may throw the whole in-

(*a*) Magendie's Journ. de Physiol. t. ix. (*b*) American Med. Recorder, vol. ix.
(*c*) Principles of Surgery, p. 17. (*d*) Elements of Physiology, vol. i. p. 220. (*e*) Ibid. p. 770.
(*f*) Human Physiology, part 1, p. 211.

of cat-gut or eel-skin," which is made by Dr. Arnott, in the least admissible, however it may be appropriate in a work on "Physicks." (1) We have already seen, (pp. 393, 396,) that whilst the vital relations of the veins to the arteries exist, there is no tendency to a collapse any more than in the heart itself. Divide a vein, and these relations will be lost, and with it the power of active dilatation and contraction." (2) Beclard remarks that

"Their vital contractility is greater than that of the arteries. This has been proved by experiments. The portion of a vein between the ligatures, when punctured, empties itself entirely and rapidly in a living animal, whilst this does not occur after death;" and what is particularly important to us, "the vein will empty itself above the ligature." (3)

This, indeed, is fully shown, as it respects the tonic contraction of the vein, in the experiment just quoted from Dr. Philip. Indeed, this very doctrine is taught by Dr. Philip himself. What, then, becomes of the "collapse of the veins by their own weight," as supposed by Dr. Philip, or of the "eel skin" of Dr.

fluence of this power upon the contents of these vessels, so long as they continue to furnish an ample supply to the heart. Should the supply, however, from this source become deficient, or inadequate to the demand of the vacuum, the blood would then be started in the compressed vessel. But, so long as the demand is answered by the blood which has a certain momentum from behind, the blood may not be moved, or but very slowly in the compressed vein, since a greater suction power would be necessary than to move the blood which receives, from another source, a momentum cooperating with the derivative power of the heart. Now, let us look at the experiment by Dr. Philip. In that instance, the jugular vein being tied, a large supply of blood to the heart was suddenly arrested, and the blood in the portion of the vein nearest that organ was "instantly and completely expelled." But, in the case of the hand, the abstraction of blood is very slight, or none at all. We must consider Dr. P.'s experiment conclusive of the accuracy of our philosophy, whilst it shows us our great liability to misconstruction where the complex animal machine is the subject of observation.

(1) Nor is this all, in the way of "*physick*," from our author, who is so much disposed to condemn the philosophy which is founded on the phenomena of life. He proceeds with the climax: "To say that the influence of the heart or chest is the power which draws the blood to the heart from the general system, is *exactly* as if we asserted that the *rising and falling of the tide at the mouth of a river is the power which collects the tributary streams in the interior of the country.*" (*a*) Perhaps it was in reference to this that Dr. Carson, at a meeting of the Liverpool Medical Society, 1834, remarked that he considered that "his theory of venous motion was as firmly established as the theory of the *tides*." (*b*)

(2) In the "cat-gut, or eel-skin," the vital properties of dilating and contracting are not only extinguished by death, but they never possessed the same modifications that appertain to the veins. Elasticity, also, is essentially influenced by the forces of life.

(3) General Anatomy, p. 300. — And so Mr. Hunter, *ut cit.*

(*a*) Ut cit. p. 451. (*b*) Med. Chir. Rev. Lon. July, 1834, p. 176.

Arnott? If we bleed an animal, *ad mortem*, do we not find, at the moment of death, the veins perfectly contracted on their contents? As life becomes extinct, and the blood gravitates from one vein to another, those which are vacated collapse. The error lies in regarding the veins in a state when not excited by their appropriate stimulus, and by their sympathetic relations to the arteries, or when divested of the powers of life. But, what shall we say of the resisting power, by which Philip, and Arnott, and even Bichat, and a thousand others, calculate that the venous coats sustain, *without dilatation*, the entire column of blood! The valves, as we shall have shown, actually contribute no assistance in the ordinary state of the circulation. By our theory the contractile power is maintained in equilibrio, by the forces at the apex and base of the cone. And here we may remark that, if the healthy vein contract so rapidly and powerfully on its contents, as we have now seen, we may safely infer that there is some great counteracting cause in venous congestion.

In their natural state, therefore, the veins possess an absolute rigidity, which, with their tendency to active dilatation, resists, in a measure, the pressure of the atmosphere. Mr. Hunter believed that the veins contract with so much force, that "they have considerable power in carrying on the circulation." (1) We conclude in the words of Huxham, —

"Siquidem totum corpus ab incumbente atmosphera comprimitur assidue, totus ideo ejusdem cruor per venas versus cor urgeatur necesse est, quum non possit, per arterias, cor petere retrorsum propter obstantes valvulas." (2)

It is not probable, however, that the whole venous column is equally moved. The trunks of the veins near the heart bear so small a proportion to the rest of the venous system, that here the motion must be accelerated. This was experimentally inferred in a ratio approximating 3 to 1, as it respects "the large and small veins," by Spallanzani, (3) and Haller, (4) We apprehend, however, that there is generally no real diminution of the

(1) On the Blood, &c. p. 185.
(2) De Aere, etc. Prolegom. p. viii.
(3) Opere, t. iv. pp. 294, 296.
(4) Element. Physiol. t. ii. p. 326.

Vander Kemp, (*a*) Brouwer, (*b*) Elliotson, (*c*) and some others, suppose the united capacity of the branches, in any part of the sanguiferous system, is not greater than that of their trunk. But, Bichat justly says that this is mere hypothesis, and condemns the mechanical course which has been pursued to determine the question. (*d*)

(*a*) De Vita, p. 51. 1782. — (*b*) Quæst. Med. Varii Argument. p. 8, 1816. — (*c*) Human Physiology, Part 1, p. 163. — (*d*) General Anat. Vol. i. p. 383.

contents of the veins, since as much blood will simultaneously enter these vessels at their origin as is abducted at their termination. By this arrangement, veins maintain an even current of blood. The left ventricle contributes, also, to the foregoing results at the moment when the right auricle dilates.

It is objected by Dr. Philip, that "the *elastic* power of the ventricle can have no operation on the veins, the auricle being interposed between them, and contracting during the diastole of the ventricle." (1) Until it can be shown that the expansive power of the auricle is insufficient, the aid of the ventricle will not be wanted. But, there can be no obstacle to the action of the ventricle upon the venous blood during the *contraction* of the auricle. On the contrary, the valve between the auricle and ventricle being now open, not only the diastole of the latter exerts a suction power, but the contraction of the auricle cöoperates with it, in the same way that would the contraction of the vena cava, or any other portion of the veins; only the more so from its greater power, and being at the termination of the veins, where, as we have seen, the current of blood is much accelerated by the narrowness of the vessels.

All this is shown by the excess of the capacity of the right ventricle over its auricle. Meckel determined by experiments, that this difference is as 37 to 23, which we find to be the mean result of seven observations. (2) We have repeated these experiments, (employing wax after the manner of Dr. Hales,) instead of quicksilver. The difference was obvious, but less than stated by Meckel, which is certainly overrated. But, according to the greater capacity of the ventricle, its dilating power must be propagated beyond the auricle; and whilst the excess of the ventricular capacity is thus employed, the auricle is simultaneously engaged in forcing onward the moving current. Wedemeyer's objection, therefore, that the systole of the auricle retards the flow of blood along the veins, must fail. (3) Again, there is prob-

(1) Ut. cit. — We know not how Dr. Billing can take it upon himself to say, that the auricle would be "unnecessary if the ventricle had an active power of dilatation," admitting, as we do, "*natura nihil agit frustra.*" (*a*)

(2) General & Patholog. Anat. vol. ii. p. 207.

(3) See Edin. Med. & Surg. Journ. vol. xxxii. p. 86, and Edin. Journ. of Med. Science, vol. ii. p. 462.

It might be worth considering how far the final cause of the greater capacity of the

(*a*) Principles of Medicine, p. 13.

ably a certain derivation exerted upon the venous blood by the contraction of both ventricles. And, as to the supposed regurgitation of blood into the vena cava, it can at most, only happen at the very moment when the auricle begins to contract, since the blood must be determined from the cava by the dilatation of the ventricle before this cavity shall have received the contents of the auricle. The final cause of the valves is so perfectly obvious, and they perform their function so completely, that even analogy warrants the conclusion that the blood is not thrown back upon the vena cava by the contraction of the auricle. But, when we consider the resistance which is made by the momentum of the blood in the vena cava, and the corresponding effect which is exerted by the dilatation of the ventricle at the moment the auricle begins to contract, it is obvious that these causes effectually determine the blood forward, and supersede the necessity of a valve at the root of the vessel, whilst its absence is a proof of an essential dependence of venous circulation on the power of derivation.

We have adverted to the derivative effect of the contraction of the ventricles, from the interest of the subject. It is a necessary consequence of any diminution of the volume of the heart that a certain vacuum should be established in the pericardium. (') There is also a farther aid contributed by the action of the vena cava, where the uniform principle of contracting and dilating appears to exist in concert with the motions of the heart.

Allowing something to the lungs, although the acts of respiration are not synchronous with those of the heart, we have a succession of causes in and about the right cavities of this organ sufficient not only to establish the theory of derivation, but to remove, in itself, the objection which has been made as to the absence of jet. These causes, indeed, are so adequate to explain the phenomenon of an uninterrupted flow of blood, that, if a more simple be not admitted, it becomes a strong evidence of their controlling the function of venous circulation.

right ventricle over the left, as shown in the elaborate treatise of M. Bizot, (*a*) may relate to the foregoing functions. This was also shown by Dr. Parsons, in his Crounean Lectures, 1744. (*b*)

(1) This will certainly follow in respect to the ventricles in a ratio more or less proportioned to the excess of their capacity over the auricles. Swammerdam, (*c*) Glisson, (*d*) Goddard, (*e*) Hunter, (*f*) Erman, and others more recently, have endeav-

(*a*) Recherches sur le Cœur, &c. in Mém. de la Soc. Méd. d'Observation, t. i. 1836.

(*b*) Philos. Trans. vol. xliii. p. 1, &c.

(*c*) Biblia Naturæ, p. 846. — (*d*) Philos. Trans. Lon. p. 356. — (*e*) Opera, t. iii. p. 191. — (*f*) On the Blood, p. 185, &c.

In the foregoing manner, also, the suction power of the left cavities of the heart is extended to the circulation of the lungs. This suggestion, as has been stated, originated with Servetus. If the doctrine be true as it respects the right cavities, it must be equally so of the left.

An able reviewer of Magendie's Lectures on the Physical Phenomena of Life is clearly wrong in saying —

"That there are two conditions of a pump which the heart lacks, and which, unfortunately for the analogy, are precisely those which constitute the essence of a pump, — namely, the play of a piston and the formation of a vacuum." (1)

Let us divest ourselves of the opinion that the animal fabric is a mere automaton, and there will be no difficulty in the case. No action, or movement, appertaining to organized matter, originates in physical forces. The vital dilatation of the cavities of the heart, (or the *elastic* if it be preferred,) is equivalent to "the play of a piston." We see not, therefore, how it should not follow that "the formation of a vacuum, and a consequent atmospheric operation," will be unavoidable results.

The only remaining objection to the theory of derivation, of which we are aware, is that relating to the experiment of tying the vena cava, and then bleeding from the distal portion. This observation being exactly analogous to a foregoing one, has been sufficiently answered. The tendency to a vacuum remaining in the vessel after the application of a ligature, the ingress of blood from the capillary arteries is sufficient to overcome a considerable mechanical obstruction; though we ask not for a tenth part of what is assumed by the opponents of this theory. The principle would remain sufficiently long to occasion fatal hemorrhage, as is known by the continued action of the capillary arteries after apparent death has taken place.(2) But, were there not a rapid abstraction of blood in the foregoing experiment, it is manifest that all the vital energies would be soon paralyzed, as would equally follow if the suction power of the heart were merely suspended. This function, therefore, is indispensable not only to the primary, but to a prolonged elevation of the column of venous blood.

oured to show by experiments, that when the heart contracts, its volume is diminished. This, however, can only be in a certain proportion to the excess of the ventricles in their capacity, over the auricles.

(1) British and Foreign Med. Rev. Oct. 1839, p. 328.

(2) See Philip on the Nature of Sleep and Death, p. 71. "The motion of the blood," he says, "in the capillaries continues long after the heart has ceased to beat and the animal in the commom acceptation of the term is dead." And so others. — See our Essay on *Inflammation.*

We shall now state in a note below, "ut nihil prætermissum quod a quovis dici possit," a particular fact which may appear to some to present a greater objection to the doctrine of derivation than any of the preceding. (1)

(1) Dr. Davis relates a case (*a*) in which there was a remarkable "pulsation in all the veins, distinct and well marked, synchronous with that of the arteries, and in the veins of the extremities, perceptible to the eye, even at the distance of two yards." The seeming difficulty consists in the statement that "pressure upon any of them stopped the pulsation between the part compressed and the heart; whilst the pulsation in the part of the vein towards the extremity was rendered much stronger, provided the return of blood to the auricle was not completely obstructed;" when the pulsation would cease altogether, as it would, also, when the circulation was stopped in the arteries. The pulse was irregular, beat but 88 strokes in a minute, and was only "pretty strong."

We think that this case, so far from being an obstacle in our way, may be made to subserve our various purposes. Much of what we have already said is applicable in the present instance. The universal pulsation of the veins was perfectly unique, and therefore depended not alone on the action of the heart, but in part also upon an unusual cause. The unusual cause we suppose to exist in some morbid condition of the veins of the skin. This we infer especially from "an eruption of *dark spots*, larger than petechiæ, which were *scattered over the skin*," (sec. 16;) from "the veins being larger than is usual at her period of life;" from the irregularity of the pulse; and, from other phenomena attending the case; as well, also, from there having been no other morbid appearance after death. The veins were in an irritable state, and contained an unusual proportion of blood. At each diastole, therefore, the suction power of the heart made an unusual impression upon the whole venous system; and, it is not difficult to suppose, also, that a vibratory motion would be thus produced in a subject emaciated as this patient was. Pulsation ceased "between the part compressed and the heart," for reasons which we stated in our former note; where, also, we explained the cause of the pulsation in the inferior portion. If the pressure was perfect, then the accumulation of blood was too great to be sufficiently influenced through the inosculating branches to render the pulsation sensible. Had it depended on the vis a tergo, it would have been still distinct, as when an artery is compressed. If "the pulsation was stopped on compressing the arteries," it was because the veins were not then supplied with blood. The pulsation was synchronous with that of the arteries, because the auricles dilate when the ventricles contract.

Instances analogous to the foregoing are recorded by Laennec, Weilbrecht, (*b*) and in the Journ. Complimentaire. (*c*) Dr. Wm. Rush observed the venous pulsation upon the hands of a child who had been long sick of bilious fever, and was "remarkably emaciated." It disappeared as the child recovered. (*d*)

Haller assigns many causes for what he denominates "vitiosus venarum pulsus." (*e*) There are other cases that show more distinctly that the motion is not communicated by the vis a tergo. In two cases by Lancisi, where post mortem examinations were made, a very different cause was assigned. He calls the beating of the veins in one "undulatio," and "fluctuatio" in the other. (*f*) Homberg, (*g*) and Morand, (*h*) dissected the subjects of two other cases; in one of which "the

(*a*) Dublin Hospital Reports, vol. iv. p. 272. — (*b*) Haller's Disputat. t. v. p. 407. — (*c*) t. xxi, 1735. — (*d*) Phila. Journ. of Med. and Phys. Sciences, vol. xiv. p. 182, 1827. — (*e*) Element. Physiol. t. ii. l. 6, s. 5, § 4. — (*f*) De Motu Cordis, — (*g*) Hist. de l'Acad. Roy. des Sciences, 1704, p. 228. *Amsterdam ed.* — (*h*) Ibid. 1732, p. 592.

Having thus endeavoured to remove the objections that have been raised against the imputed influence of the right cavities of the heart upon venous circulation, we may now say that our doctrine is founded upon immutable laws. "Nature abhors a vacuum." The cavities of the heart dilate, no matter whether by an elastic or a vital power. A vacuum must be thus established if nothing enter the cavities. But, agreeably to a universal law, the blood which is nearest the cavities, must progressively enter as the vacuum forms. The quantity thus diverted into the cavities will be exactly equal to the space which is generated, and this corresponds with what is sent out by the right ventricle. For the same reason there must be a simultaneous motion through the whole column of venous blood, corresponding with that at the apex; whilst the space must be immediately supplied at the base from the capillary arteries. The heart, urging the blood into these capillaries, it is obvious that we do not require even the propelling power of the capillaries to render the doctrine of derivation perfectly philosophical.

We shall not stop to inquire into the validity of Dr. Barry's, or rather Huxham's, theory, which also respects atmospheric pressure, its fallacy having been sufficiently shown by the Edinburgh reviewer, (1) and by others. But, admitting the principle to be true, it would equally promote our object as it respects venous congestions.

frequency of the beatings was different in the veins from that of the arteries;" and in the other "the pulsating vein had sometimes a movement redoubled one upon the other." These observers also assign different causes from the vis a tergo for the phenomenon.

It may be worth recollecting, in connection with our general subject, that, "it very rarely happens that general death commences by that of the venous auricle and ventricle. On the contrary, they are almost always the last in action, and when they cease to act, the brain, the lungs, and the red-blooded heart have already ceased to exhibit their respective phenomena." (*a*) It is therefore more from this mechanical cause relating to the heart, than from death beginning in the lungs, as supposed by Bichat, (*b*) that we so often find an accumulation of venous blood in the lungs; and the extent of it will then depend upon the slowness with which death may take place.

(1) See Edin. Journ. of Med. Science, vol. ii. p. 462. Also, Dr. Bailey in Lon. Med. Repos. and Rev. 1826, p. 519. — And how is it with the cold-blooded animals, where there is no movement of the thorax? Is there any harmony between the acts of respiration and those of the heart? Is the circulation much influenced by voluntary suspension of breathing?

Does the fœtus breathe; or, is there a peculiar law for venous circulation during

(*a*) Bichat's Researches on Life and Death, part ii. s. 2, c. 6. — Haller's Element. Physiol. t. i. l. 4, s. 4, § 24, and in Comm. Gott. t. i. — Nysten, Recherches de Physiol. &c. — (*b*) Ut supra.

Finally, some direct experiments appear to establish the special dependence of venous circulation upon the suction influence of the heart. Müller, who ascribes the whole capillary circulation to the propelling power of the heart, admits that the experi-

uterine life? Dr. Elliotson remarks that, "if the pericardium is laid bare, so that no vacuum can occur, except that from the dilatation of the heart's cavities, and the trachea tied, the right ventricle swells enormously with the arriving blood; a fact not to be explained by vacuum." (*a*) Here the force of the heart is increased by mechanical irritation as well as by that of the atmosphere; so that, the dilating power would lead, in an unusual degree, "to swell the right ventricle" with blood. The experiment appears to be conclusive.

Huxham is the true author of the hypothesis of derivation by respiration, who embraces, in detail, all the elements of the doctrine. (*b*) Haller (*c*) also adopted this hypothesis in part, but without mentioning Huxham's work, which was published in 1739, being eighteen years before Haller published his first volume of El. Physiol. and twenty-six years after the completion of the last volume. It is remarkable that Laennec defended the doctrine in extenso. Thinking it original with Dr. Barry, he considers it "the most remarkable addition that has been made to that of his illustrious countryman, Harvey." (*d*)

As to the adequacy of Huxham's hypothesis to an explanation of the movements of the brain, "and the reflux of blood into the jugular veins from coughing or a prolonged expiration," as admitted by the French Commission, (*e*) Laennec, (*f*) and others, we may say that the former are synchronous with the pulse, and in no respect with the acts of respiration; and, that the latter plainly depends upon compression of the superficial veins by the respiratory muscles of the neck. Even Hunter fell into this error respecting the reflux of blood, (*g*) and Haller made numerous experiments to show that the motion of the brain is produced as supposed by Dr. Barry. And thus Dr. Wardrop: "The ingress of blood into the lungs is so obstructed by the act of expelling air from the lungs as to cause a state of venous turgescence of the brain which elevates that organ." (*h*) We have already shown the fallacy of this opinion, (p. 248—255.) Venous turgescence, too, should be universal at every expiration; and whenever breathing is voluntarily suspended, the motions of the brain should, at least, be greatly diminished, which is not the case; and, what vicissitudes should occur, since, according to Goodwin the difference between "an ordinary and full inspiration is as 14 to 200 cubic inches." (*i*) The motions vary, however, according to the impulse of arterial blood, which may be affected by respiration; (*j*) and thence the foundation of the opinion that the brain has two motions. But, what is especially conclusive, is the existence of the cerebral pulsation in the fœtus.

Bichat states this matter truly and plainly, however he may err as to the final cause. "At each systole of the heart, the brain is lifted, just in the same way as we see a tumour lifted by the arteries which creep along the bones beneath it." "Tie the vertebral and carotid arteries, and the movement of the brain will be instantly

(*a*) Human Physiology, part 1, p. 211. — (*b*) De Aere et Morb. Epidem. *Prolegom.* p. 8. — (*c*) Element. Physiol. t. ii. l. 6, s. 4, p. 334. — (*d*) On Diseases of the Chest, &c. p. 564. — (*e*) See Dr. Barry's Experimental Researches, &c. p. 61. — (*f*) Ut cit. p. 565. — (*g*) On the Blood, &c. p. 187. — (*h*) On Diseases of the Heart, part 1, p. 73. — Also, Haller, Element. Physiol. t. iii. l. 10, s. 5, p. 171 — 180; who quotes the opinions and observations of a great number of authors upon this question; and Exp. in Opusc. Phys. t. i. p. 231. — (*i*) Connection of Life with Respiration, p. 27. — Also, Borelli, de Motu Animalium, p. 119. — Menzies, on Respiration, p. 32. — (*j*) See Hales' Hæmastatics, Exp. 1, n. 6, 7, 8, 9, et passim.

ment of Wedemeyer and Guenther "seems to prove, *beyond doubt*, the sorbent power of the heart." (1)

Will not the combined agency of the several causes upon which we have supposed venous circulation to depend, especially the absorbent power of the right cavities of the heart, explain satisfactorily the circulation in the vena portæ? "Every intelligent mind," says Bichat, "perceives that there is a great void in what has been written upon the motion of the blood in the portal veins of the liver." (2) Mr. Hunter supposed that the portal circulation must depend, in part, upon an action of the veins. (3)

It appears to us that the peculiar arrangement of the hepatic ramifications of the portal vein is intended to bring its circulation under the absorbent power of the heart. The communication is so direct between the vena portæ and the venæ hepaticæ, that injections pass freely from one to the other. (4) It was the doctrine as expressed by Müller in our last note, which was considered

interrupted." (*a*) The experiment is conclusive. Ravier (*b*) gives the same explanation as Bichat. So, also, numerous authors quoted by Haller. (*c*) "Schmucker," says Hennen, "very particularly dwells upon the appearance of pulsation in the uncovered brain, and has observed that it rose and fell more on some days than on others; and this state of pulsation regulated his employment of the lancet, — the pulsation being indicative of the increased action not only of the more superficial arteries, but also of those at the base of the brain, on the action of which the alternate rising and falling of the cerebral mass depends." (*d*)

On a former occasion, (*e*) in stating some experiments which we had made upon the brain, we drew the conclusion that the quantity of blood circulating within the substance of the brain is smaller than is generally estimated. The greater proportion circulates in the membranes of the organ; and the profusion of blood is designed, in part, to secure under all circumstances of the circulation an ample supply to the vessels of the brain, which are every where minutely subdivided; and, probably in part, to communicate at the base of the brain that motion, whose existence alone implies some important final cause.

As to the "second motion" of the brain, we have satisfied ourselves that when this is perceptible it alternates with the principal, and is merely a recoil of the organ.

(1) Elements of Philosophy, vol. i. p. 220.

(2) Anat. Générale, t. ii. p. 450.

(3) On the Blood, &c. pp. 180, 185. — We have just seen that Müller considers the "sorbent power of the heart" as established; but, he remarks at another time, that "in all vertebrate animals the *vis a tergo derived from the heart* is sufficient to propel the blood through the capillaries of the liver, after it has already circulated through the capillaries of the other abdominal viscera." (*f*) This is designed as a proof of the dependence of capillary circulation upon the propelling action of the heart.

(4) See Kiernan on the Anat. and Physiol. of the Liver, in Philos. Trans. 1833.

(*a*) Researches on Life and Death, pp. 180, 182, 196. — (*b*) De l'Influence du Cœur sur le Cerveau, &c., p. 19. — (*c*) El. Phys. t. iv. l. 10, s. 5, § 41. — (*d*) Military Surgery, p. 337. — (*e*) Lon. Medico-Chir. Rev. April, 1834, p. 433. — (*f*) Elements of Physiology, vol. i. p. 234.

inadequate by Bichat, although the latter associated a powerful capillary action with that of the heart, in explaining the general circulation.

Venous congestions of the liver also reflect some light upon the portal circulation. If depending on the propelling power of the heart or capillary arteries, considering the length and double ramifications of the vena portæ, we should look to this vein, at least, for the seat of great plethora in venous congestions of the liver. But, Mr. Kiernan states that,

"Portal venous congestion is of rare occurrence; that the *hepatic* veins are always the seat of the affection in adults, and that he has only rarely found the portal veins congested in childhood." Indeed, "the blood contained in the liver after death is almost invariably found in the hepatic veins, the portal vein being usually empty;" (1) a fact which had been before noticed by Bichat. (2)

"The portal veins and the hepatic veins," says Mr. Annesley, "are the seats of hepatic congestion; but, we believe the hepatic vein is more generally congested in the greater degree." (3)

This is probably so far true, that congestion of the hepatic veins is more frequent than of the portal. But, we have shown in our seventh section, that the vena portæ and all its abdominal connections, are often the seat of congestion; and, probably there are few observers who are not aware of the fact. According, also, to Kiernan and others, one part of the hepatic or portal veins may be congested, whilst other parts are apparently normal. (4)

Recurring again to the doctrine of obstruction, it is important to observe that the portal veins would be likely to be the more common seat of congestion, were it owing to mechanical distension, or relaxation of the vessels; and surely, if obstruction to the circulation be a cause, the loaded state of the hepatic veins should impede the circulation in the vena portæ, and congestion of the latter be at all times a consequence of the former. Nor do we believe it can be imagined that an obstructing cause can exist in the larger areas of the hepatic veins just at their termination near the immediate source of venous circulation. To these considerations may be added the relative capacity of the hepatic veins, which is less than the united capacity of the vena portæ and hepatic artery; besides other mechanical causes on which Mr. Annesley founds his doctrine of congestion of the liver. (5)

(1) Op. cit. pp. 737, 743, 755. (2) General Anatomy, vol. i. p. 439.

(3) On the Diseases of India, &c. vol. i. pp. 342, 345.

(4) Van Swieten has some curious speculations upon the pathological cause of venous congestion of the liver. — *Commentaries, &c., vol. ix. s.* 918. (5) Ut Supra.

As to the placental circulation, it is only necessary to say, if it be assumed that the propelling power of the heart is adequate to the entire process, or so long as a communication exists by which the blood is transmitted to and from the mother, it cannot be objected that the suction power of the heart may exert its ordinary influence. We think, too, that a study of this phenomenon must result in the conviction that the absorbent power of the heart, as in the case of the liver, is an obvious agent.

From what we have now said, it appears that the heart possesses within itself an almost independent control over the general circulation. Had it been otherwise,

"A thousand causes might intervene, over which the organ, so limited in its influence, could have no control, to retard or divert its course; and which, by occasioning one short delay might prevent its return forever." It is, therefore, "made the centre of pressure and of gravity, and designates the stage in the circulation in which a deficiency of supply would be the last in being felt. Hence it happens that the functions of the heart are performed, and life preserved, notwithstanding long and copious discharges of blood, which, upon any other hypothesis, must have been fatal. For, according to these hypotheses, the heart, or at least the auricles are placed at the end of projection; they mark the highest advance of the tide, and would first be abandoned by the retiring fluid. They would be drained by every profuse hemorrhage, and the heart would expend its energy in fruitless efforts to circulate a fluid that came not within its reach." (1) (See Essay on *Bloodletting*, Sec. 2 and 3.)

Upon any other theory, how could what Armstrong calls "the beautiful balance between the right and left sides of the heart" be preserved? How otherwise would the circulation be restored after syncope?

"Though we are not acquainted with any data," says Carson, "from which the power of the heart can be calculated, there must exist, nevertheless, certain limits, within which it must reasonably be supposed to be confined. If we consider that the quantity of blood in circulation is nearly one-fifth of the weight of the whole body; that this great mass is spread over an immense surface; that it is therefore subjected to great resistance from friction, especially in the small vessels where each globule is to be rolled over a fixed surface, and often pressed into an oblong shape, (?) that the currents, in consequence of anastomosing branches, are perpetually flowing in opposite directions; and that attraction must powerfully prevail between the blood and small vessels; when we consider the mass moved, the motion with which it is moved, and the resistance opposed; it is impossible to imagine that this labour could have been performed by the propelling power of the ventricle;" besides the danger of rupture at the seat of the large artery, where all this immense force must have been sustained. "The two trunks of the ascending and descending cava meet

(1) Carson's Inquiry into the Causes of the Motion of the Blood, p. 204.

at the heart in such a manner as to form a straight line. The streams of blood which are conveyed by these vessels to the heart are placed at that point in direct opposition. Upon the supposition that the blood is returned to the heart by a *vis a tergo*, by the action of the coats of the veins, or of those of the concomitant arteries, or by the compression of the muscles, this position of the vessels is the most unfavourable that can be conceived for the office that is assigned to them. The momentum of the blood in one vessel would be destroyed by that of the other; or, if the current in the descending was stronger than that in the ascending cava, the blood in the weaker stream would be prevented from ever reaching the heart. The difficulties opposed to the return of the blood from the extreme vessels to the heart, already upon these hypotheses insurmountable, would be accumulated beyond calculation, by this location of the venous trunks." (1)

Other serious obstacles exist in the angular direction of the vessels, and in the diminution of their areas as they approach the heart.

"Have those," says Dr. Philip, "who maintain that the circulation is supported by the muscular power of the heart alone, made even the rudest calculation of the degree of resistance to be overcome in driving the blood through two capillary systems at such a rate, that, in a given time, the same quantity shall be delivered by the veins, which is thrown into the arteries? Have they made any estimate of the strength necessary in the different sets of vessels, and, particularly, in the larger arteries, to sustain a power capable of overcoming this resistance? Let them give what imaginable power they will, they cannot make this power greater than the coats of the vessels will bear without rupture." (2)

And yet Dr. Philip supposes that the capillary arteries "may drive the blood through two capillary systems at such a rate, that, in a given time, the same quantity shall be delivered by the veins, which is thrown into the arteries;" and that the coats of the veins shall sustain not only no rupture, but no distension from the pressure of the entire column of venous blood. Moreover, as we shall see, even the valves are not occluded by the column of blood. The projection of the blood till it reaches the

(1) Op cit. pp. 35, 179.

(2) Dr. Philip, in Lon. Med. Chir. Trans. vol. xii. p. 401; his Exp. Inquiry, &c. Exp. 67 and 68; and On the Nature of Sleep and Death, p 70.

The fallacy of imputing the entire circulation to the propelling power of the heart, and that speculation is at the foundation of the doctrine, may be farther shown by calling to the recollection of the reader the following estimates of the force of the left ventricle; which was computed by Borelli, (*a*) at 180,000 pounds; by Fracassini, (*b*) at 3000; by Bernouilli, (*c*) at 375; by Faber, (*d*) at 63; by Arnott, (*e*) at 60; by Hales, (*f*) at 51; by Jurin, (*g*) at 15; and by Keill, (*h*) at 1 pound. (P. 112.)

(*a*) De Motu Animal. prop. 73, p. 104. — (*b*) De Motu Cordis, c. 7, p. 22. — (*c*) De Vi Cordis. — (*d*) Exercit. Med. c. 5, n. 7. — (*e*) Elements of Physics, vol. i. p. 446. — (*f*) Hæmastatics, exp. 8, p. 40. — (*g*) Philosoph. Trans. n. 358 — 9; and De Phys. Math. p. 49, &c. (*h*) Anat. s. 5.

capillary arteries involves less power than its elevation from the feet to the heart; and, in some animals, the circuit is of vast extent, whilst the capillaries have no greater strength, and probably no greater power, than in parts adjacent to the heart, or in animals of very inferior size.

Bichat thought, in the existing state of knowledge, that we must suppose that venous circulation depends on the insensible contractility of the capillaries. But, he was far from being satisfied with this explanation. "Notwithstanding," he says, "all that has been written upon this question, there is an obscurity in it in which there are but few rays of light." The circulation of the liver embarrassed him especially; since any hypothesis which should fail here must be wholly abandoned. Nevertheless, he considered it

"Incontestably proved, that when the blood has arrived in the general capillary system, it is absolutely beyond the influence of the heart, and that the left ventricle has no influence in the venous system." "The whole doctrine rested, as we know, upon the great extent which they gave to the movements of the heart." (1) This doctrine is beginning to prevail. "You must have perceived from the very beginning," says Dr. Graves, "I have rejected the idea that the blood is propelled through the system by the vis a tergo alone." (2) So, Mr. Morgan, (3) and other late observers.

The experiments by Dr. Hales appear to be conclusive, (4) especially since the capillaries had lost their vital tonicity. Whytt, taking for the basis of his opinion the experiments of Hales, be-

(1) Gen. Anat. &c. vol. i. p. 342, vol. ii. p. 37.

(2) Lectures on Inflammation and the Motor Powers of the Blood, in Lon. Med. Gaz. June 30, 1838, p. 560; July, pp. 559, 605, 632.

(3) Principles of Surgery, p. 17.

(4) Hæmastatics, Exp. ix.

The observations of Wedemeyer appear to have established the continuity of the arterial and venous capillaries. Having ascertained this fact, we are more disposed to trust to the law of analogy than to the microscope in relation to the hypothesis that "the muscular coverings of the capillaries are ultimately laid aside, and that they are mere furrows in the cellular tissue, *running like brooks in the moist earth.*" Why do not these "furrows," being in contact with each other, run together, result everywhere in ecchymoses, and an immediate extinction of the circulation? Haller, Hunter, Home, Winteringham, Meyen, Wolff, Doellinger, Gruithuisen, and even Wedemeyer, found the strength of dead arteries increases, in the ratio of their size, as they recede from the heart. The iliac exceeds the aorta, at the junction of the renal arteries, in the ratio of 1897 to 1000. Schwann supposes he has discovered that the small capillary arteries have not only a coat, but circular fibres precisely like the trunks, and that they contract on the application of cold. (*a*) Müller also proves "the existence of membraneous tubes around the capillary stream. (*b*) (P. 146—156.)

(*a*) Muller's Archiv. 1836. (*b*) Elements of Physiology, vol. i. p. 217.

lieved that a globule of blood in the arterial capillaries does not possess a degree of its *original* motion equal to its own weight. We have already adverted to Poiseuille's *hæmodynamometrical* experiment as related in Magendie's Journal. (1) The conclusions from his apparatus, as applied to the circulation of the blood, are opposed to fundamental laws in hydraulics and projectiles. Thus, "if a force acts in a direction contrary to that in which a body moves, it will lose a proportional part of its motion." Such a resistance is made by the subdivision of the vessels, the angles which they form, their curvilinear direction, friction, &c. (2) It will be readily seen, too, that there is very little analogy between the elements of the experiment and such as appertain to the circulation of the blood. Hales' observations probably determine nearly the measure with which the propelling power of the heart moves the blood in the capillary arteries, which, though small, cannot be lost. Keill estimated the original momentum of the blood in the fortieth branch from the aorta at 5000 times less than in the aorta itself. (3) Dr. Arnott agrees nearly with Dr. Hales. (P. 422, *note.*) The agency of the vis a tergo is also considered unimportant by Dr. Arnott, compared with the principle in hydrostatics, by which a fluid in one tube will rise in another communicating with it till both divisions are on a level. Yea, "the pressure of the descending arterial column of blood would be sufficient to lift that in the veins not only up to the heart but considerably beyond it." (4) This doctrine, long ago advanced, and more recently defended by Dr. Billing, (5) would have much plausibility were the animal organization an inanimate structure. But, why is a different provision made? What the final cause of the great projectile power of the heart? "Nature," says Dr. Arnott, "does nothing in vain." Why does the blood instantly cease flowing from the vein when syncope takes place, — we had almost said when death itself? When we consider, also, that all the force of the heart is expended in the arterial system, can it

(1) S. 8 and 9. — The experiment was intended to show that the blood moves with its original momentum in the capillary arteries.

(2) See some interesting observations upon this subject in Lon. Philos. Trans. 1800, p. 98. — Müller thinks the resistance so great in the capillaries as to give rise to the pulse. — *Elem. Physiol. p.* 199. (Vol. i. p. 68, *note.*)

(3) Tentamina Medico-Physica, t. ii.

(4) Elements of Physics, vol. i. pp. 446, 449.

(5) First Principles of Medicine, p. 12, &c. 1837.

be allowed that the hydrostatic principle is of much, if of any, avail? Will it account, in the least, for venous circulation when the body is in a horizontal posture, in the bodies of quadrupeds, in reptiles, &c.; or, for the nearly equal force and fulness of the venous current under all circumstances of posture? Should not syncope be most effectually relieved by the erect position? Does not blood accumulate in the lower extremities by long standing? Even Spallanzani, like Haller, protested against the application of "mechanical principles, and hydraulic laws to the living system," as original sources of a single movement.

"Acknowledging, however, that these laws must exert an influence upon the circulation, it is equally evident that their influence is counterbalanced by opposite causes inherent in the organs of circulation." (1)

We may say, in conclusion, that, what especially settles the fact, in a direct manner, as to the agency of the arterial and capillary system in the circulation, is the full attainment of fœtal growth without a heart. It cannot be supposed that the arterial circulation of the fœtus is carried on by the heart of the parent where the fœtus is supplied with this organ, and, therefore, not where it is deficient.

Spallanzani threw a ligature around the aorta, and opened the trunk of the mesenteric vein,

"When the circulation was actually increased in velocity, not only in the larger veins, but also in the smallest ramifications, almost all of which emptied themselves (sproveducti di sangue.") In another like experiment, he "opened a large vein of the liver, from which the blood issued till arrested by a coagulum; upon renewing which, however, the blood began again to spout out, (è tornato a spiciare,) through the opening." And so the vena cava. (2)

Haller made the same experiments, and with the same results, which especially led him to the full adoption of the theory of derivation. (3)

We have said that the hydrostatic hypothesis must yield to the fact alone that the pressure of the venous column would produce not only dilatation, but rupture of the veins. Bichat was aware that this objection might be made to the doctrine of vis a tergo.

"As the blood enters the veins," he says, "the weight of that which is before it not being overcome, it would produce a general dilatation, and the blood

(1) Opere, t. iv. de'Fenomeni della Circolazione, Esper. 106, p. 228.
(2) De'Fenomeni della Circolazione, Diss. 3 *za.* s. 3, Esper. 51 to 57.
(3) Elem. Physiologiæ, t. ii. l. 6, s. 1, § 40; s. 4, § 4 and 7.

would not reach the heart. But the valves counteract this, by supporting at short distances the column of blood." (1)

Tiedemann, though adopting the theory of derivation, observes, that

"The blood advances from the twigs towards the branches, during which progress the valves prevent its retrograde movement, and support the column of fluid thus divided into numerous sections."(2)

And so, Hunter, (3) Harvey (4) Haller, (5) Elliotson, (6) &c. But such cannot be the final cause of the valves. For, whilst the current of blood is moving in the veins with its usual velocity, it must be at every instant passing the points of intersection. The valves, therefore, must be constantly open, and the moment the column should press upon them, they must close, and the current become interrupted. It is with them as with the valves of the heart, whilst the current is passing. And since, therefore, the valves are clearly not occluded in the ordinary circulation, it follows that the pressure or gravity must be counteracted by a derivative source at the apex of the column. The foregoing doctrine is also contradicted by the absence of valves in the veins of the abdominal viscera, and the remarkable exemption of the vena portæ from accumulations of blood. (7) We may also say that the absence of pressure is fully shown by the readiness with which even the smallest veins of the feet contract from the slightest influences.

And here we may add, that if morbid dilatations of such veins as are provided with valves depend on stagnations of blood, then should those vessels appear knotted and tortuous; which, in their early stages at least, is contrary to fact. The knots of varicose veins, as we have shown, depend on another cause.

(1) Gen. Anat. vol. i. p. 421.
(2) Comparative Physiology, vol. i. p. 149.
(3) On the Blood, p. 185. (4) De Motu Cordis, c. 13.
(5) El. Physiol. t. i. p. 203—217. (6) Physiol. p. 191.
(7) The final cause of the valves is to prevent a reflux of blood where pressure, to which the veins are so much exposed from muscular action, &c. may operate upon them; and to determine the current, at the same time, into the anastomosing branches, which are equally provided for this contingency. Thence, as there is nothing superfluous in the animal economy, we find the valves wanting in nearly all the veins which are not exposed to pressure; whilst, on the contrary, where they are most exposed, there the valves abound most.

SECTION XII.

ANALOGIES, ETC. RESUMED.

(See Sec. 9.)

"Modi morborum hi sunt, partim quidem cognati sunt, de quibus interrogando discere oportet, partim a regione, familiares enim sunt per multas, et multi ipsos noscunt; partim ex corpore, et victus ratione, ac temperiei constitutione, aut a temporibus. At regiones ad tempora male disposita tales morbos pariunt, quali tempori similes fuerint, verbi gratia inæqualis calor, aut frigus eadem die, quum talia fuerint, morbos autumnales facient. Et in aliis temporibus secundum rationem, alii ab odoribus cœnosis, aut palustribus; alii ab aquis calculosi, splenici; alii a ventis bonis, et malis incipiunt. Qualia vero sunt tempora, tales erunt etiam morbi, et constitutiones ex ipsis. Si tempora tempestive, et ordinate se habuerint, morbos judicatu faciles faciunt. Vernaculi autem temporum morbi indicant mutationes, et prout evariaverit tempus, similes, aut dissimiles erunt morbi qui in hoc tempore oriuntur. *Si vero similiter procedat, ejusmodi quoque fuerunt morbi, et ad talem modum tracti*, veluti est morbus regius autumnalis. Frigora enim ex caloribus, et calor ex frigora." (1) HIPPOCRATES, *L. de Humoribus*, *v.* 123 — 136.

"Naturarum aliæ quidem ad æstatem, aliæ vero ad hyemem bene, aut male se habent. Morborum alii ad alia tempora bene, aut male se habent; et ætates quædam ad tempora, et regiones, et victus." "In constantibus temporibus, si tempestive tempestiva reddantur, morbi constantes, et judicatu facillimi fiunt. In inconstantibus autem, inconstantes, et qui difficulter judicantur." — *Ibid. Aph. s.* 3, *v.* 3 — 16.

"So long as Dr Otto withholds the proofs of his assertion, we shall continue to believe, that the laws discovered to regulate the relations of those diseases, in *other* climates, *hold good in the capital of Denmark.*" (2) BRITISH AND FOREIGN MEDICAL REVIEW, *January*, 1840, *p.* 218.

HAVING endeavoured to show in our last section, (as well for the purpose of farther illustrating the laws of the animal system, as for our immediate object,) that venous circulation is mainly determined by the vital actions of the right cavities of the heart and the coincident pressure of the atmosphere, we reach the conclusion that accumulations of blood cannot take place in the veins without the interposition of some mechanical obstacle, or some morbid condition of their coats. That it does not depend on impediments to the circulation we have already shown; and that the dilatation of the veins must be of an active nature appears not only from our specific proof, but the absorbent power of the heart would prevent any distension that might otherwise arise

(1) See p. 205; and vol. i. pp. 307, 280, 471 — 474, 541 — 548. — The writings of Hippocrates are everywhere imbued with the doctrines which stand at the head of this section.

(2) See our vol. i. pp. 278 — 281, 285 — 293, 299 — 309, 317 — 325, 322 — 335, 626, *note*.

from any supposed relaxation of the veins. This principle is also more extensively stated with a very different view by Dr. Carson, and is therefore more important to our conclusions.

"So long," he says, "as there is an open communication between the blood, in any part of a vein and the heart, however circuitous that communication may be, the stagnation of blood, either before or behind the obstructed part, is not to be apprehended." (1)

The truth of the foregoing induction we have endeavoured to substantiate by various facts and illustrations; and it must, therefore, equally follow that local obstruction to the circulation is no more a cause of venous congestion than passive relaxation. As to the latter condition affecting suddenly and universally the entire venous system of an organ, and that organ alone, even had we any analogies to show that the power of vital affinity declines without a diseased and even inflamed state of a tissue, the hypothesis appears to be in the highest degree improbable; whilst, if we admit a suction power of the heart equal to the hydrostatic pressure of the venous column, as is manifested in health, we see not how any moderate relaxation of the membranous veins should favour an accumulation of blood. Indeed, the very idea of relaxation presupposes the existence of inflammatory action as its cause—at least, if there be any dependence on a wide range of the strongest analogies, and any distinction between the laws of vitality and those of physics.

In our argument, (Sec. 4,) founded upon the operation of bloodletting, we endeavoured to show the inconsistency of denying the induction that its operation is the same in venous congestion as in ordinary inflammation, and that its results conclusively establish the latter condition of disease as the pathological cause of the former. We may now say, that if congestion depend upon relaxation of the veins, or, as supposed by many, upon diminished power in the heart and arteries, or upon other passive accumulations of blood, the abstraction of blood, where the circulation is excited, should increase the evil by lessening either the suction power of the heart, or the force of the vis a tergo, whether in its relation to the heart or the capillary arteries; and, in those cases where there exists a great prostration of the circulatory powers, and the blood is accumulated about the centre, and bloodletting raises the powers of circulation, there is actually determined upon the congested veins of any organ remote from

(1) Inquiry, &c. p. 164.

the heart, as the brain for instance, a greater volume of blood, whilst the enlargement of the veins is simultaneously reduced. No tone has been directly imparted to the circulatory organs by the loss of blood. Their renovated action has been the simple consequence of the universal impression on the capillary blood-vessels, (see *Bloodletting*,) and the consequent removal of an oppressive load from the heart. But, whilst a general enlargement of the veins has been thus effected, the congested veins have, at the same time, undergone a diminution of their volume. In common phraseology, they have been "relaxed" where they were contracted, and contracted where they were "relaxed;" or, to pursue the rationale, the loss of blood has diminished the tone of the contracted vessels, but increased that of the debilitated. But, let it be observed, (lest it be surmised that the congested veins are reduced by the increased force of the circulation,) that in other cases where the general circulation is thus rëestablished, or even preternaturally exalted, the local congestions as stated by Armstrong, and as universally known, may remain unsubdued, whilst in other instances where the circulation was excited antecedently to the loss of blood, the congestions are subdued by bloodletting in proportion to the diminished action of the heart and arteries. We have already seen, that Dr. Armstrong perceived the difficulty of explaining, in many cases, the effects of bloodletting in venous congestion, on the common principles of its operation, and on the ground of the imputed causes of the disease. In some of these instances he concludes that "early depletion is to be considered as a *stimulus*, since it diminishes or removes those congestions which oppress the vital functions, and thereby it tends to produce an uniform and general excitement." But, this is clearly an assumption to suit the hypothesis. He, however, immediately after lays down a rule, "that *stimuli*, without depletion, are generally dangerous, and their exhibition requires great care." (1) It is true that stimulants will sustain for a time the sinking powers of life, increase arterial action, &c. But do they, like the loss of blood, in any degree overcome the morbid state of the congested veins? Do they not, on the contrary, aggravate the congestion if incautiously exhibited? (Vol. I. p. 222, &c.) Are not our practice and its results opposed to the conclusion that bloodletting and stimulants operate

(1) On Typhus Fever, p. 119.

in venous congestion upon a common principle? May not the question be determined by their ordinary effects in inflammation? Is it not an illusion to suppose that bloodletting has acted as a stimulus "because it has tended to produce a uniform and general excitement?" Even Dr. Brown, who admitted of no sedative, places bloodletting as the lowest stimulant, "since it is that which most completely carries off a stimulus." (1)

What, therefore, is called reaction in local congestions and congestive fevers, does not appear to depend upon the agency of remedies which operate as stimulants; but, upon that influence of an opposite nature which overcomes a certain pathological state that stimulants rarely fail to aggravate. Reaction, therefore, is only a consequence of the interruption of a condition very different from the embarrassed equilibrium of the circulation.

It was probably apparent to Dr. Armstrong, that his own mechanical doctrines of venous congestion would never explain the occurrence of this phenomenon in the brain, and least of all its remarkable frequency, and the vast accumulations of blood. (2) Gravity, the abundance of veins everywhere communicating with the sinuses, and the most important of the latter formed by bony parietes, probably excluded the belief that congestion could depend here either upon obstruction or relaxation of the vessels; whilst the no less fallacious doctrine of remora was clearly inapplicable in most of the cases. He therefore thought that "*the anatomist may find, in the peculiar structure of the venous apparatus of the head, the cause why this organ should so often suffer in congestive fever.*" (3) But, the more the anatomist toils at the philosophy, the more will he be convinced that it is not of a mechanical nature. (4) Besides the obvious provisions for a free return of blood from the head, the analogies which are supplied by venous congestions of other organs are conclusive that there is nothing predisposing in the structure of the "venous apparatus of the head," and that the phenomenon is everywhere owing to a common cause. (Vol. I. pp. 161, 626, 703; and *Appendix on Analogy.*) The greater liability of the

(1) Elements of Medicine, p. 183.

(2) Chisholm relates a case in which, "exclusive of what was lost in opening the cranium, fully two pounds of blood were collected."— *Essay on the Pestilential Fever of the West Indies, vol. i. p.* 185.

(3) On Congestive Typhus, p. 55.

(4) See Spallanzani, Degli Effetti della Gravita nel Sangue, Dis. 3, s. 2.

brain to venous congestions than other organs grows out of peculiar modifications of its vital endowments, and its physiological relations. (Pp. 335, 362; and *Bloodletting*, Sec. 3 and 5.) Thus, upon our theory, do we readily explain how sudden accumulations of blood may supervene in the brain upon gastric disease, as is especially witnessed in children. Here the law of sympathy is particularly involved; and, as its operation is wholly concerned with the vital powers, we may understand how the cerebral veins may become the subject of its influence on principles which relate to the laws of life.

Venous congestions are peculiarly liable to attend those fevers which are also accompanied by local inflammations; and no organ, unless the liver, is so frequently the seat of the former as the brain. It is also important to remark that the two affections are generally in distinct organs, or either shall prevail almost exclusively in different subjects of the same epidemic fever, though both may cöexist in the same organ. M. Louis thinks that no one will be likely to ascribe the cerebral symptoms attending the cases of typhus which he relates to inflammation of the brain. He considers them the result of a remote anatomical lesion which will be noticed in our essay upon the writings of this observer. Nevertheless, we presume it will be generally allowed that the cerebral phenomena depended immediately upon some functional or other lesion of the brain. Dissections exhibited little less than venous congestion. But, will a simple fulness of the cerebral veins produce all the phenomena which attended that organ, all the "spasms" and other influences which it exerted? Experiment shows us that it will not. (P. 248 — 255.) Headache occurred in all but 4 of the fatal cases, and in 55 of 57 that recovered; always continual, and one of the earliest symptoms. To this symptom succeeded delirium in three-fourths of all the fatal cases. Of the number who recovered, a few only were exempt from delirium, the mean duration of which was about ten days. There was somnolency in all the fatal cases except 5, which was accompanied by stupor in one half. It intermitted with delirium. Of 57 who recovered, somnolency appeared in all but 8. It was more remarkable in the typhoid affection, even in the mildest cases, than in any other acute disease, and was, therefore, one of its diagnostic symptoms. Spasms occurred in a large proportion of the fatal cases, and were considered another important diagnostic symptom. There

was generally, also, an injection of the tunica conjunctiva, painful affection of the eyes, frequently noise in the ears, dulness of hearing, and deafness. In 33 of 45 who recovered, hearing was affected. Such, indeed, was the violence of "the cerebral symptoms upon the manifestations of those dependent upon the severest lesions, that they masked them in nearly all the cases." "Consideranda sunt hæc, quo, et unde, et propter quid." Our author taking, as we suppose, the foregoing counsel of Hippocrates, concludes that,

"We must infer that it is in the lesion of the glands of Peyer, and *not in any other*, that we must look for the *cause* of the *delirium*, and more especially of the *somnolency*" in typhoid fever. "*All the symptoms seem to prove* that the *delirium* of the typhoid affection cannot be explained by any appreciable alteration of the brain." (1)

With many other similar remarks, calculated to show that the brain is not the source of its own morbid manifestations. True, our author partially qualifies this doctrine by remarking,

"That we have no right to say, because there is a want of connection between the cerebral symptoms and the anatomical condition of the encephalon, that, consequently, the brain had no influence upon the *issue* of the disease." But this is immediately cast away by the inquiry, "what importance is it, however, with respect to effects, for us to know what is the cause of trouble in our functions, when this trouble is of a serious nature?" (2)

Pythagoras, said an ancient philosopher, looks at the sun very differently from Anaxagoras. The former carries his eyes into it, like a god; whilst the latter looks up to it as unfeelingly as a stone.

But, we recur to the injunction of Hippocrates, "consideranda sunt hæc," &c. It is a matter of no moment to our present purpose whether the foregoing Parisian disease was typhus fever, of irresistible progress, or an intestinal inflammation of natural and artificial consecutive lesions of other organs. It was remarkably distinguished by the severity of its symptoms, and mainly so by the cerebral, which "masked the manifestations of those dependent upon the severest lesions." We have little else, however, in the brain than venous turgescence; and, since no such phenomena, not even constitutional ones, arise when the jugular veins are tied, and never unless there be venous congestion, or common inflammation, or some organic affection of the brain, the conclusion appears to be unavoidable that the cerebral results depended upon a highly morbid condition of the

(1) M. Louis' Researches on Typhoid Fever, vol. ii. pp. 5, 113, 115, 125, 127, 132, 140, 142, 144, 151, 169, 451. — (2) Ibid. p. 145.

venous tissue of that part, in all the cases where venous injection was the only appreciable lesion. And this becomes the more obvious when we pursue the inquiry analogically, and remark, that the worst conditions of disease are such as are constituted by venous congestions of other organs, especially if they be permitted to continue without mitigation.

Leaving, however, the consideration of simply local affections, for their complications with idiopathic fever, it is certain that the early manifestations of the latter depend on inscrutable changes in the vital powers and functions throughout the system; and it is certain, also, that these changes must more or less influence, and be influenced by, the local affections, whether they consist in inflammation or venous congestion. For this reason great varieties will occur in the constitutional symptoms, and in the modifications of the pathological states of the local developments, — depending greatly upon the specific nature of the predisposing causes, or, as the local affections may depend mainly upon those causes, or proceed more directly from the febrile action. Thence the fallacy of imputing to a particular lesion, as a principle, where idiopathic fever may be concerned, the development of others, or the whole array of the symptoms. There is nothing more false in pathology than the sophism "post hoc, ergo propter hoc." It is unimportant whether the Parisian disease was a constitutional fever, or constituted by local affections. If the former condition be supposed to have existed, the inductions would be equally calculated to mislead; especially considering that there were but few important organs which had not sustained most serious lesions, whilst the whole were referred to the affection of the glands of Peyer. We are now, however, most interested in showing the nature of the venous injection of the brain in typhus, and other idiopathic fevers. We are far from ascribing any other importance to the general influence of the venous disease than is warranted by the strictest rules of induction. The congestion may be greatly the consequence of a constitutional affection, as it undoubtedly is in numerous cases; but, that the development is often anterior, or simultaneous with the general lesion, is abundantly shown by the local symptoms, and it is equally evident that it performs an important part in the general dénouement.

Authors have differed greatly as to the causes which produce the peculiar modifications of typhus fever.

"If the local inflammation," says Prof. Alison, "which can be ascertained to take place during fever, is inadequate to explain the characteristic typhoid symptoms, it is equally in vain to seek for an explanation of these symptoms, as some have done, in the mere circumstances of irregular distribution of blood, and congestion of blood;" meaning venous congestion. (1)

The difficulty, however, is overcome when we consider the amount of the venous tissue involved, the importance of the organs with which it is connected, the extensive influences of undoubted venous inflammation in comparatively unimportant parts, and the fact, as stated by all observers, that phlebitis produces symptoms closely resembling those of typhus fever; from which circumstance, indeed, has arisen amongst the humoralists the opinion that injections of irritating substances into the veins produce the genuine typhus. (Vol. I. pp. 397, 515—522, &c.) a common pathological cause should be suspected in all these affections; and since typhus fever is essentially a general affection of the system, ab origine, and the others are more or less local, that cause must appertain to the venous tissue. And, if we now take the analogies in their proper bearing, we shall find that the venous congestions of typhus fever are identified with phlebitis through the artificial results which are obtained by injecting putrid matter, urine, ink, &c. into the veins; when all the acknowledged phenomena of phlebitis take place where no venous inflammation has been suspected, but where venous congestions are always induced. And here we may revert to the experience of the former observers of nature, and compare it with what has been subsequently developed as to venous inflammation, and the constitutional nature of the venous system.

"Congestion of the venous system of the abdomen," says Dr. Clarke, "has long been regarded as a fruitful source of disease. It was familiar to the pathologists and physicians of the last century, (p. 218,) and although less attended to, has not been overlooked by the moderns. Such of my readers as are conversant with the writings of the German physicians of the middle of the last century, particularly Stahl, Hoffman, and above all, Kæmpf and his disciples, will be aware of the extensive influence of this state of the abdominal circulation, and the importance attached to it at that time. Of the various phenomena presented by a person strongly predisposed to, or labouring under tuberculous disease, a congestive state of the venous circulation will, I believe, be found on close investigation, to be one of the most constant. But the influence of this pathological condition in the production of tuberculous disease is not well understood." Abernethy, Philip, Ayre, and Todd, are quoted to the same effect. (2)

(1) Outlines of Physiology and Pathology, p. 517.
(2) On Pulmonary Consumption, pp. 198, 201.

These, as well as later philosophers, suppose that the phenomena arose from the venous accumulation of blood. But from what we have hitherto said, it follows that all the phenomena depend upon an inflammatory state of the venous tissue. We believe that this conclusion cannot be refuted, with a proper reference to our premises.

Some authors have gone so far as to ascribe the constitutional symptoms of phlebitis to the absorption of pus; notwithstanding the ample proof of their existence before any pus is generated, the undeniable influence of venous inflammation upon the system, and the direct proof, and the admission of humoralists, that "laudable pus" is not morbific when circulating with the blood. (Vol. I. pp. 523.) Among the late distinguished defenders of the exploded doctrine are, Arnott, Ferguson, Roupell. (Vol. I. pp. 388, 516, 540, 686, and *notes*.) Dr. Arnott complains, repeatedly, that "although the violence and fatality of the constitutional affection have been equally recognised, authors have not shown *how* the primary local affection produces the constitutional." (1) This is something like the demand which is often made for farther proof of the existence and nature of the vital powers. The information, however, for which Dr. Arnott asks, is distinctly and fully given by the very authors whom he quotes; especially by Hunter, Travers, and Hodgkin, all of whom mainly refer the constitutional symptoms to the direct sympathetic impression of the inflamed vessels. If the principle of sympathy be denied, so must all the facts, and there would be no common ground between us. (Vol. I. pp. 134—138, 157—160, 568—575.) "How" the effect is produced no one will ever tell us, any more than as to the modus operandi of any other cause. Nor do we perceive that Dr. Arnott has been any more successful in referring the results to the purulent matter, or in explaining its mode of operation. If it add, as considered possible by Mr. Hunter, (2) to the general disorder, we have a right to conclude that it does so by extending the venous inflam-

(1) Medico-Chir. Trans. vol. 15, pp. 6, 7, 8, &c.

(2) Mr. Hunter is not always fairly represented upon this subject. "In all cases," he says, "when inflammation of veins runs high, or extends itself considerably, it is to be expected that *the whole system* will be affected. For the most part, the *same kind* of affection takes place which arises from *other* inflammations." He then adds that when *no adhesions prevent* the entrance of pus into the circulation "it *may* add to the general disorder," &c. (*a*) (Vol. I. p. 635.)

(*a*) Trans. of a Socy. for the Improvement of Med. and Chir. Knowledge, vol. i. p. 18.

mation when its properties are acrid, or by the mechanical irritation of the globules, if such there be. (P. 188; and Vol. I. pp. 523, 524, 627, 686.) Dr. Arnott sustains his conclusions mainly by the case of Arnold, who suffered inflammation of the cephalic vein, from venesection, to the extent of "about six inches." But it is important to notice that this very case contradicts the doctrine of purulent action; since "the punctured vein contained pus for about two inches below, and four inches above the wound, *where a coagulum of blood was found filling the cavity*," and thus cutting off the introduction of pus into the circulation. The venous inflammations which sprang up in other parts we believe we shall have shown to have been of a sympathetic nature. Our author, however, appears at last to recede from his position, and to take the one which we have occupied; since he concedes, that,

"The *early* appearance of the symptoms in some cases seems scarcely to correspond with the time usually required for the productions of pus; as in one which occurred to Mr. Freer, where they came on, suddenly, four hours after the ligature of the saphena." (1)

This single case is sufficient to settle the principle; whilst it shows how much may be owing to the different susceptibilities of different subjects. (Vol. I. p. 626, *note.*) Blandin affirms that, when phlebitis occurs in parts remote from wounds, as in amputations, that if the divided vessels of the soft parts be not the seat of a similar affection, it will be found to exist in the veins of the bone. (2) And so Cruveilhier.

Here, however, is another case illustrating fully the prodigious influence which venous inflammation may exert upon the powers and actions of life, and abolishing the necessity of pus to aid our physiological interpretations. It is an instance of fatal phlebitis which followed venesection.

"The cause of the intense constitutional derangement ending in death," says Dr. Ingham, "had left *no trace;* for the alteration of structure in the arm was neither of a kind, nor to an extent, to account for the consequences which had resulted from its influence upon the system." (3)

(1) Ut cit. pp. 44, 46. — In estimating the deleterious effects of inflammation upon the system, it is important to consider that its effects do not arise, as supposed in part by Mr. Hunter, from a propagation of the inflammation to the heart, but from the direct sympathetic influence of venous inflammation. This is known by the rarity of any cardiac affection.

As to pus, we do not find that the symptoms become modified after its formation, whilst in numerous fatal cases where the affection has been quite circumscribed, no pus has been detected.

(2) Revûe Médicale, Sept. 1837. (3) In Lon. Med. Gaz. vol. xv. p. 103.

This, and analogous cases help us to understand how venous congestions so frequently destroy life, without leaving another trace behind. This subject will be resumed after speaking of narcotic poisons.

Having endeavoured to illustrate the pathology of venous congestion, in our ninth section, by the coincident relations of varix, and venous hypertrophy, we shall go on in the present to consider its connections with the admitted pathology of active phlebitis.

In the first place, as we have already stated, there is generally an absence in phlebitis, of that capillary injection of the internal coat of the veins which has been so often assumed as indispensable to inflammation. There is generally the same dilatation of the vessels as in venous congestion, especially when the inflammation is *least violent*. That part of the veins which is the seat of the most intense action is frequently found in a disorganized and contracted state, though often greatly enlarged. But, it is observed by Dr. Davis, (one of the best and original expounders of the true nature of one form of phlegmasia dolens,) that,

"The superficial veins, sometimes the finest cutaneous veins, are to be seen in many cases of this affection much enlarged and clustering together into extensive *varices*, *increasing proportionably* in dimension as they ascend high on the limb, to *ramify* in *still bolder paintings on the hips and abdominal regions*." "These appearances are to be recognised in a *great majority* of cases." (1)

What analogy can be more striking? These superficial veins are, to all appearances, in the same condition as the veins in ordinary cases of congestion. And is not an extension of a low degree of inflammation from the deep seated veins the cause of the enlargement in the superficial? The whole disease, indeed, appears often to exist in its early stage in the undistinguishable form of venous congestion, with the exception of a strong predisposition to increasing inflammation growing out of peculiarities in the remote causes, and probably, also, the particular constitution of the affected veins.

Dr. Davis, after describing particularly the enlargement of the superficial veins in uterine phlebitis, "leaves the *explanation* of the fact to future opportunities of observation." He is disposed to consider it, however, as a "circuitous circulation" to

(1) Essay, in Med. Chir. Trans. Lon. vol. xii. 1823.

replace some supposed obstruction in the larger vein. But, since "he is unacquainted with a single case among a great number which have been recorded since the time of Puzos, where the establishment of the circuitous circulation has been prevented by general bleeding alone;" and since the disease is often too soon overcome to admit the supposition that any obstruction had taken place in the femoral vein, nor can any rational explanation be given of the operation of the curative means upon the hypothesis of obstruction; and, more especially, as later experience has shown that obstructions, when they occur, do not take place till the enlargement of the superficial veins has supervened; and since, also, venous congestion supplies analogies by which we may interpret the dilatations of the superficial veins in active phlebitis upon vital principles; and unless, too, the supposed obstructed vein be among the largest, the blood will be so distributed through the collateral and anastomosing vessels that the consequent dilatations can be scarcely appreciable, we must seek for the cause of the enlargement of the superficial veins in the propagation of a lower degree of inflammation from the deeper seated, whose dilatation, or subsequent contraction, depends on a higher degree. This solution of the phenomenon is shown to be correct by numerous cases. Take the seventeenth of Dr. Lee. The inflammation existed in the superficial veins in a state of great intensity; those of Dr. Davis having been affected by an inappreciable degree of it.

"The hard, lumpy cords," says Dr. Lee, "found running up on the inside of the thigh in the direction of the superficial veins, and which were exquisitely painful, proved that the saphena veins were in a state of inflammation. How this state of the saphena veins had originated, I was unable at the time to explain; and it was not, until a much later period that I had traced the inflammation of the iliac and femoral veins, in phlegmasia dolens, along the trunk of the iliac vein to the uterus." Here we have a regular continuity of inflammation from the iliac vein to the extreme branches. Again, "in some women affected with crural phlebitis, the inflammation of the femoral vein has appeared to be suddenly arrested at the part where the trunk of the saphena enters it, and the inflammation has extended along the superficial veins to the foot." "In one case, after the swelling had subsided several months, large clusters of dilated superficial veins were seen proceeding from the feet, along the leg and thigh, to the trunk; and numerous veins *as large as the finger* were observed over the abdominal parieties." (See our p. 366 — 369.)

Whatever obstacle may have existed in the larger vein in the last of the foregoing cases, it cannot be supposed that the circuitous circulation would have demanded such a contribution

from the abdominal veins, were it compatible with anatomical relations. But we have another sign in these more doubtful cases. Thus, the same writer:

In a case of crural phlebitis, after the lapse of two months, "the superficial veins of the lower part of the abdomen, and upper part of the thigh, were *enormously* enlarged; and around the ankle there were large clusters of *varicose* veins. These vessels were subject to occasional severe attacks of pain." (1)

In this instance we have coincidences which illustrate the dependence of varix, and of apparently simple dilatations of the veins, upon inflammatory action. It is important, however, to state that in most of the instances of recovery, the superficial veins return to their natural size, and under the direct influence of an antiphlogistic treatment.

Here is another, and still more striking case; one, also, which illustrates, like the foregoing, what we have endeavoured to show,—the activity of the vital powers and the predominance of sympathy, continuous and remote, in the venous system.

The case which follows exemplifies, also, the long continuance, even of active inflammation, to which the veins are liable, and how the system will resist its influence in degrees of moderate activity when the affection is confined to the vessels of "non-vital" parts. Like the foregoing, it also fully shows the dependence of venous dilatation, and varix, upon inflammatory action.

On the 15th of July, "three weeks after delivery," says Mr. Anderson, "she complained, for the first time, of an *obscure pain* in the left side, groin, and hip; but declared it to have existed for a *few days*, and *gradually* increasing. Three days after, the femoral vein was found to be exquisitely tender and painful to the touch. Next day, whole limb much swollen, &c. the skin *white*, but *every superficial* vein was visible, of a rich blue colour. These veins were *exquisitely painful* when touched even with a feather. Next day, the superficial veins on the left side of the abdomen, as high as the umbilicus, and posteriorly on the loins up to the ribs, were precisely in the same condition as those in the extremity, all equally painful to the touch. Next day, numerous other superficial veins gradually became visible, *enlarging from day to day*, and clustering together *after the manner of varicose veins*, (p. 365 — 370,) evidently loaded with *circulating* blood. In the absence of paroxysms of pain and rigor, the *pulse fell to the natural standard*." (P. 294 — 298; and Vol. I. p. 235.)

After many vicissitudes and hair-breadth escapes, this patient became convalescent, but not till she had evidently passed the ordeal of pelvic phlebitis; or, as the patient expressed it, till "the disease had extended from the leg into the bowels." On the 25th September, however, the disease attacked the other leg,

(1) Dr. Lee, Op. cit. pp. 67, 118, 145.

where the appearances of the veins became precisely similar as upon the other limb.

"On the 26th Sept. the veins, reflected from the groin up the abdomen, were felt enlarged, hard, and painful. On the following day, these veins were traced as high up as the epigastrium; the day after, the veins on the opposite side of the lower part of the chest were seen in the same condition, &c. On the following day, the veins round the whole of the chest were evidently affected as high up as the axilla. Their *enlargement* then spread into both axillæ, up *both the jugulars*, and along the facials over the margin of the lower jaw, and *down* both brachial veins to the *ends of the fingers*." "The disease gradually subsided." "From the total absence of all typhoid symptoms, it was probable that suppuration of the internal coat of the veins did not take place."

This latter conclusion belongs to the humoral pathology, and may, therefore, clearly be controverted. The "suppuration did not take place," because the inflammation was not sufficiently active, and for this reason, also, no violent constitutional sympathies were inflicted. This case, too, is interesting as showing the distinction between the vital powers of veins which appertain to the organs of animal and of organic life, since a lower degree of venous inflammation affecting any of the great organic viscera produces far greater constitutional disturbance; whilst in the elements of this consideration may be found another proof against the foregoing humoral rationale. Nor should we neglect saying, that we have in this case a striking demonstration of the indisposition of inflammation to extend from the veins of the superficial parts to the organic viscera, as shown especially by the exemption of the brain when the inflammation, in its general circuit, "extended up both the jugulars." This phenomenon is clearly owing to a difference in the vital condition of the two systems of veins, and is an evidence of that difference. (Pp. 333, 335, 362, 363, 377.)

The subsequent state of the veins in February, in the foregoing case, is also interesting as reflecting light upon the original pathology of varix and venous hypertrophy.

"Upon the body, clusters of veins may be observed, assuming a varicose state. They are very compressible, but rather painful to the touch. Both femoral veins are large and hard, and still painful to the touch. The internal saphena veins are both *very much swollen* but *soft*. The external saphenas are in the same state." (1)

An inquiry naturally arises as to the cause of the enlargement over so vast an extent of the venous system. If it be neither obstruction to the circulation, nor passive relaxation, but

(1) London Med. Gaz. vol. xv. p. 794.

inflammation itself, it supplies a full ground for induction when the same phenomenon presents itself in venous congestions. And, whilst thus adverting again to the doctrine of obstruction, we may adduce the condition of the vena cava in the first well authenticated case of uterine phlebitis, by Meckel, and others like it.

"All the veins in the vicinity of the uterus, and the vena cava inferior, were *greatly enlarged.* All the uterine and spermatic veins were increased to an *extraordinary extent* in their calibre." (1)

Thus, also, in a case of encephalic phlebitis, by M. Gendrin, where the longitudinal sinus became the seat of severe inflammation. The middle cerebral veins on either side, connecting with this sinus, were tortuous, and enlarged to the size of a goose-quill, being also filled with purulent matter. (2) Dr. Lee mentions an instance in which

"The left spermatic vein, from the uterus to its junction with the left emulgent vein, was seen distended to nearly the size of the vena cava itself;" there being no affection of other veins. (3)

We learn, also, from numerous cases of phlebitis, that a complete obstruction of the femoral vein may exist without any remarkable enlargement of the superficial veins. In most of the original cases of crural phlebitis which are given in detail by Dr. Lee, and in all of which there were obliterations or perfect obstructions of important veins, and, in three instances, of the vena cava, there does not appear to have been any enlargement of the superficial veins. In one of his cases,

"The left internal iliac vein and femoral veins, to the middle of the thigh, were all thickened and filled with soft coagula of lymph and pus. The vena cava, to about two inches below the entrance of the hepatic veins, was completely blocked up with a coagulum of lymph adhering to its inner surface; and yet, the right common internal and external iliac and femoral veins were *all in a healthy condition.*" (4)

Or, is it still surmised that the veins, in the early stage, are in a state of relaxation, and not of active dilatation? Why then do we find in other veins connected with the dilated ones, that the same mode of disease has resulted in their perfect contraction? Is an antecedent state of relaxation favourable to tonic contraction? Or, does not the difference rather depend upon the influence of different degrees of inflammation upon the venous tissue, and is analogous to the artificial impressions produced by differ-

(1) De Vasorum Sang. Inflam. A. G. Sasse, 1797. (2) Revûe Méd. Avril, 1826.

(3) Op cit. p. 62. — In these cases the enlargement was, of courše, owing to more or less disorganization.

(4) Op cit. p. 136.

ent agents as stated at page 387 ? (1) Disorganization is certainly not the primary cause, since this may exist equally in the dilated as in the contracted veins. From the obliterated parts we may often pass in continuous and gradual succession to other veins in a state of enlargement, where inflammation is still active, but less violent, (developing here the operation of that constitutional principle which subserves the ingress of blood from the capillary arteries, as it does in the former case that of contractility,) and pursue our analysis till we come to others where it is less strongly pronounced, and onward yet till it fades away in their dilated and remote extremities. Again, in some cases of phlebitis, where the predisposing causes have operated extensively, we find other connecting media in those sympathetic affections of the veins which supervene on the primary inflammation. In Arnold's case, (p. 436.) bleeding in the cephalic vein induced violent phlebitis. After a few days, "all the superficial veins of the knee and thigh became excessively swollen, forming a conspicuous net work." But the inflammation was so comparatively slight in these veins that Dr. Arnott says not a word of their appearance on dissection, but regards the cephalic vein, for about six inches, as the only seat of venous inflammation. It is, however, in the sympathetic succession of active phlebitis in some remote part to venous congestions of the organic viscera, as we have hitherto stated, and as will farther appear, that we may legibly trace the pathology of the latter through the well known condition of the former.

In phlebitis, therefore, we have the various gradations of inflammation, from that which results in disorganization of the principal veins, to its ultimate, insensible state, which constitutes the pathologic condition of the superficial vessels, and which we regard as a near approach to an instance of simple venous congestion. And, although the phenomenon suggested by Dr. Arnott, that "the inflammatory changes are limited by the current of blood," &c. (p. 369,) be generally true as it respects the activity of disease, there often happens a propagation of the affection in subdued degrees beyond the supposed limitations; but, it is especially in the direction of the radicles that the inflammation is continued

(1) If it be surmised that the difference be owing in the latter case to a specific difference in the exciting causes, we may infer the same as to the different degrees of inflammation; since even common inflammation undergoes remarkable modifications at its different stages, as denoted by the great difference in its products.

in the latter conditions. This is particularly remarkable in the lower extremities, and instances are recorded, as by Meckel,(1) where traumatic phlebitis of the umbilical vein has been propagated to the vena portæ and its branches, and to the peritoneum. (See APPENDIX on *Erysipelas.*) It equally shows, however, the principle as indicated by Arnott, though liable to those modifications which forever happen in the abnormal conditions of organized beings where an absolute fundamental law is not concerned. (Vol. I. p. 626, *note.*)

Having now seen that the dilatation of the veins in varix, hypertrophy, and phlebitis, depends upon inflammation of their coats; having seen that this phenomenon is commonly attendant on undoubted inflammation of the venous tissue, whether affecting the veins of the lower extremities, or those which descend from the head, where gravity favours the return of blood, and, therefore, the dilatation is occasioned by it; and since it cannot be shown that in simple congestion it depends upon any other cause, it appears to be a philosophical conclusion that the same cause is common to the whole. When contraction follows intense degrees of inflammation, there is ample proof that it is consecutive to a state of dilatation, being the result of a progressive increase of disease.

We have pointed out in our ninth section a variety of products which are found in varix, the dilated veins connected with carcinomatous tumours, ulcers, &c. which go with other phenomena to prove the existence of inflammation as the pathological cause. Analogous coincidences may be sometimes traced in venous congestions of the organic viscera, especially in the existence within the veins of coagulable lymph. In these particular situations we believe that no antecedent obstruction to the circulation has ever been rendered probable. We shall take an example from the brain, where gravity cöoperates with the other causes of venous circulation in preventing all passive accumulations of blood. The symptoms had been violent; the subject 20 months old.

"On raising the dura mater," says Dr. Bright, "*all the larger veins* on the surface of *both* hemispheres, running into the longitudinal sinus, were seen round and hard, quite filled with yellow coloured coagulum, *as if injected with wax*, while the whole vertex was covered under the membranes with extravasated blood of a deep purple colour. The longitudinal sinus was also filled with a

(1) Dictionnaire de Méd. t. xvi. Art. Erysipèle.

similar coagulum of fibrin, taking the exact form of the sinus." A section of the brain presented many small points of blood, and the ventricles were *distended with serum.* (1)

Here was no other physical evidence noticed, than the foregoing, that throws any light upon the state of the veins; but the vital phenomena, the size and extent of the solid mass, the disappearance of the colouring matter, the effusion of blood, (Sec. 16,) and the entire absence of any primary cause of obstruction to the circulation, evince the dependence of the "coagulum" on an inflammatory state of the venous tissue. Besides the powerful tendency of gravitation to prevent an accumulation of blood, the structure of the longitudinal sinus precludes the supposition that any obstruction could have existed. Had, also, the "coagulum" been merely a deposition of fibrine from stagnant blood, it is obvious that the vessels should not have appeared "quite filled, and as if injected with wax," but the coagulum should not have occupied more than one half the area of the veins. It certainly appears almost incredible that such a formation should have been compatible with a temporary prolongation of life, in whatever way it may have happened; but we think with our author that it must have taken place antecedently to death. We have, also, another important object in noticing this particular case, which we shall state in a note below, with other objections to our author's conclusion. (1)

(1) Bright's Medical Reports, vol. ii. case 24, p. 347.

The plate by which Dr. Bright illustrates this case clearly shows that it was one of capillary effusion. (See our vol. i. p. 377.)

(2) Dr. Bright has given a practical bearing to the foregoing case which is of the highest moment as to the principles it involves. He supposes that the loss of blood by 14 leeches, applied in numbers of about three at distant intervals in as many days, was the cause of the coagulation. On the contrary, we think that this case was less actively treated than the symptoms demanded. Experience teaches us, (vol. i. pp. 152, 338,) that in instances of this nature leeching is often powerless. In infancy general bloodletting is especially important, far beyond any of the inflammatory affections of other organs incident to that age; which may be often advantageously treated by leeching where the same condition of disease in adults might require the lancet. The reason of this we have already stated; (p. 377 — 379.) The brain is now undergoing a rapid development, and the cerebral veins and tonic forces predominate in this organ. (P. 362.) Inflammatory affections of these vessels will, therefore, be more likely to yield to the universal impression of the general remedy. (See *Bloodletting*, Sec. 2, and 3.) Again, the supposed coagula under consideration have never been shown to have resulted from the most extensive hemorrhages and obstructions of blood. (Vol i. pp. 173, 275 — 277, 288 — 293, 310 — 330.) If they occur as coagulations, it is after the death of the subjects. M. Piorry put this question at rest for the many thousandth time. "I was curious to know," he says "whether these coagulations could have

The formations in the cerebral veins of which we are speaking have been noticed for centuries, and have presented so little the characteristics of simple coagula, that they were anciently regarded not only as organized substances, but as real animals. The history of the cases shows them to have depended upon cerebral congestion. (1)

occasioned the death of the dogs. To resolve the question, I opened them immediately after death, (and where the dogs only died some hours after bleeding,) when no coagulated lymph could be discovered." "Dogs also require enormous (énorme) quantities of blood to effect their death." (*a*) With a reference to this inquiry, we have visited slaughter-houses, where the butchers are in the habit of bleeding the animals excessively for many hours before the fatal blow is given. They are then immediately opened, and we have never been able to discover the supposed coagulations, even in the heart. If, also, the coagula depend, as is supposed, upon a languid circulation of the blood, why do we not meet with them in cases where the general circulation is extremely prostrated by disease? Why only in the veins of the brain, and not in those of the legs where gravity is against the return of blood?

This doctrine of stagnation and coagulation appears to have originated with Hippocrates, (p. 230 ;) and to have been generally cultivated when less was known than at present of the physiology of life. It was attacked by Pasta, (*b*) and Senac, (*c*) who deny that polypi ever form in the heart or blood-vessels during life, but consist of coagulations which take place at the time of death, or immediately after. Testa, Corvisart, Andral, Bright, Burns, Hope, Hodgson, Kreysig, Bouillaud, Velpeau, Laennec, and many other eminent observers, have revived the ancient doctrine of vital stagnation of blood as the source of polypiform concretions, and the foundation of disease. (*d*) Chisholm very gravely describes what he denominates an "epidemic polypus" which attended a fever at Grenada in 1790. There was sometimes a concretion two feet in length, extending from the heart into the pulmonary artery. (*e*) Mechanical doctrines in relation to life generally have their extremes. This is one of the instances, since some authors, as Hoffman, (*f*) suppose the coagula to depend upon a redundancy of blood.

From what we have now said, it follows that there is no foundation for the belief that polypi are generated in the heart by the abstraction of blood; an opinion which has been sometimes expressed with so much confidence and force, and in such distinguished quarters, that it has been a fearful obstacle to the proper treatment of disease. Dr. Hope, for instance, thinks that "one of the greatest dangers of excessive bloodletting arises from the risk of the formation of polypi in consequence of a languor of circulation." (*g*) With such an apprehended evil, what might be otherwise a moderate is very liable to be considered an "excessive bloodletting."

(1) Several cases are related in the Sepulchretum, t. i. l. 1, s. 1, obs. 116, 117, 118, 119, 120, 121. In obs. 116, they occurred in an epidemic apoplexy. Also, s. 2, obs. 1, Gricsel says he "always found concretions in the apoplectic subjects whom he had examined, either in the brain or heart, or in both."—*Miscellan. Curios. An.* 1670, *obs.* 74. Wepfer also describes them in apoplectic cases; and Bayle found

(*a*) Collection de Mémoires, pp. 225, 227.

(*b*) De Motu Sanguinis post Mortem, et de Polypi Cordis.

(*c*) Essais de Physique ; and Dissertationes Physiol.

(*d*) See Bouillaud, in Archives Gén. de Méd. t. ii. and v.

(*e*) In Duncan's Annals of Med. 1800, p. 407; and Chisholm on the Pestilential Fever of the West Indies, vol. ii.

(*f*) De Magno Venæ Sect. t. iii. p. 267. (*g*) On Diseases of the Heart, &c. p. 512.

"In chronic diseases of the liver, the branches of the veins are sometimes filled with plugs of coagulum. This fact was first pointed out to me," says Dr. Hodgson, "by Dr. Farre, and I have since had *several* opportunities of observing it. I have also found the branches of the pulmonary veins filled with coagulum in cases of extensive tubercular disease of the lungs." (1)

It is in such affections of the lungs that venous congestions are very apt to take place in these organs, and which have been well described by Dr. Armstrong.

"It is perhaps impossible," says Dr. Lee, "to determine, for the most part, the precise period of the invasion of uterine phlebitis, from the total absence of *local pain, or of other symptoms;* but, it is most probable that it begins soon after delivery, and remains *stationary* for a time," &c. "The uterus may return to the reduced volume it usually assumes after delivery; the lochial discharge may continue, and the inflammation and suppuration of the veins which had caused the whole constitutional disturbance may have been *wholly overlooked.*" And again, "where the *veins alone* are inflamed, the peritoneal and muscular tissues remaining unaffected, *there is often either no pain*, or only a *dull* pain with a *sense of weight* in the region of the uterus, and *no other local* symptom by which the disease can be recognised." (2)

In a remarkable case by Mr. Anderson, which we shall soon relate, Mr. A. was "unable to pronounce upon the character of the disease till he recognised the swollen and hard condition of the femoral vein." Dr. Duncan has also shown that where phlebitis is produced by dissection-wounds, &c. "that the diagnosis is *often* difficult." (3) Kirkland, one of the first who maintained that true puerperal fever often depends upon inflammation of the uterus, agrees with the foregoing writers in saying that "the *uterus* will frequently show no other sign of being diseased, for some time, than the *constitutional* symptoms; which may, perhaps, be owing to its particular structure." (4) Such, however,

them in the brain in the same affection. — *Tract de Apoplexia, p.* 96. Morgagni found them in the vessels of the head; but they were not common in apoplectic subjects. — *Ep.* 4, *a.* 23. Pasta, Marc, Vicq. d'Azyr, say that these formations exist chiefly in the sinus rectus, and at the junction of the lateral and longitudinal.

(1) On Diseases of the Arteries and Veins, p. 527.

(2) On Some of the Most Important Diseases of Women, pp. 51, 54, 55. 1833.

(3) In Edin. Med. Chir. Trans. vol. i. p. 617, 1824. Generally our *italics*.

(4) On Child-Bed Fevers, pp. 79, 49, 66, 71, &c. 1774.

Still we do not believe that a vigilant practitioner can overlook the early existence of active phlebitis. Venous congestion is often of more difficult recognition. The "constitutional symptoms" must be especially regarded in either case; but these are often "overlooked" in seeking for what has no existence. It is like that "fallacious security which may impose upon an inattentive accoucheur, and allow his pa-

is not true of inflammation of the uterine tissue; and we shall have endeavoured to show that the obscurity is owing to the constitutional nature of the veins, which were the seat of the inflammation where Kirkland had suspected other tissues of the womb. (Vol. I. p. 540, *note.*)

The foregoing facts are applicable to venous congestions, and it is often the "constitutional symptoms" alone which indicate the presence of the disease. The absence, therefore, of local symptoms, from what we have just seen of active phlebitis, in no respect contradicts our philosophy of venous congestion. On the contrary it should lead us to the conclusion that an analogous state of the veins in congestion is an essential cause of that constitutional disturbance which so often simulates the phenomena of active phlebitis. The resemblance is often remarkable in simple venous congestions, but becomes conspicuous in congestive fevers. This condition of the veins, indeed, imparts to idiopathic fevers their great malignancy; and, even in simple venous congestions, the subjects are not unfrequently snatched away when no danger is apprehended. In these instances, nothing is revealed by the dissecting knife but a fulness of the cerebral, or abdominal veins, and this species of plethora has not been considered adequate to the unexpected issue. In other cases, also, appearances have been even more delusive, since the blood may have gravitated from the congested veins into the vicinity of the heart. In such cases, the vital powers have been rapidly depressed, as they often are in uterine phlebitis. The functional changes at large are in no respect such as result in organic lesions.

The sunken, prostrated state of the circulatory organs in numerous cases of venous congestion and active phlebitis affords a striking parallel; and, under equal circumstances, it has often deterred practitioners from the use of the lancet even more in violent inflammations of the veins than in venous congestions. What pathology indicates, but fear condemns in one case, unaided experience, from its greater opportunities, compels us to adopt in the other.

"In all the cases of *crural* phlebitis which I have witnessed," says Dr. Lee, "there has been such feebleness of the pulse, and prostration of strength, that

tient to perish of uterine hemorrhage, before there is any suspicion of the danger which threatens." (*a*)

(*a*) Capuron, Cours d'Accouchmens, p. 286.

I have not ventured to draw blood from the arm." (1) And again, "the symptoms in *uterine* phlebitis, from the commencement, are generally those which contraindicate general bloodletting." "In many cases the blood will not flow in a stream where venesection has been performed, a few drops trickling from the arm." And so, often, in venous congestions; when syncope will readily supervene in either affection. (Vol. I. p. 208, &c.) Dr. L. "considers the local abstraction of blood at the commencement of the attack to constitute by far the most important part of the treatment." (2)

Such were once the prevailing remarks in respect to those venous congestions and congestive fevers which are attended by a prostration of the forces of life; and it was only a cautious, though still neglected experience, which demonstrated the pernicious effects of a stimulating or procrastinating treatment, and the utility of general bloodletting. Nevertheless, we have no doubt of the greater necessity of general bloodletting in venous congestions, than in active phlebitis, from the circumstance of the former being generally connected with vital organs. (Vol. I. pp. 145, 184 — 192, 198 — 233, 235, 269, 279, 289, 342 — 360, &c.)

Hippocrates appears to advert, in several places, to uterine inflammation, and many of its results. Thus: —

"Si ignis sacer in uteris (pregnantibus) fiat, tumores fiunt *lividissimi* a pedibus usque ad crura tota, et ad lumbos. Quanto vero tempus amplius progressum fuerit, etiam pectus percipit, et febris habet magna, et rigor supervenit, et spiritus densus, et animi deliquium, et debilitas, et dolor totius corporis, et tristatur, et mente variatur. Et affectio ascendit ex imo ventre ad lumbos, et ad dorsum, et præcordia, et pectora, et caput, et stomachum, et mortua esse videtur." (3)

The symptoms of phlebitis have been compared by most writers to those of congestive typhus; (4) and, how constantly

(1) On Some of the Most Important Diseases of Women, &c. p. 175.

(2) Op cit. pp. 112, 175, 177. — Leeching, to a certain extent may take the place of general bloodletting, but we hold it doubtful; whilst the former remedy, if carried far, may be pernicious where the latter will succeed. (Vol. i. p. 145.)

(3) De Morb. Mulier. l. 2, s. 3, v. 8 — 15.

(4) Vide Hodgson, on Diseases of the Arteries and Veins, pp. 511, 515, 518. — Travers' Essay on Wounds and Ligatures of Veins, in Cooper's and Travers' Surg. Essays, vol. I. p. 286. — Ribes, in Mém. de la Société Méd. d'Emulation, t. viii. p. 628; and in Revûe Méd. Jul 1825, p. 1. — Breschet, in Journ. Compliment. du Diction. des Sciences Méd. t. ii. p. 325, t. iii. p. 318. — Bouillaud, in Revûe Méd. Juin, 1825. — Roots, in St. Thomas' Hospital Reports, No. 3, p. 358. — Williams, in Ibid. p. 327. — Diction. des Sciences Méd. Art. Phlébite, t. xli. — Duplay, in Journ. Complim. 1833; and Journ. Hebdom. 1830. — Marsh, in Dub. Hosp. Rep. vol. iv. p. 521. — Tonnellé, in Archives Gén. 1830. — Dance, in Ibid. 1828 and 1829. — Luroth, in Répert. Gén. d'Anat. Patholog. 1828. — Duncan's Cases of Phlebitis, in Edin. Med. Chir. Trans. vol. i. pp. 470 — 547, 602 — 631. — Dupuy,

does it happen in severe cases of simple venous congestion, acute or chronic, (Vol. I. p. 199,) that analogous symptoms sooner or later take place. Whilst it is the tendency of all these affections to excite the general circulation under particular circumstances, modifications are constantly occurring in which all the forces of life are excessively prostrated. These contingencies depend on a variety of causes; such as the peculiarities of the predisposing, the violence with which they operate, the organ which may be the seat of the affection, especially if the brain, &c., which have been considered in our Essay on Bloodletting. (Sec. 3, 5, 9, 10, 12, 13, 15.)

In the city of New York, where typhus fever is almost unknown, but where venous congestions are common, either in a purely local form, or complicated with intermittent or remittent fevers, there are many practitioners who designate the diseases in their worst forms as typhus fever. Every week our bills of mortality furnish cases of death which bear the last denomination. Nearly all the instances, however, we have reason to believe, were either cases of venous congestion, of the foregoing nature, or consecutive to some active form of common inflammation. (1)

Injection des Matières putrides dans la Veine Jugulaire, in Nouv. Bib. Méd. 1823, p. 90; and the various experimenters with injections into the venous system. (Vol. i. pp. 397, 515 — 521, 531 — 533.) Armstrong, *passim*.

(1) The gentleman to whose memory we have inscribed this work, and who became familiar with typhus fever during our last war, as it prevails in a more Northern climate, assured us that he had never witnessed a case of the disease in this city, during a long and extensive practice. Dr. Gilbert Smith, also one of our able and eminent physicians, has not seen typhus in New York for the last twenty years. Dr. Bliss met with typhus in the New York Hospital about 25 years ago, (now 1837,) since which it has disappeared from his observation in this city. These cases, he states, were brought from cellars, or other places remarkable for their filth. Other gentlemen have confirmed to us this opinion. Nevertheless, it would appear that cases of genuine typhus fever were received at the New York Hospital, in 1836; but they had been all contracted in Ireland. (*a*) (Vol. i. p. 304, *note*.) Our able and learned Prof. Lee remarks that, "fevers of a purely typhoid character from the beginning, are almost unknown at present in New York. Those denominated nervous, and typhus, "117 reported deaths for 1836,) are generally manufactured, or made thus by improper treatment." (*b*) The number of reported deaths, in the city of New York, from "typhus and nervous fever," from 1805 to 1836, was 3602; whilst those from all other fevers, where the type was designated, including the endemics of yellow fever, were only 2486. This fact justifies the remark of Dr. Swett, as stated in our Vol. i. p. 296, (also, p. 293 — 309.) The confusion arises in part, however,

(*a* See New York Journ. of Med. & Surgery, July, 1839. p. 100.
(*b*) In American Journ. of Med. Sciences, vol. 19, p. 32.

The humoral experimenters, having produced some of the phenomena of typhus fever by injecting putrid, and other mor-

from the want of practical information as to the character of typhus fever, whilst we are told by high trans-Atlantic authorities, that we may find in puerperal fever, phlebitis, and in the fatal stages of local inflammations, the purest specimens of typhus. (*a*) Nay more, that we may generate the disease, and even yellow fever, by putrid and other injections into the veins of animals. (Vol. i. pp. 397, 515 — 520, 539, &c.) And is not this allusion embellished by its fanciful application to the doctrines of contagion (*b*) and the humoral pathology? With such standards of comparison, and a just reverence for learned and able men, it is not remarkable that we should be beguiled from a stern observation of Nature.

We have made these remarks from the apparent conflict of the foregoing statement with that tribute which we have paid, in another place, to American physicians, which they so eminently deserve, and the merit of which makes us proud of the genius of our country.

Typhus fever appears to be confined to the north of the latitude of New Haven, in Connecticut, prevailing as an endemic or epidemic between that limit and the most northern. Smith agrees with us in this opinion. (*c*) We saw it regularly, fall and winter, in Montreal, during a residence of five years in that city, and have witnessed it in many of the highest inhabited parts of the state of Vermont. We never saw it in Western New York, but constantly met with the congestive diseases of this region, and which, we may observe, are rarely seen, except in isolated places, north of the New-Haven line. From that line south, they prevail far into South America. These, and many other facts which we have collected, but have no room for insertion, are opposed to the conclusions at which Dr. Boot arrives as to the identity of plague, typhus, yellow, remittent, and intermittent fevers, &c. in his able Memoirs of Dr. Armstrong; whose second volume is devoted mainly to this inquiry.

The Hospital of Philadelphia has furnished many instances of typhoid, or typhus fever; but we infer not only from the foregoing facts that they were either imported cases, or derived, as in the sporadic cases of our own hospital, from cellars, &c.; but from the long experience of the distinguished Dr. Dewees, of that city, who "*has never had an opportunity* of seeing a case of *typhus*," "although," he says, "we hear constantly of this disease, and our bills of mortality never fail to record deaths from this fever." (*d*) He considers it peculiar to northern climates, and has no doubt of its having been faithfully observed and described by many writers. (*e*) And so Dr. Davidge: "Typhus is not, so far as my observation has extended, a disease of Maryland, perhaps not of America; at any rate not south of the New England States." (*f*) "Typhus," says Bancroft, "is properly a disease of cold climates." (*g*) Hunter considered it a disease of winter. "Heat," he says, "proves a prevention to the disease, as much as cold forwards its production." (*h*) The same statements are

(*a*) Mr. Morgan, in his able work on the Principles of Surgery, frequently speaks of typhus fever supervening even as an effect of surgical operations. We suppose he must refer to cases in which venous inflammation is induced.

(*b*) Let us try the experimenters by their own hypothesis. Will they maintain that the artificial diseases can be propagated according to their views of the contagion of typhus? And does not this conclusion equally show that pneumonia, enteritis, &c. can never degenerate into typhus? (Vol. i. pp. 451, 519, 521, 532, 533, 535, 543, & notes.)

(*c*) Nathan Smith, on Typhus Fever, 1824. — (*d*) Practice of Physic, pp. 170, 171.

(*e*) See Gallup's able work on the Epidemics of Vermont, 1815; and Nathan Smith, ut cit.

(*f*) Davidge, in his Edition of Bancroft on Fever, &c. p. 518.

(*g*) Ibid. p. 342. (*h*) Lon. Medical Trans. vol. 3, p. 348.

bific matter into the veins of animals, have appropriated this term in the most indiscriminate manner. The coincidences, as we have endeavoured to show in our Humoral Pathology, depend essentially upon venous inflammation ; and it is even stated by such as confound the affections, that "*bits of stick* inclosed in the veins produce exactly the same phenomena." (Vol. I. pp. 515—524, 531, 539 — 549, &c.) It is clearly the venous affection, in all the foregoing instances, from spontaneous phlebitis down to the most simple form of venous congestion, which imparts a general colouring to the whole ; and, as that may happen to predominate in any of the cases, so is the term typhus fever applied to the whole. Armstrong, in common with others, carries this doctrine as far as "dissection wounds," and even to the "introduction of putrid animal matter into the blood." In the sentence which follows next, he allows that "the practical application of the doctrine is of great importance." How important it is, we have endeavoured to show in our Humoral Pathology. But observe :—

"I have seen," he says, "twelve cases of dissection wounds. *They have all had the same character.* If I *put out* the consideration of *the local affection*, and the *red tender line running up the arm*, and the *tenderness of the axillary glands*, I should have said it had the precise characters of typhus fever, and I should say that the *remote occasion is peculiar.*" (1) (Vol. I. pp. 489, 491.)

Here we have a striking example of the manner in which the humoral hypothesis may vitiate the medical philosophy of a distinguished observer, and prompt him to "*put out of consideration*" the most essential part of the pathology of disease. Even our own unique Rush observes, that,—

"It has been proved by facts, that opium, when taken in an excessive dose, acts by inducing a *similar* state of the system with that which is induced by the *miasmata* which bring on *malignant* and *inflammatory fevers.*" (2)

Here, again, we see the tendency of even great genius, by neg-

made by Blane, Lind, and Trotter, in their respective treatises on the Scurvy, &c. Even in the city of Boston, "there was not in the general hospital any case of typhoid fever in 1821, one case only in 1822, and five in 1823; so that almost all the cases occurred in the subsequent twelve years." (*a*)

We are aware that we shall be asked, "have you examined the glands of Peyer?" We certainly have not, excepting to satisfy ourselves that they are morbidly affected in our bilious remittent fevers. And may we not in our turn propound the inquiry, whether some of our reputed cases of "typhoid fever" do not obtain their nosological designation after the glands of Peyer have been duly investigated?

(1) Lectures on Acute and Chronic Diseases, vol. ii., pp. 103, 127.

(2) Medical Inquiries and Observations, vol. iv. p. 357.

(*a*) Jackson's Report on Typhoid Fever, Introduc. 1838.

lecting the differences in the remote causes of disease, and the nature of the vital properties, to grasp at superficial signs, and are admonished to scrutinize the opinions of the most enlightened reformers of error, whilst we pay them every homage which genius and erudition can demand.

"It was," says M. Double, "by applying this term, (typhus fever,) to a variety of affections, that you were led into an inextricable chaos of difficulties in your late discussion on that subject." (1)

In speaking of the "typhoid fever" of Paris, we have endeavoured to exhibit the evils of these mistakes — leading to the abandonment of bloodletting in the most violent and disorganizing inflammations, or to their treatment by tonics and stimulants. In erysipelas, also, which is regarded by some able observers as a venous inflammation, so analogous are some of its worst phenomena, in many cases, to those of congestive fevers, that very eminent practitioners take the broad ground of stimulation.

"The bark and wine system," says Dr. Armstrong, "is of all the most fatal practice generally in erysipelas, though it is the common practice in London."

And, as a commentary upon this eulogium, and standing in near connection with our preceding extract, he remarks,—

"It is strange how men, with strong intellectual faculties, superior to my own, go on to gray-headed old age without profiting at all by their experience." (2)

Again, he is sustained by Mr. Travers, in his practical conclusion, who says,—

"At this moment, there are hospital physicians, and others of equal repute, who treat the disease (erysipelas) with ammonia, camphor, bark, and even alcohol, at its outset." (3) (Vol. I. pp. 296, 303.) We have never known the practice adopted in America.

Since the foregoing remarks in relation to the coincidences between phlebitis and congestive fevers were written, according to our statement introductory to this essay, other observers have appeared, as already said, with facts and opinions which afford confirmation to our general doctrine of venous congestion. Dr. Roupell is of this number, from whom we may quote the following paragraph as nearly coincident with some of our own remarks.

"We see," says Dr. R., "that inflammation of a vein, and admixture of morbid secretions from the inflamed part with the blood, causes, on ordinary occasions, typhoid symptoms, and gives rise to a fever attended by subsultus tendi-

(1) Gaz. Médicale; and Lon. Med. Gaz., May 13, 1837.
(2) Lectures on Acute and Chronic Diseases, vol. ii. p. 101.
(3) Further Inquiry into Constitutional Irritation, p. 145.

num, low muttering delirium, and a black tongue. The similarity of symptoms in phlebitis, and in one stage or period of typhus, gives reasonable ground for belief that the corresponding appearances in the two diseases arise from a not dissimilar cause, and hence we see an explanation of one great train of effects in tyrnus," &c. (1)

Returning again to our parallel between venous congestion, and simple active phlebitis, we may say that in the former case we have often a greater extent of affected veins to supply the amount of constitutional disturbance which is impressed upon the system by the more limited affection; besides the disturbing influence which arises from the derangement of the more vital organs that may be the seat of congestion. (Vol. I. pp. 198—204, 211—232.) Violent inflammations of the arm produce comparatively little disturbance of the system with that which follows phlebitis of one of its large vessels. Not so, however, with the organic viscera. Their inflammations produce a powerful constitutional impression. So, also, when acute, though low degrees of inflammation affect extensively the venous system of the liver, brain, &c., we should have results corresponding with the physiological constitution of those organs, and their natural relations to other parts, in which their venous tissue especially participates.

A common phenomenon of venous congestion, and of ordinary phlebitis, is a contracted state of the general systems of the arterial and venous capillaries, and a consequent accumulation of blood about the right cavities of the heart. Nor is this condition peculiar to inflammation of the veins. It is well exemplified in some of the paroxysms which attend tuberculous phthisis. Indeed,

"Every experienced physician must know," says Dr. Armstrong, "that, under particular circumstances, even in acute inflammations of the viscera, the action of the heart is so much depressed, that the general excitement does not at all correspond with the danger and extent of the topical disorder." (2)

Venous congestions often spring up in different parts in rapid succession. Such, also, are the manifestations of phlebitis. The latter is apt to be attributed to pus. (3) (Vol. I. p. 523—524.) In both cases, however, it is greatly owing to the special tendency of the venous tissue to sympathize with its various parts. (Vol. I.

(1) A Short Treatise on Typhus Fever, p. 125, 1839.

(2) On Typhus Fever, p. 74.

(3) If pus be the cause, why are not the veins more universally affected; why not secondary abscesses in the brain? Why do they not more generally, or always, occur when phlebitis results in venous suppuration?

p. 134 — 138.) So great is this sympathy, that "*it is a well known fact that in cases of phlebitis, venesection, performed at any part, however remote from the seat of the inflamed vein, is apt to be attended by an attack of phlebitis in the wound.*" (1) That such remote determinations of the disease are more common in venous congestions than in active phlebitis is owing, in part, to congestive disease constantly affecting vital parts, where the principle of sympathy predominates, not only in relation to the organs generally, but to their venous system, which partakes the nature of the organ of which it forms an integral part. In the following example it will be at once apparent to such as are familiar with common pneumonia and pneumonia typhoides, that it is in venous congestions of the lungs that the distinguished observer has formed his experience. Thus,

"It has been remarked, and by no less an authority than Sir A. Cooper, that inflammation of the veins in the arm after venesection is much more frequent when the patient is bled for thoracic inflammation, than for any other disease whatever. Whether the condition of the lungs and vein *act* and *react upon each other* is a point which we leave where we find it." (2)

Nevertheless, it is evident that the reviewer, although advocating the humoral doctrine of secondary abscesses, felt the contradictory application of the foregoing fact, and was half inclined to tolerate the doctrine of sympathy. (Pp. 248, 279, 283, 290, 333, 351, 364, 369.)

Again, much is due to the predisposition induced by the more universal impression of the remote causes upon which venous congestions depend, as well as to the modified form of the inflammatory action. So, also, if idiopathic fever exist, or other peculiar states of the system, the influence of the local affection may be greatly modified. In these conditions, also, which may be simultaneous or consecutive to each other, there will be a reciprocal tendency in each to maintain the other. When venous congestion affects the brain, the phenomena, in the early stages, may be very analogous to common inflammation of that organ; but, at a later period, and whilst the disease may be yet in its simple state, there may succeed, gradually or suddenly, that prostration of the vital powers which distinguishes the most complex forms of venous congestion; or, there may be developed, as in apoplexy, either from the direct effect of disease upon some

(1) Med. Chir. Rev. Lon. vol. xxviii, p. 539.
(2) Med. Chir. Rev. July, 1828, p. 477.

peculiarly modified state of the cerebral powers, or from this cause in connection with pressure, an instantaneous and pernicious influence of the brain upon all the animal and organic functions,—perhaps extinguishing them in a moment of time. (Vol. I. pp. 157—179, 189, 342—370.)

When simple congestions pass on to a fatal termination, it frequently happens that the various constitutional phenomena of active phlebitis become strongly pronounced, from the increased intensity of the venous affection; and the patient is then commonly said to have died of typhus fever. In active phlebitis, some local cause is commonly necessary to develope an attack. But when, in this affection, some general cause has been in powerful operation, as in the case of Arnold, (p. 442,) a corresponding series of veins, remote from the primary seat, become sympathetically and extensively inflamed; or, as frequently happens, the veins of the great viscera will come under the morbific influence of the superficial; when the result, as we have seen, is commonly ascribed to the irritation of pus.(1) Generally, there may be most excitement, or the least depression of the circulation, when venous congestion or active phlebitis are owing to causes whose operation is local; and, in these instances, where the inflammation is violent, or the affection in either case suddenly induced, as after certain injuries, the commotion in the organs of circulation may be very great. Of this local nature we regard the sympathetic influences of the stomach in developing those cerebral congestions which lay the foundation of many apoplexies; whilst, on the other hand, where the same affection depends on a different modification of cerebral congestion which has been induced by malarious or other analogous causes, and where, too, there is generally a cotemporaneous congestion of the liver, the circulatory organs and vital forces are apt to be depressed at the invasion of the paroxysm. Prostration, however, is less in these cases if the paroxysms be developed by an overloaded stomach, than where it results from the natural progress of the disease; and this is true of congestions of other organs where the predisposing causes have been of a general nature. The same, also, may be affirmed of active phlebitis, if induced by a local cause, as by venesection, when the predisposition has been established

(1) See Breschet Mém. sur l'Influence des Veines, in Journ. Comp. du Diction. des Sciences Méd. t. iii. p. 318.

by general causes. In uterine phlebitis it may be otherwise, if parturition be regarded as a cause in a local sense. But this cause, in itself, is often of a very prostrating nature, and gives activity to venous congestions of the abdominal viscera which have been antecedent to the uterine affection, and upon which the latter has especially depended, as does puerperal erysipelas, where uterine phlebitis, or puerperal fever, are endemic. This, then, will be another source of depression, and is the reason, in part, why the commotion in active phlebitis from more direct injuries is greater than in the uterine affections. The effects are always modified by any important coexisting venous congestions; and it is these, in part also, which have more or less paralysed the hand of art in uterine phlebitis. When, also, active phlebitis is produced by mechanical causes alone, the inflammation is more of the common kind, than when the result of the specific properties of morbific agents; which, too, may be equally affirmed of venous congestions. In the latter affection, therefore, the viscera involved, and the modified forms of inflammation, determine an order of symptoms which bear relations to those of active phlebitis of spontaneous origin, or from mechanical injuries, according to their respective peculiarities and the nature of their causes. That congestion, also, should affect the organs which may be its seat in a very different way from common inflammation may be inferred from the phenomena of uterine phlebitis. This observation is important, and is remarkably exemplified in those divers congestions of the brain which contribute so largely to apoplectic and comatose affections, or as they are induced by narcotics, &c.

Although for many reasons which we have stated, the heart and arteries are liable to be variously influenced in venous congestions when not complicated with idiopathic fever, they will be generally found to have been more or less excited in the early stages of the disease, though never so remarkably as in the active forms of common inflammation. The degree of this excitement will obviously depend on the organ which may be the seat of the affection, the extent of disease, &c. But, unless the disease run wholly, or in part, into common inflammation, there generally succeeds, especially in its acute forms, a depression of arterial action, and, of consequence, an accumulation of blood about the right cavities of the heart; in which state it is often for the first time seen by the physician. It is often with simple venous con-

gestions, as with congestive typhus; in which, according to Dr. Armstrong,

"The stage of excitement may pass away, and then come those malignant symptoms, and its effects, which, viewed independently of the preceding one, have contributed to mislead so many pathologists and practitioners." (1)

It is more apt to be otherwise, however, with congestions of a more chronic nature, where a preternatural frequency, and hardness of pulse, and even force of the circulation may be unintermittingly established. But this condition is liable to a sudden aggravation, when the depressing effects may follow. (Vol. I. p. 199.)

When puerperal fever is not complicated with active phlebitis, but what is called the "contagious species," an intermediate variety of venous congestion appears to be often presented, which more especially demonstrates the affinity betwixt congestion in its simple form and phlebitis. In this variety there is a preternatural fulness of the veins of the abdominal and pelvic viscera, which are commonly but incidentally noticed from the little importance that has been ascribed to the phenomenon. It may be well to say, in the first place, that Armstrong states three fundamental sources of "predisposition to puerperal fever."

"1st. One of the most remarkable predisposing occasions is a plethoric condition of the system." "2d. In pregnancy there is an increase of the sensibility and irritability of the body. A pregnant woman is a perfect sensitive plant." "3d. The peculiar condition of the uterus and abdomen powerfully predisposes to inflammation." (2)

The two last propositions involve the first, which is only a branch of the humoral pathology. (Vol. I. p. 621—625.) They are important internal elements, which must be constantly borne in mind, in connection with the light from other sources. Puerperal congestions may spring up from these sources alone, or through the agency of external causes, such as miasmata, &c. When the latter operate, then we have the most fearful conditions of puerperal disease. Armstrong appears to have had scarcely any knowledge of the venous congestions of the puerperal state, but regarded these local affections as common inflammations.

If we look at the symptoms, we shall find them analogous to those of "uterine phlebitis," and severe forms of congestive fevers, or to less complicated venous congestions.

(1) On Typhus Fever, p. 87.

(2) Lectures on Acute and Chronic Diseases, vol. ii. p. 214—216.

"The eye," says Dr. Douglass, "instead of being flushed as in other species, is pellucid, with dilated pupil. The countenance pale and shrunk, with an indescribable expression of anxiety. The surface of the body is usually soft, and clammy, and of a heat not above the natural temparature; and, not only is the skin cool, with clammy exudation, but the muscles, to the impression of the finger, feel soft and flaccid, as if deprived of their *vis insita* by the influence of the contagion. Indeed, there is such prostration of muscular strength and depression of the vital principle from the very outset of the attack, that I must suppose the contagion to act on the human frame through the medium of the nervous system in a manner analogous to that of the *plague;* nor is puerperal fever less destructive or less quickly fatal than the plague itself." (1) And so Mr. Hey. (Vol. I. p. 221.)

Here every thing, even the pathology, is supposed to consist in the direct effect of the morbific cause upon the nervous system. The operation of such a cause may be immediately prostrating to the forces of life; but, unless morbid actions follow immediately, we apprehend it will have been because the "*vis insita*" has at once reacted and recovered its natural condition. The altered actions may not be denoted by any physical appearances after death; but in the cases before us, not only the analogies, as it respects the symptoms, with other well ascertained conditions of disease in which venous congestions predominate, but the results of many inquiries into the various forms of puerperal fever, show that the venous system sustains, at least, a part of the weight of disease. Thus;

"In all the bodies of many women," says Dr. Wilson, "who died of puerperal fever, and which I inspected with Dr. Clarke, we found that the peritoneal coat and substance of the uterus had become inflamed, and that in *most* of them, *pus* had been formed, and often in a large quantity in the *veins* of and leading from that organ." (2)

M. Tonnellé states, that of 222 fatal cases of puerperal fever that occurred at the hospital Maternité in 1829, ninety exhibited inflammation of the veins, and forty, of the absorbents. In 132 of the cases, a puriform fluid was found in the veins and absorbents of the uterus. (3)

Baudelocque (4) gives a very graphic account of the symptoms, corresponding with the foregoing. Dr. Collins states that

"In four epidemics of puerperal fever, the symptoms were usually of the lowest *typhoid* description. The patients, in several instances, exhibited somewhat the appearance of those labouring under cholera." (5)

In all the foregoing cases, too, the blood not only trickles from

(1) Dublin Hospital Reports, vol. iii. p. 155. (2) On the Blood, &c. p. 415.
(3) Des Fièvres Puerp. observ. à la Maternité, &c. in Archiv. Gén. 1830.
(4) On Puerperal Peritonitis, p. 154. (5) Practical Treatise on Midwifery, p. 193.

the arm, in venesection, at certain stages of the disease, but it is very dark, viscid, &c. It often refuses to coagulate, or may speedily acquire its natural appearance after venesection. (Vol. I. pp. 129, 195—197, 440—445.) Bloodletting, too, is the *remedium principale* for the whole.(1) Analogies would thus multiply as we might go on with observers. There is a family likeness too striking to be neglected; yet every one regards many of the diseases as being distinct. It is the venous inflammation in all, which imparts the resemblance. The symptoms of puerperal fever are so allied to those of phlebitis when produced by irritating injections into the veins, that we have seen that the distinguished professor Ferguson, and others, ascribe all "the various forms of puerperal fever" to a "vitiation of the fluids," occasioned by the absorbtion of acrid matter from the uterus. (Vol. I. pp. 516, 540.) We recur to this opinion for the purpose of illustration; and we may farther add, in opposition to the imputed absorbtion of pus, or other matter, that M. Nonat has generally traced the diseased veins in uterine phlebitis to their placental terminations; where the orifices were so plugged up with coagula of blood, as well as the smaller ramifications, that it was apparent that the purulent matter in the venous radicles could not have been conveyed into the circulation.(2) See Arnott's case, (p. 436.)

It appears to us that the want of information as to the true pathology of venous congestion has been a fruitful source, not only of maltreatment in puerperal fever, (Vol. I. p. 312,) but often of an erroneous pathology in imputing the seat of disease to tissues which may have been scarcely, if at all involved in the morbid process, or mostly so as a sympathetic and perhaps unimportant result; or has led practitioners to confound one disease with another. Thus, Dr. Gooch:

"The most remarkable circumstance which the experience of the last few years has taught us about *peritoneal* fevers is, that they may occur in their *most malignant* and fatal form, and yet leave *few or no vestiges in the peritoneum* after death. The state of this membrane, indicated by pain and tenderness of the abdomen, with a rapid pulse, appears to be not one uniform state, but one which varies so much in different cases, that a scale might be formed of its several varieties. This scale would begin with little more than a *nervous affection*, often removable by soothing remedies, and when terminating *fatally*, leaving *no morbid* appearances discoverable after death.(3) *Next* above this, a

(1) See our Letters on the Cholera Asphyxia of New York, pp. 37, 38, 99.

(2) Revûe Médicale, Sept. 1837.

(3) We believe this to be generally received as good doctrine, though when we

state in which the nervous affection is combined with some degree of congestion, indicated in the cases which recover, by the relief afforded by leeches, and in the cases which die, by *slight redness in parts* of the peritoneum, and a slight effusion of serum, sometimes colourless, sometimes stained with blood. *Above* this might be placed those cases in which there are, in the peritoneum, the effusions of inflammation *without its redness*," &c. "*Lastly*, the *vestiges* of acute inflammation of the peritoneum, viz. redness of this membrane, adhesion of its contiguous surfaces, a copious effusion of serum, and large masses of lymph." (1)

That we are right in our construction of the dangerous tendencies of an erroneous pathology in puerperal fevers is shown by a remark of our author which follows, immediately, as an apparent induction from the foregoing premises, though he alleges it as being grounded upon experience. No experience has justified his conclusions against the advantages of bloodletting, whilst we have shown that the best experience establishes its great importance. (Vol. I. pp. 221, 312, &c.) The accumulated results of experience also show, that in these puerperal fevers there is a remarkable coincidence of venous inflammations and venous congestions of different parts; whilst we find various gradations of the manifest form of phlebitis till it escapes observation under the aspect of venous congestion of the abdominal viscera. That the peritoneum suffers more or less is certain; and if, as we shall see in our *appendix* on *Erysipelas*, the morbid condition of this membrane be constituted, as supposed by many, by venous inflammation, it will reflect farther light upon the more important pathology of other veins. We may say, however, that M. Ribes, (2) and others, pronounce a close affinity between puerperal fever and erysipelas, and regard the peritonitis of lying-in women as an erysipelatous inflammation. Some 40 years ago, Dr. Lowder considered the inflammation of that nature, and the fever typhoid. (3) M. Tonnellé states that the peritoneal covering of the uterus often presented little blisters filled with a serous, or purulent matter. (4)

According, therefore, to the degrees of venous affection, the parts in which that affection may be situated, the extent in which the veins may be involved, and whether complicated with com-

come to the most unequivocal marks of inflammation the same pathologists deny its existence in many other affections.

(1) Gooch, on some of the Most Important Diseases peculiar to women, p. 95.

(2) Mémoires de la Société Méd. d' Emulation, t. viii p. 627

(3) Gooch on some of the Diseases of Women, p. 25.

(4) Des Fièv. Puerp. &c. ut cit.

mon inflammation of the peritoneum, and whether the system at large be impressed by malarious causes, will the phenomena of puerperal fevers be diversified, and form the basis of a "scale" whose sub-divisions will be almost as numerous as the individual cases. And here we may remark, that these affections carry strongly a conviction of the necessity of principles in medicine.

A learned reviewer is evidently impressed with sentiments somewhat like those which we have now expressed. Thus;

"The only question in our mind is, whether a disease thus produced be genuine puerperal fever; or is not rather uterine phlebitis, with or without peritonitis, accompanied by typhoid symptoms. *It would be difficult to draw the precise line of demarcation between the two affections. Their extremes are*, it is true, *far enough asunder.* But where uterine phlebitis is accompanied, as is frequently the case, with low ataxic fever, *it is hardly possible to distinguish them.* And again, "that puerperal fever does bear a strong analogy to certain forms of erysipelas, cannot be doubted. We have already pointed out the similarity of their origin. That a similar identity exists between the worst forms of uterine phlebitis and these species of erysipelas, we have also endeavoured to show." (1) (See *Appendix* on *Erysipelas.*)

That venous congestion of important viscera is, also, a leading cause of the phenomena attending erysipelas, and what imparts malignancy and fatality to the disease, is abundantly established. It is the depressing effect of the former condition which has led to the fatal bark and wine treatment, although it be a singular coincidence that the comparitively unimportant inflammation of the skin is supposed to constitute the pathology of the disease. It appears to us that there can be no greater mistake, either in therapeutics or pathology.

We have no doubt, from the history of cases, and from morbid anatomy, that a state of venous congestion of the uterus is generally antecedent to what is known as uterine phlebitis, and that the subsequent development of the more active form is owing either to parturition, or to a neglect of the congestion should it supervene upon labour. The opinion of other observers is here of great weight, since they had not in view our doctrine of the pathology of venous congestion. Thus, Dr. Ramsbotham:

"Although a *congested state of the uterine veins* is not generally spoken of by authors, I am fully persuaded that it *not unfrequently occurs*, unattended at the onset with arterial excitement. If, however, it be allowed to proceed un-

(1) British and Foreign Med. Rev. No. 4. p. 485.

heeded, *active inflammation* both of the *uterus* and *peritoneum* may supervene." (1)

Here our able observer has embraced the whole philosophy of our subject in a single paragraph; and it is only surprising that his pathology of the "active inflammation" of the veins did not begin with their *congested* state, and that he did not carry it to all other morbid venous congestions. If the congestion, he says, "be allowed to proceed unheeded, *active* inflammation may supervene," or pass from a lower to a higher degree. But what is to be done? "Bleed," says our author, "as in inflammatory disease." What, then, is the principle upon which bloodletting relieves the venous congestion, and prevents the occurrence of "active inflammation"? Thus, also, Dr. Lee: —

"Like inflammation of other organs of the body, that of the uterus varies greatly in severity, in different cases. At some particular periods we have remarked the existence of a *disposition* to the disease in certain puerperal women, evinced by tenderness of the uterus on pressure, when inflammation has not been actually developed." (2)

These cases are doubtless often constituted by the venous congestions of which Ramsbotham speaks. That these uterine congestions should more frequently pass into more active forms of phlebitis than in other organs, probably arises from the violence the uterus undergoes from parturition; though much may be owing to the peculiarities which appertain to the vital properties of the organ. The pathology of these cases has been peculiarly embarrassing, probably on account of the doctrines of "obstruction," "stagnation of blood," and "relaxation of the veins."

"It is evident," says Dr. Lee, "that the disordered action must precede the change of structure, since we find that puerperal disease, *exhibiting all the characteristic symptoms, frequently occurs*, which *rapidly proves fatal, and not a vestige of inflammation* is to be detected. Yet, doubtless, were the disease to continue *some time* before it proved fatal, the evidence of its action might *always be detected* in the tissues; for, continued disorder invariably produces organic alterations." (P. 316 — 330.)

Even Mr. Moore, after having endeavoured to show that puerperal fever may exist independently of inflammation, on reviewing the subject, finds something in the general character of the disease which leads him to state,

"That puerperal fever has a *decidedly inflammatory character*, whatever be its cause, is now *undisputed*, and that phlebitis frequently attends it." (3)

(1) Lectures on the Diseases of Women and Children.
(2) Cyclopædia of Prac. Med. Art. Puerp. Fever.
(3) On Puerp. Fever, p. 142.

This, too, according to the statement of Dr. Helm, is the prevailing opinion at Vienna. He places "inflammation of the veins of the uterus, of its appendages, and surrounding cellular tissues as the most common primary form of puerperal disease." (1)

The vast experience of Dr. Collins leaves him in the same doubt as to the pathology of puerperal fever as we have just seen expressed by others. Nothing appears to explain the remarkable nature of the symptoms, or the rapid termination in death. The disease differs very greatly from common peritoneal inflammation, which, in lying-in women, is constantly denominated puerperal fever.

"When epidemic in hospitals," says Dr. Collins, "it is directly the reverse; at least in four epidemics which I have witnessed, the symptoms were usually of the lowest *typhoid* description, the pulse being so feeble and indistinct, as to make you dread in many, even the application of leeches; the patients in several instances of this form of the disease, exhibiting somewhat *the appearance of those labouring under cholera.*" (2) (Vol. I. pp. 221, 231, 254, 312.)

On carefully examining the symptoms and anatomical facts in Collins' cases, the conclusion appears to us obvious that there was generally more or less peritoneal inflammation, but, that it was masked by a more formidable state of venous congestion, upon which, indeed, the phenomena and fatality of the disease mainly depended. How far the inflammation of the peritoneum was of an erysipelatous nature, we cannot learn from Dr. Collins. He, however, states what is important to our views as to the agency of the veins in the general phenomena, and perhaps, also, in relation to the peritoneal affection, that,

"Puerperal fever has become epidemic in our hospital, upon *several* occasions when *typhus fever* prevailed in the city, and at other periods when *erysipelas* was met with." (3)

"Dr. Gordon," says Mr. Moore, remarks "that with puerperal fever, and at the same time, epidemic erysipelas begun, progressed with equal pace, arrived at its acme, and terminated together. He also says, that a very frequent crisis of the disease was an external erysipelas. Mr. Hey remarks, that infectious fevers were common at the time, and he does not recollect ever having seen such malignant cases of erysipelas as then. Dr. Clarke also observes that those inflammatory diseases which occurred were principally erysipelatous. Dr. Armstrong states that, in 1813, (the year of its greatest prevalence throughout England,) low fever, and typhus, also prevailed to an uncommon degree." (4)

There is good ground for believing that other diseases prevail

(1) Lon. Med. Gaz. 1838. See Kirkland on Child-bed Fevers, pp. 49, 66, 69, 71, 79, 83, 88. 1774.

(2) Practical Treatise on Midwifery, p. 193. (3) Ibid. p. 189.

(4) Inquiry into the Pathology, &c. of Puerp. Fever, p. 164.

simultaneously with epidemic puerperal fever, in which a similar pathological state of the veins performs an important part; and the British and Foreign Med. Review remarks, that the coincidence of "epidemic erysipelas," &c. "tends to strengthen our view respecting the generic character of puerperal fever." We might adduce many authorities to show the coincidence between congestive puerperal fever and other fevers of a congestive nature, the striking analogy in certain important vital signs, which always coincide with the existence of venous congestions, and which sooner or later put on what is known under the denomination of typhoid symptoms. Thus, one of the most accurate and an early commentator upon puerperal fever:

"It has been observed from the days of Hippocrates, that women in child-bed are very liable to epidemic diseases; and it might be from this cause, that in one of the *plagues* of Constantinople, most of the women in child-bed died." (1)

Nor is it remarkable, considering the doctrines which have prevailed in respect to venous congestion, and the peculiar symptoms by which it is diversified, their modifications by peculiarities in the remote causes, by the degree of its pathological cause, and its particular seat in the body, that the distinguished reviewer, just quoted, should concede that,

"The subject of puerperal fever is confessedly one of no slight difficulty, and candidly own that there is none the discussion of which we have always approached with more reluctance." (2)

The facts which are now accumulated must, if ever, enable us to form conclusions as to the foregoing disease. The symptoms, with analogies in the etiology, the phenomena, and effects of treatment in venous congestions, congestive fever, and active phlebitis, supersede the necessity of any farther aid from the debris of the body. (3) We apprehend that there is sufficient ground for an important distinction between epidemic and sporadic puerperal fever. That the former is truly an idiopathic fever compounded with local venous congestions, or more active phlebitis, and often with erysipelatous or common inflammation of the peritoneum. That the latter, or sporadic, is generally a local affection, in which there may be peritonitis, venous congestions, acute phlebitis, or inflammation of

(1) Kirkland, On Child-bed Fevers, p. 53.

(2) British and Foreign Med. Rev. No. 4, p. 487.

(3) Some observers, like M. Nonat, (*a*) have stated that injection of the visceral veins of the abdomen is the most uniform morbid appearance.

(*a*) Revue Médicale, Sept. 1837.

the cellular tissue of the womb. Authors have generally been impressed with a distinction between the epidemic and the sporadic diseases. Thus,

"The extreme difference of opinion, and the very opposite measures recommended by practitioners," says Dr. Collins, "arise chiefly, I am satisfied, from their treating of every variety of puerperal fever as one and the same disease; whereas, there is perhaps not *any other* which exhibits a greater diversity of character, and even in the same situation at *different periods*." (1)

And so Douglas, who attributes the discrepancies of opinion as to the nature and treatment of puerperal fever to "deficiency of nosological distinction." (2) The same opinion has been more recently advanced and illustrated by Prof. Ferguson; (3) though we object to the statement, as practically important, that "Gordon and Hey saw, for the most part, the peritoneal form; whilst Wm. Hunter, and Lowder observed chiefly the complicated form." The whole bearing of this may be seen by referring to our first volume, pp. 221, 255 — 256, 312, 517, 540, 716, and *notes*. It is manifest that Gordon and Hey were often concerned with the worst forms of congestive puerperal fever. But they are among the most unfortunate in the use of stimulants, and the most successful when they altered their practice to copious bloodletting; "the measure being greater and less limited," in those cases which otherwise "destroyed with more rapidity and greater certainty than the plague." (Vol. I. p. 221.)

"Hinc etiam fit, ut diversi medici, ab iisdem remediis, non eosdem experiantur effectus, sed alii faustos, alii infaustos." (Vol. I. p. 311.)

"I have observed," says Baudeloque, "that when peritonitis is sporadic, the state of the pulse approaches the character assigned by Delaroche, whilst, on the contrary, in epidemic peritonitis, it possesses those described by Hulme, Leake, Doublet, &c." (4)

"The true character," says Dr. Marsh, "of the epidemic puerperal fever seems to be typhus fever, accompanied with local inflammation of the diffuse, or erysipelatous kind. This would also seem to be the true character of the fever which ensues upon wounds received during dissection." (5)

(1) Op cit. pp. 193, 390.

(2) Dublin Hospital Reports, vol. iii. p. 144.

(3) Essays on the Most Important Diseases of Women, 1839. — It should not, however, be forgotten that Professor Ferguson maintains, that "the phenomena of puerperal fever originate in a vitiation of the fluids;" and that, "the *various forms* of puerperal fever depend on *this one cause*."

(4) Puerperal Peritonitis, p. 179.

(5) Dublin Hospital Reports, vol. iv. p. 521.

Notwithstanding these close resemblances, Mr. Travers, (*a*) Dr. Colles, (*b*) and

(*a*) On Constitutional Irritation. (*b*) Dublin Hospital Reports, vol. iv. p. 240.

In this, other late and able writers agree. One, also, is a very fatal disease, whilst the other is comparatively little so. One unnerves the practitioner, the other invites him to decisive bloodletting. Nevertheless, if venous congestion, or active inflammation of the veins exist in any of the cases, a common aspect will be given to the whole by this particular lesion. But, whilst venous congestion thus imparts a general similitude of many diseases to each other, they may be fundamentally different in a constitutional character. We may also say, that the best observers agree that the causes which produce the true puerperal fever will also develope local congestions, and active phlebitis, without the constitutional form of disease; just as the predisposing causes of remittent fever will lay the foundation either of the general affection or of local congestions.

As to the occurrence of common inflammation in puerperal women, when puerperal fever prevails, it is only remarkable that it is not more frequent, considering the nature of the immediate exciting cause, and the facility with which venous congestions pass into common inflammation. In Tonnelle's 222 cases, (p. 458,) what he considered the purely inflammatory affection occurred in only 39; but these were all evidently more or less complicated with venous congestions of some of the great organs of life, especially the liver, the alimentary canal, and the brain. In these cases, as well as in those of lymphatic inflammation, the "inflammatory symptoms were rapidly followed by typhoid, were

others, consider the affection which follows wounds in dissection a disease *sui generis*. The difference is founded in the causes we have indicated, and they may be often complex.

That close observer of nature, Dr. Armstrong, had some misgivings in his views as to the identity of these family complaints, and occasionally points out certain distinctions. Thus, in speaking of dissection wounds, he says that "he has seen several examples of fever from this circumstance, and all these cases have had the character of a *specific* fever, exactly resembling typhus fever, *except that it does not become intermittent or remittent, but always assumes the continued form*." "If I am guided by the facts I have seen, I should say the affection to which I refer arises from the introduction of putrid matter occasioning a *peculiar fever*." (*a*) Just so in phlebitis, epidemical puerperal fevers, fever from putrid injections, &c.; and if we analyze the symptoms with proper care, we shall find manifest signs that contradistinguish them from genuine typhus fever. It is pretended by the humoralists, it is true, that they can generate typhus, and even yellow fever, by injections of putrid matter into the veins. But, they commonly neglect to say that "ink," "bits of stick," &c. when inserted into the veins, will produce nearly the same phenomena, and the same physical lesions. (Vol. i. pp. 397, 519, 540, *note*, &c.)

(*a*) Lectures, on Acute and Chronic Diseases, vol. ii. p. 427.

rarely well pronounced, and sometimes disappeared in a few hours." And so Walsh, (') of other puerperal epidemics.

It is probable, therefore, from what we have hitherto said, that when common inflammatory diseases are rife, congestive puerperal fever will be most intense, not only as it regards the activity of venous inflammation, but its liability to be complicated with the common forms of inflammation, however the latter may sustain modifications from the influence of venous disease, &c. And if it be true that erysipelas depend on inflammation of the capillary veins, or even a sympathetic result, in the form of common inflammation, of venous congestion of the abdominal viscera, the same affirmation may be made of this affection. And, although it must be considered that the predisposing causes of venous congestion, especially the atmospheric, are not identical with those of common inflammation, the latter will give activity to the former when they occur in combination. Nevertheless, it is the tendency of the predisposing causes of venous congestion to produce, also, some of the modified forms of common inflammation, (such as the intermittent ;) and since, as we shall see, there is an easy transition of venous congestion into common inflammation, we may infer, in the general absence of the latter, a mildness or rarity of the former. For this reason, as well as from facts, we do not confide in the statements of some authors, that inflammatory diseases disappear in the congestive form in tropical climates. As venous congestion, however, especially in its epidemic form, depends on malarious causes, it is obvious that common inflammation may prevail independently of venous.

Finally, the true endemic puerperal fever being a constitutional malady, the local congestions, and inflammations cannot be regarded as necessary to the disease, however they may be more or less antecedent to the general explosion, and contribute most to the fatality of the disease. Cases, indeed, of great mildness not unfrequently occur in the course of a malignant endemic, in which it is shown that local congestions and inflammations are nearly or wholly absent. The seat and degree of the venous inflammations, therefore, which may attend this affection may be very various. There may be active phlebitis of the womb, or it may be absent, (p. 350;) but, in most of the severe cases there is hepatic or other abdominal venous congestions, which often impart as great a malignancy as any uterine phlebitis.

(1) On Puerp. Fever, p. 7.

Just as our printer is reaching this part of our work, (April,) we have been obligingly favoured by Dr. Philip A. Davenport with full answers to interrogatories which we had addressed to him in relation to a late endemic puerperal fever at the Bellevue Almshouse, three miles distant from our City Hall, and located on the bank of the East River. Dr. D. resides permanently at the institution, and is an intelligent observer of disease. He was assisted in the post mortem examinations by Alonzo Clark, M. D., who is eminently qualified for investigations in morbid anatomy.

There were 16 cases, of which 14 were fatal ones. Two were attended with uterine hemorrhage; and in these instances there was hour-glass contraction. Ten cases commenced with a *chill*, doubtful in four, and absent in the remaining two. Pain in the abdomen, tympanitis, frequent pulse, head-ache, and thirst, either accompanied or rapidly succeeded the chill. The pain began in the hypogastric, or in one of the inguinal regions, soon became more general, and extended to the loins and back. The pain in the abdomen was constant, but aggravated at short intervals, and increased by pressure or the slightest motions. The face expressed acute suffering; pulse became more frequent; restlessness and thirst increased; stomach irritable; *green vomit* took place in every fatal case. Delirium set in, 6 or 8 hours before death in about half the cases; in the remainder, the mind was entire to the last. Tongue coated white in the centre, red at edges, and became brown in the course of the disease. The *lochia* went on with two exceptions, and were generally of a *natural colour*. The severity of the symptoms, especially at the beginning, did not correspond with the amount and danger of the disease in all the cases, and one might in some instances have been deceived if not closely watching the symptoms. (P. 219.)

The patients were generally treated by venesection, leeches, Dover's powder, warm fomentations, and occasionally blisters to the thighs. Calomel and opium, after venesection and leeching, in 5 cases. Spts. Turpentine in 3 cases, and Burns' treatment by large doses of bark was tried in one case. One of the cases treated by calomel and opium, and occurring 21 days after delivery, recovered. The other favourable, but a mild case was treated only by cups, leeches, and Dover's powder. The patients were bled from the arm; but one case more than once. This patient was bled three times, and lost ℥ xxxviii of blood, and 47

leeches to the abdomen. About ℥xvi or ℥xx were generally taken. The subjects did not faint readily from loss of blood. The disease generally proved fatal in 3½ days.

"Dissections were made in all the fatal cases. In some, a product appearing like lymph adhered firmly to the lining membrane of the uterus, of an ashy-gray colour; in others, a loose shreddy secretion covered the whole membrane, sometimes of a bright pink, in others of a dark red colour. In 2 cases, pus mingled with a fibrinous secretion was discovered near the orifice of one of the fallopian tubes; and in 2 cases it occurred throughout the whole length of the tubes; an opaline fluid in 3. Ovaries natural in 3 cases; enlarged in 3 on both sides, with interstitial deposition of lymph; the same with the left ovary in another. Substance of uterus presented no purulent or other effusion.

"No pus in any of the veins, (Vol. I., p. 540, *note*,) and nothing noticeable in their coats. The iliacs, vena cava, and large uterine veins, were generally stained with blood on their inner coat when the examination was made after 7 hours."

We now come to the cadaverous appearances which are most relative to our purposes. We have seen nothing in the uterine *symptoms* or in the lesions of this organ, or of the foregoing veins, to explain the very fatal nature of this endemic. Whilst it will be evident, from the analogies supplied by other congestive fevers, that any corresponding congestions may be taken as important concurring causes with the febrile action in precipitating death. The facts will require no farther comment than we have hitherto made.

"The *liver* appeared *softened* in 3 cases. In a number of cases the veins of the portal system were crowded with blood, and this extended to the small venous ramifications." It is not stated whether the tissues of these veins were examined. "In most cases the right cavities of the heart were filled with blood. The lungs were usually *moderately* congested. The membranes of the brain were usually congested, and in some cases effusion had taken place." (Pp. 239—267, 279, 289, 311, 312, 313, &c.)

"In all but 2 cases there was an effusion of dirty cream-coloured serum in the cavity of the abdomen; and, in many of the cases lymph was floating in it, or adhering to the peritoneum. The quantity was usually about ℥viii. *It irritated the hands.* (P.

197, *note.*) In all the cases, more or less injection of the peritoneum occurred."

"In some of the cases an accelerated pulse was noticed for many hours before any other symptom developed itself." (1) (Pp. 219.)

The discoveries which have been lately made, in respect to the connection of the lymphatic with the venous system appear, as well as from physiological facts, to evince a close alliance between them, and to open a view of some interest in relation to phlegmasia dolens. We see these vessels, also, apparently possessing analogous endowments, as denoted by their great susceptibility to diffuse inflammation, the analogous phenomena, (2) their exemption from ossification, their valvular structure, &c.

This view of the subject will reconcile the conflicting views as to the seat of phlegmasia dolens, and prepare us for its manifestations in the lymphatic as in the venous system. It will be readily apprehended, how it should be limited in different cases to one system or the other, when we consider by what causes the inflammation is circumscribed when affecting the veins, as stated by Arnott and others. (3) (Pp. 369, 438.)

(1) It is probable that these cases will appear more in detail in the July No. of the New York Journ. of Medicine and Surgery.

(2) The symptoms are compared by Duplay, (*a*) Alard, (*b*) Armstrong, (*c*) Velpeau, (*d*) to those of phlebitis and typhus fever. When arising from dissection wounds "they exactly resembled," says Armstrong, "those of typhus fever." And, again, when similar poisons affect the venous system, there is the same train of symptoms. Morbid anatomy shows an affection either of the lymphatic, as stated by Armstrong, or of some parts of the venous, system. Here the effects of the poison may be propagated from the wound by continuous sympathy, or its invasion of the vascular systems may be mainly through the nervous influence, just as it is, more or less, when aerial poisons induce venous congestions. (Vol. i, pp. 474, 489, 570.) This corresponds with the well-known effect of injections into the circulation of putrid animal matter, and of many other irritants, to develope acute phlebitis and venous congestions. Bichat, reasoning upon "the analogy of structure, and the continuity of the peculiar membrane of the veins and absorbents," concludes that the same vital results would follow irritating injections, in both orders of vessels. (*e*) We have seen, too, (p. 378,) that according to Breschet and others, the lymphatics are susceptible of great enlargements from inflammation. With Müller, and Dumas, Breschet supposes the fluid of the lymphatics to resemble the blood in its lymphatic part. (*f*)

(3) This will account, also, for the greater success of some practitioners in treating

(*a*) In Journ. Compliment. 1833. — (*b*) De l'Inflam. des Vaisseaux Absorbans-Lymphat. &c. — (*c*) Lectures on Acute and Chron. Dis. vol. ii, pp. 102, 147. — (*d*) Sur l'Inflam. des Absorb., &c. — (*e*) Gen. Anat., vol. ii, p. 119. — (*f*) Sur Sys. Lymphat., &c., p. 223, &c.

This principle, as we have seen, holds to a remarkable extent, though not as it respects the inferior degrees of inflammation. But, since there are constitutional differences between the veins and lymphatics, it is a highly probable induction that the foregoing principle prevails more extensively in its joint relation to the two orders of vessels. Tonnellé, (1) Duplay, (2) Graves, (3) however, have shown that lymphatic phlegmasia dolens and uterine phlebitis occur simultaneously in the same individual.

It cannot be that Lee, Velpeau, Davis, Arnott, and others, should be mistaken in their facts, and that cases of phlegmasia dolens should not have been constituted by phlebitis; nor should we be disposed to renounce our own experience. On the other hand, it is as little to be questioned that White, Trye, Ferriar, Hull, Houston, James Johnson, Dewees, Graves, &c., have often met with instances where the lymphatic system was the seat of inflammation.

Passing to the consideration of other well-marked congestive diseases, we find a striking illustration of our whole ground in purpura hemorrhagica. Here there may be an almost universal affection of the veins, and here we see nature coming to its relief in excessive effusions of blood; whilst the very process by which its elaboration is effected is a proof of the active condition of the disease. Dr. Hannay, as we have seen, has answered the demands of morbid anatomy, whilst Parry, (4) Kingsley, (5) Brown, (6) Latham, (7) Marsh, (8) Harty, (9) Dawson, (10) Seymour, (11) MacLeod, (12) Chambers, (13) Stoker, (14) Mills, (15) Belcher, (16) Mack-

phlegmasia dolens, than is enjoyed by others, when the same means are employed, — phlebitis being at all times more dangerous than inflammation of the absorbents.

(1) Des Fievres, &c., in Archiv. Gén., 1830.

(2) Journ. Hebdom. de Méd., Mai 29, 1830.

(3) In Lon. Med. Gaz., Aug., 1837, p. 747.

(4) Elements of Pathol., &c., p. 156, and Edin. Med. and Surg. Journ., vol. v, p. 7. — (5) Lon. Lancet, No. 199; and Med. Chir. Rev. vol. xii, p. 207, 1828. — (6) Med. Chir. Rev., vol. xv, p. 237. — (7) Ibid., vol. xiii, p. 213, and Lon. Med. Gaz., vol. i, p. 544. — (8) Dublin Hospital Reports, vol. iii, p. 307. — (9) Edin. Med. and Surg. Journ., vol. xxxiv., p. 57 — 74. — (10) Nosolog. Practice of Physick, p. 249. — (11) Lon. Med. Gaz. Dec. 17, 1836, p. 394. — (12) Ibid., Feb. 4, 1837, p. 697. — (13) Lon. Med. and Phys. Journ., vol. lvi., Jan. 1827. — (14) Patholog. Obs. on Dropsy, Purpura, &c., 1824, pp. 21, 159, &c. — (15) On the Utility of Bloodletting in Fever, pp. 74, 156. — (16) Med. and Phys. Journ., vol. 53, p. 175, 1825.

intosh, (1) Combe, (2) Fairbairn, (3) Bateman, (4) Bree, (5) Pretty, (6) Gardner, (7) Gairdner, (8) Macmichæl, (9) Rush, (10) Kift, (11) Darwall, (12) Magee, (13) and others, have done a more valuable service in expounding the nature of the disease by its symptoms, the buffing of the blood, and in showing by the effects of its abstraction, that the corresponding effort of nature is one of her vital attempts to remove inflammatory action." (14)

(1) Elements of Pathol. und Practice of Physick, vol. ii, p. 241. — (2) Edin. Med. Journ., No. 66; and Lon. Med. and Phys. Journ., vol. 56, 1826. — (3) Edin. Med. Chir. Trans. vol. ii, p. 161. — (4) On Diseases of the Skin, Art. Purpura. — (5) Bree, in Med. and Phys. Journ., vol. xxi, p. 321. — (6) Plumbe, on Diseases of the Skin, s. 5, c. 1, p. 283. — (7) Glasgow Med. Journ., April, 1833. — (8) Edin. Med. Chir. Trans., vol. i, p. 671. — (9) Lon. Med. and Surg. Journ., vol. i, p. 468. — (10) On the Bilious Fever, pp. 73, 270; and *Note* to Cleghorn's Diseases of Minorca, c. 7, p. 183; where he says, "hundreds have perished in smallpox from the petechiæ being supposed to mark a putrid, instead of a highly inflammatory disease." — (11) Edin. Med. and Surg. Journ., vol. xxiii, p. 71. — "The morbid appearances clearly point out that a depletory plan of treatment, carried to a greater extent than above adopted, would have probably afforded a better prospect of success." "Blood presented a firm, sizy crassamentum." — (12) Ibid., vol. xxiii, p. 53. — Treatment by purgatives. Excreted blood coagulated. View of pathology not expressed. — (13) Ibid., vol. xxiv, p. 307. — Spirits of turpentine employed upon the ground of its "acting almost as a specific in enteritis, peritonitis, colick, and certain inflammatory affections of the abdominal viscera." (?)

(14) A broad statement is made by Plumbe, (*a*) that "there are not the usual evidences of inflammatory action afforded by the appearances of the blood in three out of four cases when bloodletting has been had recourse to in purpura;" and to show that inflammatory action is not the pathological state, and that bloodletting is not a proper remedy, "*debility*," it is said, so rules the capillary vessels that they "are ruptured by the ordinary force of the circulation," although "the strength of the system at large remains unimpaired"! (Vol. i, p. 371—384.) With the same view, our author refers to "Dr. Johnson's Med. Chir. Journal, (Review,) which abounds with the details of cases of the disease." Accustomed to rely upon such references to facts, we were not a little startled by this declaration, and accordingly examined that Review, by "taking the index of the different volumes for our guide," as recommended by Dr. Plumbe. The result of this inquiry is, that, with the exception of two or three cases, the doctrine of inflammation, and the depleting system, are fully sustained. We refer the reader particularly to vol. vii., p. 284; vol. x., pp. 62, 200; vol. xii., p. 207; vol. xiii., p. 213; vol. xv., p. 237; vol. xxi., p. 379; vol. xxiii., p. 154. These are the principal cases. Plumbe recommends the purgative system. How does it strengthen "the debilitated and tender coats" of the blood-vessels? What induces such a fragile state of the vessels? How shall we explain the hereditary cases of purpura described by Mr. Murray? (*b*) Here is a case in which the constitutional effects of mercury were the evident cause of relief. The affection was mainly confined to the legs and feet, "which were *quite tender to the touch*." "During the existence of the ptyalism, the petechiæ nearly disappeared." "As the symptoms of ptyalism declined, the petechiæ returned." "During the whole of the salivation, the hemorrhage from the mouth and gums was very slight." "Attention must be

(*a*) On Diseases of the Skin, s. 5, c. i, p. 258, *Note*, 284, &c. — (*b*) In Edin. Med. and Surg. Journ. vol. xxvi., p. 33.

In this country of venous congestions, we have frequently met with purpura hemorrhagica at all ages, and, in most instances, have treated it succesfully by bloodletting and cathartics; employing the former especially at the early stages. We have always found the blood buffed, and more or less cupped, if drawn in a full stream into a wine-glass. We believe tonics and stimulants to be always fatal in America, unless in the advanced stages; and even then they are generally prejudicial unless the affection be complicated with intermittent, or typhus fevers. When we come to the consideration of spontaneous hemorrhage, we shall probably find some important proofs which concur with what we have already stated in different places, as to an active, inflammatory condition of the veins being the immediate pathological cause of purpura hemorrhagica. Nevertheless, if analogy were permitted to have its proper weight, it appears to us that the proofs which we have offered as to the universal dependence of morbid venous congestions upon inflammation, either active or sub-active, or an approximating condition, of the veins, should be conclusive of the existence of such condition in p. hemorrhagica.

We shall, however, abstract from what we purpose saying of capillary hemorrhage, a case which we have lately witnessed, and arrange it in this place as an illustration of the dependence of the hemorrhagic effusion in p. hemorrhagica, upon purely vital action. (Vol. I. p. 456.) It occurred in a gentleman, Mr. Oliver Malaby, who resides in this city. Having passed a few months on the alluvion of the Mississippi, on his return hither he was attacked with an irregular intermittent fever. In its early stage, a general extravasation of blood took place over about one half of the posterior part of each humerus, occupying in each arm the central part. The inferior portion of the lobe of each ear was distended with extravasated blood, and just at the middle of each helix was another narrow oblong extravasation of about half an inch. A similar appearance existed in the

paid to its cause and pathology." (a) (Vol. i., p. 540.) Did the mercury act as a strengthener to the vessels? Childhood is subject to p. hemorrhagica. At this period of life the capillaries may be supposed to be more fragile than at middle age. Why, then, being so greatly liable to cerebral congestions and inflammations, does not cerebral hemorrhage more frequently occur, if depending on ruptured vessels? The only intelligible explanation consists in the absence of that modification of the vital properties upon which capillary hemorrhage depends, and the philosophy of which is stron ly exemplified in the hereditary predisposition to this affection.

(a) Dunglison's American Med. Intelligencer, Dec. 16, 1839.

conjunctiva at each inner canthus of the eye. There was not a trace of any effusion upon any other part, or from any of the internal membranes. The parts where it existed were very tender to the touch. There were obvious indications of hepatic congestion. A large bloodletting immediately arrested the effusion, and improved, at once, the strength of the patient, who soon became convalescent. The symmetrical peculiarity of this case is worthy, also, the consideration of the humoralist. (See pp. 267, 472, *note*.) A variety of isolated facts are scattered through the records of medical observers, which, if duly analyzed, would dissipate all doubt as to the pathology of venous congestions and the hemorrhages to which they give rise. As an example of such as require some exercise of thought, and an enlarged view of physiology and pathology, here is one from Dr. Mitchell, who says that the lungs in yellow fever "were sometimes *full* of *black or livid spots*, and that *on* these spots were to be seen small *vesicles or blisters*, like those of *erysipelas*.(1) (P. 300.)

And here a coincidence to which we have frequently adverted, as showing the sympathetic development of venous congestions in remote parts, is worth observing in relation to purpura. It is the opinion of Harty, (2) Plumbe, (3) and others, that this affection depends upon hepatic and gastric congestion. Bateman thinks that "all the symptoms tend to excite a suspicion that some local visceral congestion or obstruction is the cause of the symptoms in different instances." (4) M. Biett relates a case in confirmation of this opinion, where the tongue was twice its natural size, and of a deep blue colour. So, also of scurvy. "It is fair to presume," says Plumbe, "that, as in purpura, the direct exciting cause must be hepatic congestion," &c. The great disposition of the liver to take on venous congestion is especially manifested in tropical countries, where, according to Annesley, "there is seldom seen a case of disease in which, upon dissection, the liver and spleen are both sound." (5) Their general condition, especially if the subjects die early, is that of venous congestion.

If we turn our attention to scurvy, we may trace other links which connect the various species of the great family of venous congestions. In the London Medical Journal for 1809, and

(1) Op. cit. p. 75. (2) Edin. Med. and Surg. Journ. vol. xxxiv. p. 57.
(3) Diseases of the Skin, e. 5, c. 1. (4) Synopsis, &c. p. 113.
(5) On the Diseases of India.

therefore before the true nature of p. hemorrhagica was at all suspected, are some ingenious editorial remarks intended to show that purpura and scurvy have a near affinity; which, indeed, was the opinion of the earliest writers upon the latter affection. Eugalenus, (1) Van Rozen, (2) Sydenham, (3) Hoffmann, (4) Duncan, (5) Lister, (6) Plumbe, (7) Willan, (8) Good, (9) Evanson & Maunsell, (10) Graves, Kerr, and many others, more or less identify the affections.

"I consider purpura," says Willan, "under all the forms described, as pertaining to scurvy."

"Salted provisions, together with confinement and unwholesome air," says Dr. Graves, "render the children of tradesmen and petty shopkeepers of Dublin peculiarly liable to purpura hemorrhagica, in a form which merits the name of land scurvy." (11)

"Mr. Kerr observes that, "a comparison of the *symptoms alone* would seem to warrant the conclusion that they are constituted by diversities of degree only of the same morbid condition; and that their causes are nearly the same." "It is often impossible to recognise the distinction between them." (12)

Dr. Green thinks them "different diseases; for, purpura is still frequently met with, whilst the assemblage of symptoms denominated scurvy is very rarely seen indeed, and then only under particular circumstances." (13)

The latter opinion we hold to be correct, though in a more limited sense than our author implies. We apprehend, however, that an important part of both consists in venous congestions, and that, so far, the difference is constituted by peculiarities in the modifications of venous disease which arise from differences in the predisposing causes. There is the same alliance as between these diseases and beriberi. (P. 314.) The affinity is of the same nature, and grows out of analogous differences in causes, as that which subsists between the *constitutional* elements of plague, typhus, remittent, intermittent, yellow, and other idiopathic fevers. Still the analogy appears often to be very great; as manifested during the prevalence of scurvy, when many individuals, even at sea, are afflicted only with the common symptoms of p. hemorrhagica.

At other times we have diseases, occurring epidemically, which appear to form an intermediate link between scurvy and purpura.

(1) De Morb. Scorbut. — (2) De Purpura Chron. Scorbut. — (3) Op. — (4) De Scorbut. — (5) Med. Cases from Public Dispensary at Edin. &c. 1784. — (6) Exercit. de Morb. Chronic. Ex. 5. — (7) On Dis. of Skin, s. 5. c. 1. — (8) Diseases of Skin. — (9) Study of Med. — (10) Dis. of Children, p. 276.

(11) Dublin Journ. of Med. & Chem. Sci. 1833.

(12) Cyclop. of Prac. Med. Art. Scorb.

(13) On Diseases of the Skin, p. 215.

Thus, in the late epidemic scurvy in the province of Queen Adelaide, Dr. Delmege remarks that,

"It in no way resembles the true scurvy by which sailors are sometimes attacked; and that it also differs a good deal from the pupura hemorrhagica of Bateman and Willan." Dr. Malcolm, after detailing a case, states that "it was called a case of purpura, which appears to be a modification of scorbutus; and the report of the effect of the depletory treatment (*bloodletting*) *stamps its superiority* over the tonic and stimulating plan." (1)

Again, Mr. Ribes has frequently found the capillary and larger veins with the characteristic marks of inflammation in the scorbutic old subjects at the hospital of Invalids. The Germans, among whom the scurvy was first noticed in 1486, considered it an inflammatory disease; whence their name for it, *scharboock*, and thence scurvy. (2) Looking at the results of venous inflammation when artificially excited by irritating injections, we find that petechiæ, and large extravasations of blood, are a frequent phenomenon. In Cruveilhier's experiments, the veins were greatly dilated, and the inflammation active. (3) And, considering the constitution of man, should we not have an universal purpura hemorrhagica, so far as the manifest phenomena are concerned, were he to be made the subject of a like experiment? This consideration brings us to the coincident attendants of congestive fevers; and, as an example, we may say that of 194 subjects of typhus fever, at Valterra in 1817, Raikem and Bianchi state in their Report, that 156 exhibited petechiæ. And so in the cholera asphyxia, "the general anatomical characters, as shown by preparations of each organ, were accumulations of blood, ecchymosis, called by the French, apoplexy," (4) &c. (See anatomical characters, p. 303 — 310.)

Having again adverted to beriberi, we may say that Dr. Hunter supposes that this affection and scurvy are the same disease; whilst Dr. Christie admits that there "is a similarity in their causes." (5) Mr. Heward, of the Madras army at Ava, states that beriberi

"Does not appear to possess any other striking peculiarity in its character but the extreme fatality which attends it; and which may reasonably be accounted for from the combination with *scurvy*." (6) And Marshall, "there is a

(1) Dr. Murray's Annual Report from the Cape of Good Hope, for 1836.

(2) Boerhaave, Aph. 1148.

(3) Anat. Pathologique. Livrais. 12, p. 4, &c. See also Plumbe on Diseases of the Skin, s. 1, pp. 274, 260, *note*.

(4) Mackintosh, in Report, &c. *ut. cit.*

(5) Diseases of the Lascars, p. 82.

(6) Report, Sept. 1824.

much greater degree of connection between the two diseases, than is at present supposed." (1) Dr. C. Rogers of British India, only objects to their similarity because "scurvy is cured by acid fruits, &c. whilst they fail in beriberi." (2)

There is clearly a general resemblance amongst some of the most important vital phenomena of all the affections which we have hitherto considered, however far the extremes of certain idiopathic fevers, and the most simple and purely local forms of venous congestions, may be removed from each other. The latter we assume as the test, and carry it through all the gradations till we ascend to the plague itself; and, calling anatomy to our aid as we go along, we find throughout our entire analysis various organs affected with venous congestion. There is also ample proof that the predisposing causes of plague, yellow fever, typhus, &c., may develope either simple venous congestions in certain organs without fever; or, they may, in other cases, establish a full predisposition to idiopathic fever, along with the local affection. (3) Here, in the congested veins, we believe, is the secret of the great similitude, and by no other possible construction can it be explained.

But, whilst we would repudiate an attempt to establish an exact identity amongst any of the various affections we have considered, it is manifest that they possess some important pathological condition, more or less modified in common. Thanks to minute anatomy, which has so well contributed in these instances to put at rest what the vital phenomena, the results of treatment, the dilatation of the veins in connection with its obvious dependence upon vital action, and the plainest effects of irritating agents upon the venous system, the simple type as seen conspicuously in varix, and the whole force of analogies as derived from the history of common inflammation, must have established without the auxiliary aid.

"Mille morborum causæ, et suarum symptomatum naturam perturbant, ut dificile fit veritatem investigare, nisi complexus horum omnium sagaci rationis usu perpendatur, et illustretur." (4) (Vol. I. p. 512.)

Morgagni and Boerhaave, who analyzed so masterly the remote causes of disease, remark that, a disease may be very simple as to its essential pathological cause, and yet be very complex;

(1) Diseases of Ceylon, p. 210. (2) Thesis, Edin. 1808.

(3) Mr. Malcolmson thinks that beriberi is generally a local affection, but often complicated with idiopathic fever. (*a*)

(4) Baglivi, Op. p. 30.

(*a*) On Beriberi, p. 39.

and this without any reference to humoralism. It is to the liability of the vital powers to various alterations, though often nearly allied, and to their natural modifications in different tissues, that we must look for the diversities which distinguish idiopathic fevers, the various conditions of common inflammation, and venous congestions, and come at the reason why it has been said that scurvy simulates many other diseases, (Vol. I. p. 667,) or why, as observed by Kirkland, "under the name of puerperal fever our modern writers may comprehend a thousand disorders." (1)

We have variously spoken of the modifications of venous congestion in respect to its pathological cause, as determined by age, habits, the peculiar constitution of each part, the series of veins which may be affected, whether the visceral or cutaneous, the large or the capillaries, and especially by the remote causes. As introductory to the following remarks we may say that Maygrier, and others, have shown that the mode of cerebral congestion, though apoplectic, to which pregnancy predisposes, differs from that which results in apoplexy in others; and that this distinction is not only justified by the phenomena, but by a difference in the curative means, Cheyne, Haygarth, and others, look upon the apoplexy which supervenes on gout as different from other forms of the disease. Greater distinctions are seen in intermittent apoplexies, not only in relation to symptoms, but to some of the curative means. We have collected upwards of thirty supposed varieties of apoplexy, which have been thought to derive certain peculiarities either from external causes, or from sympathetic determinations upon the brain. (2) In all these varieties, dissections generally show a venous engorgement of the brain. In our 15th section, we shall speak of a fact which denotes a specific nature of the remote causes of the true typhus fever, and how that affection is clearly distinguished, independently of the other characteristics we shall have stated, from other fevers. And, in our appendix on *Cold*, we shall endeavour to show how inflammatory affections are more or less modified in

(1) On Child-Bed Fevers, p. 46, 1774.

(2) Sauvages says of them, "fatcor illas principiorum diversitates in apoplexia actuali curanda, parvi esse momenti, ob lethalitatem hujus morbi; verum, iterum dico ad *præventandum* apoplexiam *causarum* proegumenarum varietas est attendenda." (*a*)

(*a*) Nosolog. Method. t. iii. p. 454.

their pathological essence by different degrees of that remote cause.

That what we have now said may be the more easily appreciated, we may advert to the differences which arise from the different modes of operating upon varix, whether by the knife, or caustic, or ligature, or pins, &c. or even whether there be a free incision by the knife or a subcutaneous one. And that this may be the better comprehended, we would refer the reader to the discriminating observation of Dr. Johnson as to the differences in inflammation which grow out of different kinds of wounds. (See Vol I. p. 610, *note.*) And then we have, by way of illustration, those peculiar conditions which consist in varix, and hypertrophy, and those modifications which give rise to pus, or lymph, or phlebolites, or carcinomatous, and other formations within the veins. Each specific product denotes some peculiarities, in each instance, of the inflammatory action. And may we not gather something as to our principle, from the various limitations of phlebitis,—stopping where a vein terminates in a trunk; or, if passing on, abruptly subsiding at this point into a low state of inflammatory action, and thus sometimes pervading in this low degree the whole system of the larger cutaneous veins. Or, again, affecting exclusively the capillary veins of the skin, but, at another time, though more rarely, stretching into the larger veins in the most violent form of phlebitis; or, as in varix, and ordinary "phlebitis," affecting the larger veins, but leaving the capillaries intact; or, as in venous congestion, beginning probably in the capillaries of the organic viscera, and pervading the whole venous tissue of the organ?

We come now to consider other modifications, and to regard certain remote causes which determine venous congestions of a particular organ; and since this organ is the brain, the special proof which we have already offered of the dependence of morbid accumulations of venous blood upon venous disease in a part where the force of gravity destroys all mechanical tendencies to such a result, is prima facie indicative of the truth of the conclusions at which we shall arrive. We shall find, too, in the particular and well marked local influences of these morbific agents a significant illustration of the principle upon which certain malarious agents develope hepatic congestions more frequently than of other organs, and why gastric influences may speedily lead to cerebral, or other congestions, &c.

We now refer to narcotic, alcoholic, and other analogous poisons. In direct poisoning by these agents, it is the brain, and almost the brain alone, that manifests any morbid lesion; and where the passions operate with rapidity, the visible lesion consists mainly in a turgescence of the cerebral veins. Nevertheless, the primary effect consisting in an alteration of the vital properties, and doubtless involving the general properties of the brain, the intensity of the morbific cause may be such as to extinguish life before the vessels manifest any influence from the morbific impression. Here, too, the lesions of function are principally manifested, and the phenomena are various according to the specific nature of the morbific agent. Besides our every day observation of these facts, countless experiments have been made to illustrate the subject. And here our own species have made an offering to science which is generally the privilege alone of inferior animals. We allude particularly to the 22 Leipzig physicians who poisoned themselves with the cherry-laurel, "the whole action of which was concentrated upon the brain," in consequence, as supposed by Professor Jörg, of "a turgescence of its vessels." "The primary action of digitalis, camphor, and musk, was powerful upon the brain."

But, what appears, especially, to show the accuracy of our conclusion that the turgid state of the veins is owing to a diseased condition, is its determination upon particular and different parts of the brain by different narcotics. And, since the experiments had no reference to our inquiry, we may employ them without objection. Thus, Flourens found, after numerous trials, that opium always produced venous congestion in the cerebral lobes alone, and,

"Que l'action spécifique de l'opium sur ces lobes reproduit exactement tous les phénomenes qui dérivent de leurs lésions méchaniques." (1)

It is also important to say that this was sometimes attended with a partial effusion of blood, or it extended over the entire lobes. Again, Rush affirms that "the remedy for poisoning by opium is copious bloodletting. In two of the cases it was very copious. In one of them the blood exhibited an inflammatory crust." Four patients were cured in this manner. (2) Here, then, is a coincidence betwixt the vital signs, morbid anatomy, and

(1) Recherches Expér. &c. du Système Nerv. p. 254.
(2) New-York Med. Repository, vol. v. p. 124.

the effect of remedies. (Vol. I. p. 568.) Dr. Rush in referring again to these cases, says,

"It has been proved by many facts, that opium, when taken in an excessive dose, acts by inducing a similar state of the system with that which is induced by the *miasmata* which bring on *malignant*, and inflammatory fevers." "It is probable, from the uniformly stimulating manner in which poisons of all kinds act upon the system, that bleeding would be useful in obviating their baneful effects." (1)

The action of belladona, in Flourens' experiments, was chiefly confined to the tubercula quadrigemina. It produced "a deep coloured venous engorgement" of the part, and phenomena analogous to injuries of that part. There was also extravasation of blood. Here the "observations" of Andral "On Hemorrhage of the Cerebral Hemispheres" (2) should be consulted. But, it should be borne in mind that these were cases of cerebral disease dependent, perhaps in every instance, upon a variety of remote causes and sympathetic influences.

In a fatal case of poisoning by the extract of aconite.

"On dissection, the veins of the cerebral membranes were highly congested, and the substance of the brain, when divided, exhibited numerous points of dark coloured blood." Other organs were also "gorged with black blood;" the venous system having been extensively affected in this case. (3)

Alcohol, in certain doses, affected in the same way, and exclusively, the cerebellum. Flourens also demonstrated that parallel results arise from camphor, and many other substances of an analogous nature.

We are far from supposing that precisely the same results would happen to man, but, doubtless sufficiently so to sustain our principle. Nor would a failure of the precise local effects in any respect defeat the principle, since if shown in the former instances, the general conclusion is equally substantiated. Nevertheless the fact is only of subsidiary importance. (Vol. I. p. 474—515, 568—581.)

In respect to the sanguineous effusions, the fact is coincident with our theory of such effusions, (Sec. 16, and Vol. I. p. 371,) as well as corroborative of our theory of venous congestion.

"Le point où elle s'effectue est *toujours* le sinus veineux correspondant à la

(1) Med. Inquiries and Observations, vol. iv. p. 357.

We have stated a remarkable instance of the effects of bloodletting in a case of poisoning by arsenic in our vol. i. p. 328, *note*.

(2) Medical Clinic on Diseases of the Brain, Phila. 1838, *second order*, p. 133.

(3) Med. Chir. Rev. Lon. Jan. 1840, p. 240.

partie cérébrale affectée, partie dont elle dénote ainsi, à l'extérieur, l'engorgement ou infiltration intime." (1)

In a case of poisoning by strychnine, "on opening the vertebral canal, a great quantity of thick fluid blood escaped. The plexuses of the spinal veins, and of the pia mater were *highly congested;* and, on removing the membranes, some serum escaped from beneath them. The upper part of the spinal marrow was exceedingly softened. The *brain* was *not altered in any way,* and the *membranes* were healthy," &c. (2)

Where death from acrid narcotics has been gradual, the brain has been found softened by Orfila, Lallemande, Ollivier, Andral, and others. And here are other facts of the foregoing nature, accompanied with opinions, and analogies, which go to our common purpose:

"L'opium agit en *poussant* le sang vers la tête, dit Stoll; en *retardant* la circulation du sang dans la tête, dit Lazerme. Les poisons narcotiques, dit Portal, ainsi que le méphitisme, n'occasionnent-ils pas une pléthore locale dans le cerveau, qui est bientôt *suivie d'inflammation, indépendamment de leur action délétère sur les nerfs* et sur les muscles, qui détruit la sensibilité des uns et l'irritabilité des autres." (3)

The operation of carbonic acid, and mephitic gases, appears, like the narcotic poisons, to be directly upon the vital properties, and that, like them, they affect the venous system of certain parts, particularly the brain. The patients, on recovering, according to Bichat,

"Affirm, in almost all instances of poisoning by the vapour of charcoal, that the first thing they were sensible of, was more or less pain in the head." And, what shows the profound influence of this and analogous causes upon the brain is the fact that, "there are many examples of persons, who, after escaping from the pernicious effects of the vapour of charcoal, have afterwards been subject to paralytic affections, and loss of memory." (4) So, Nysten, Collard, (5) &c.

In all these cases, venous congestion of the brain is the characteristic lesion. The following is the latest example we have seen:

"The appearances before death were those of a person labouring under apoplexy, with constant spasms of a tetanic nature," &c. "Scalp thin and *bloodless;* vessels of dura mater congested, and all the sinuses full of blood. (P. 267.) General fulness of the vessels of the pia mater. Basilar artery, and the arteries of the corpus callosum, were full of coagulated blood. Arachnoid transparent; sub-arachnoid effusion in the depending parts. One drachm of clear fluid in the lateral ventricles. Choroid plexus very pale. *Cerebral substance everywhere of normal* appearance and consistence, presenting large, but not numerous, bloody

(1) Flourens, Recherches Expér. du Système Nerveux, &c. p. 242—266.
(2) London Lancet, Jan. 27, 1838.
(3) Granier, Trait. sur l'Apoplexie, p. 143.
(4) Physolog. Researches, p. 249. Rech. sur la Vie, &c. pp. 377, 454.
(5) Bulletin des Sciences, 1826.

points in the posterior lobes of the hemispheres. About six drachms of fluid remained in the base of the cranium after the brain was removed. *Both lungs perfectly free;* no fluid in pleura; *lungs hardly at all contracted, pale* and emphysematous anteriorly, of a light purple colour posteriorly, everywhere crepitant; *tissue not infiltrated*, light, and of normal consistence." (P. 249—255.)

"Pericardium and heart *perfectly healthy, not contracted; some* dark grumous coagula filled the right cavities, mixed with a small fibrinous clot; the left ventricle contained a very small quantity of very dark clot. The blood contained in the large vessels was coagulated and dark."

"Small intestines were perfectly healthy throughout. Mucous membrane of cœcum of a deep purple colour, as if *ecchymosed*, and of normal consistence and thickness. *Elsewhere* the membrane was *pale*, and perfectly healthy."

"*Liver of perfectly healthy appearance and colour. Gall bladder full.*

Kidneys dark coloured from congestion; otherwise perfectly healthy. Bladder empty, and firmly contracted." (1)

We have marked certain parts of the foregoing quotation to avoid the necessity of comment. When duly considered in their proper connection, and in relation to what we have said of the special and independent localities of venous congestions, the causes, remedies, &c., we cannot but think that our conclusions must prevail. We may say, however, that in the foregoing case, as well as in a multitude of other coincident ones, it is fully apparent that the cerebral congestion had no dependence upon an interrupted circulation in the lungs, or liver, or upon accumulated blood in the heart. (P. 230—267.)

Since the foregoing was written, we have met with Dr. Bird's observations and experiments in relation to this subject. He regards carbonic acid as a specific poison, and that it produces death by determining *venous congestion* of the brain, or an apoplectic condition. (2) But, this is only a consequence. What is the essential pathology? This is the great question; for without its knowledge, practice must be wholly empyrical.

The virus of hydrophobia sometimes falls with much force upon the venous tissue of one or more organs. Morgagni has a case manifestly of iliac phlebitis from this cause, complicated with congestions.

"The lungs were gorged with coagulated blood, and appeared as if gangrenous at the posterior part. The *veins* of the *brain* were excessively turgid, and the *iliac veins* were enlarged to the diameter of the *small intestine*." (3) (See a case by Marshall, in APPENDIX I.)

In cases of death from submersion, strangulation, a vacuum, &c., the turgescence of the veins is more or less universal. This

(1) Hore, in Lon. Med. Gaz. January, 1839.

(2) Guy's Hospital Reports, April, 1839.

(3) Epis. 8, a. 27.

phenomenon is owing to other causes than such as affect the venous tissue; and these appear to have been explained, in part, by Bichat,(1) and in part by Reid,(2) Williams,(3) Alison,(4) Edwards,(5) Kay, &c. (Pp. 38 — 43, 48.) It is only a higher degree of that general injection of the venous system which we find in all cases after death. But it takes place, in a measure, during the whole process of asphyxia; and here it is that the peculiar combination of causes operates which are stated by Bichat and others. Nevertheless, whatever may be the direct cause of the venous engorgements in these cases, should the subjects survive, the accumulated blood, by the stimulus of distension, may induce the true pathological condition of venous congestion; and then will follow a different train of symptoms, requiring a very different treatment from the antecedent.

The rapidity with which venous congestions are produced by prussic acid, the narcotic poisons, &c. so far from being an objection to our pathological views, appears to be strictly confirmatory of their accuracy. Any hypothesis which may suppose another cause than that of some morbid change in the venous tissue, must fall at once from its complete inadequacy; especially when viewed in the various relations which we have hitherto indicated. Besides, we have presented abundant analogies in our Humoral Pathology where absolute disorganization has followed within a minute after the application of the morbific cause. Still, in large doses, these agents may extinguish the vital powers so instantaneously, (which, in all cases of disease undergo the first and indispensable change,) that no apparent lesion of structure may arise. (Vol. I. pp. 471 — 473, 488, 563 — 575.)

The dissimilarity of the functional disturbances that arise from the foregoing poisons is owing, in the first place, to the specific nature of the substance by which the cerebral powers must be supposed to be more or less peculiarly affected; and the venous disease must always sustain some corresponding modifications. This, we apprehend, is the ground of distinction among idiopathic fevers, &c. In the former case, also, more or less must be ascribed to the particular parts of the brain upon which

(1) Rech. sur la Vie, &c. part 2, c. 6, p. 341, c. 9, p. 428 — 456.
(2) Edin. Med. and Surg. Journ. vol. xvi. p. 387.
(3) Lectures on the Physiology and Diseases of the Chest, lec. 3.
(4) Exper. on Respiration, &c.
(5) Influence of Physical agents on Life, &c. *passim*.

the action of particular poisons may expend its force, and to the various influences that may be determined in a direct manner upon other organs. In cases, too, of great cerebral congestion, more or less is doubtless owing to the pressure of the accumulated blood upon that organ.

In respect to alcohol, it may be objected, perhaps, that, in delirium tremens, opium, sometimes in large doses, is a curative means, whilst in an excessive quantity, at other times, it induces venous congestion of the brain. We might dispose of this apparent difficulty at once by analogies in relation to all other poisons which are employed in the treatment of disease. In their curative doses they may overcome the very disease which they excite in greater quantities. This is even true of bloodletting itself. (P. 205, Vol. I. pp. 239 — 248, 253, 259 — 262.) But, the very fact that opium is often curative in delirium tremens is a proof that the affection which it overcomes is of an inflammatory nature, since it is well known that it has been largely employed in full doses, after bloodletting, to complete the cure of pneumonia and other inflammations: whilst, as we have seen, when opium and other narcotic poisons are carried so far as to produce its poisonous effects, Dr. Rush pronounces bloodletting the best remedy. (P. 480.) If opium cure the venous congestions of the brain, which are produced by alcohol, it will generally aggravate those from most other causes, far more so than it will inflammation of other tissues. This exemplifies the principle to which we have before referred, that every distinct morbific agent is liable to alter the vital powers in a way peculiar to itself, though there may be common modes of action in many of the cases, and common principles of treatment. Thus, too, in the venous congestions which attend intermittents, the bark may cure even when apoplexy, depending on cerebral congestion, is present; whilst it will aggravate congestions which depend on other causes. But, bloodletting is also a cure, both, for these intermittents and for delirium tremens. The general principles, we have said, are the same in all, the general character of the local pathology being the same. Thus, in delirium tremens there is great irritability at the nervous centre, and opium, in full doses, operates as a sedative. (1) Opium will produce its deleterious effects, as is obvious enough, only when it exceeds the

(1) We apprehend that opium operates as a sedative in large doses, whilst in small, its effect is more or less analogous to stimulants, just like electricity.

capacity of the vital properties to be benefited by it, and the quantity, of course, will depend upon the state of those properties. It is then curative, as in common inflammations. It therefore often happens, from modifications which arise in delirium tremens, that opium in any quantity is deleterious. These modifications generally consist in some coexisting inflammation of the general tissue of organs, or, more frequently, of venous congestion of the liver. In the latter case, especially, opium may aggravate the hepatic congestion, and thus indirectly the cerebral. This arises from differences in the natural condition of the vital properties of the brain and liver, and corresponding differences in the same condition of the disease. Alcohol, too, may have been more a predisposing cause of the congestion of one organ than of the other, and may have operated more extensively in modifying the general properties of one than of the other.

We are aware, too, that it has been said that "eight ounces of brandy has, like a charm, calmed in a moment all the distressing symptoms of mania a potu."(1) We doubt not, that in some peculiar conditions this practice may succeed, just as we shall see that it may in certain cases of common inflammation. For farther comments on this subject we refer to our first volume, pp. 221, 239—248, 259—263, 273—278, 293, 296, 302—304, 311—324, &c. Perhaps, however, we ought not to neglect the authority of the distinguished Stokes to the same effect; who goes so far as to say, "this seems to be the best mode of treating the disease," where the subject "is in the habit of taking wine and spirituous liquor, every day, in considerable quantities," and where "the delirium is produced by the want of the customary stimulus." What! Delirium a potu produced in one instance by an excess of liquor, and in another by the want of it? And is it true that "the frequent error" of treating the disease in habitual, or in casual drunkards, "even by persons whose authority is high in the medical world, arises from the system of prescribing for names and not for things."?(2) Is not the essential predisposing cause of this affection the same in all the cases? The common nature of the remote and pathological causes must generally determine a coincident mode of treatment. Where is the remarkable difference in the vital phenomena to justify opposite extremes of practice? Aye,

(1) Dr. Anderson's Hospital Report, in American Med. Intelligencer, vol. ii. p. 111.

(2) Stoke's Theory and Practice of Physic, p. 43.

too, what says morbid anatomy, which is to conduct us, with so much certainty, to the nature of the pathology and an unerring mode of treatment? Does it show any difference in the vascular changes, the serous and sanguineous effusions, or other consequences? (Vol. I. pp. 260, 296, &c.) And, finally, is it shown that they, who treat those supposed varieties of delirium tremens upon general principles, are less successful than those who abjure principle? Is the practice of treating yellow fever at one season by bloodletting, &c., and at the next, by bark and wine, almost in regular alternation, justified by the results? We speak of principles in their relation to pathology, not of those variations of a common plan of treatment about which the highest effort of medicine is constantly concerned. We speak of disease, too, in its early stages; not after the powers of life are sinking from the protracted effect of disease, or from structural lesions. Indeed, as we have seen on some former occasions, Dr. Stokes appears, at last, to be much of our way of thinking, as to principles; for, in condemning

"The ordinary practice in hospitals of giving wine, brandy, and opium, to such as have been attacked by delirium tremens, after a violent debauch," he says, "the practice seems to him as ridiculous as the old principle of treating a case of *hydrophobia with the hair of the dog that bit.*" (Vol. I. pp. 302, 605, *note.*) (*As a test of Homœopathy, we propose another bite.* "*Similia*," &c.)

In a late valuable paper by Prof. Ware of Boston, we find it stated that,

"If we compare the mortality of a given number of cases in which opium was pushed to the full extent advised by writers on this disease, with those in which an active remedy was employed, we have a mortality of 1 in 2, against a mortality of 1 in 29." "Of 13 patients who were bled from the arm, 2 only died, and these were both affected with peripneumony." (1)

In a later publication, Dr. Ware enlarges the foregoing statement. "Of 15 cases in which opium constituted the principal remedy, 6 died; whilst of 54 in which opium was used not at all, or only incidentally and in small quantities, only 5 died." (2) Höegh-Guldberg of Copenhagen, in his "remarkable work on Delirium Tremens," advises the use of the lancet in the "sthenic variety." (3)

It is precisely upon the foregoing principle that tartarized antimony operates; so much commended for this affection by Dr. Graves, (4) and some others; as, also, digitalis, by Drs. Pierson, (5) and Huss; (6) and other remedies which control inflammatory

(1) Boston Med. and Surg. Journ. Vol. xviii. p. 167.
(2) Appendix, &c. to the foregoing, April, 1838.
(3) Brit. and Foreign Med. Rev. vol. vi. p. 328; and vol. ix. p. 218.
(4) Clinical Lectures. (5) New-England Med. and Surg. Journ. 1820, p. 110.
(6) Brit. and Foreign Med. Rev. Jan. 1838, p. 242.

action. Bloodletting was employed by Huss in some of the cases. This success of the depletive system is conformable with our own experience. The objections which have been indiscriminately made to the use of the lancet are not founded upon any correct observation. There are scarcely two cases, however, which admit of precisely the same treatment; but a general principle is applicable to all, in the early stages. We have commonly found one or more mercurial cathartics important, and opium generally useful after depleting remedies.

Why is this affection so generally fatal, when its appropriate remedies are withheld? Some serious manifestation of disease should remain after death, considering the duration of the complaint, and the advantages of the antiphlogistic treatment. We should certainly expect some marks of common inflammation in a general sense; or, if we be right in our pathology of venous congestion, this condition, considering the phenomena, should be usually present. Have authors arrived at any solution as satisfactory as our own? Let us hear our latest and best pathologists.

"When," says Dr. Armstrong, "a patient dies of a brain-fever of drunkenness, you, generally, upon examination of the body, find very little morbid anatomy *to account for his death. Turgescence of the veins*, and slight preternatural fulness of the arteries of the pia mater and arachnoid, are most frequently found, and of these there are evidences during life. Effusion of serum into the ventricles of the brain, or between the pia mater and tunica arachnoides, will also be seen, and mostly *congestion in the liver*. (P. 431—433.)

"The appearances on dissection, then, do not explain the phenomena of this affection, and for want of a better name we call it a *nervous affection*. And what is a nervous affection? We know nothing at all about it, and the name amounts to a confession of our ignorance." (1)

Here, then, we have the amount of our author's pathology as to venous congestion; especially when we connect it with his doctrines of "local obstruction," and "remora," and "debility," (Pp. 230—243.) Superadded to all this, however, should be the contribution which is received from galvanism, in explaining the agency of the nervous system. Thus our author:—"*we cannot account for the influence of the nervous system, except by reference to galvanism and electricity.*" (2) So much "*we know.*"

(1) Lectures on Acute and Chronic Diseases, vol. i. p. 268.

(2) Ibid. vol. ii. p. 251.—It has been lately said by a distinguished reviewer, that galvanism, as an agent in the processes of life, has been generally abandoned. This is a mistake. The doctrine is more rife now than ever. We have supplied abundant evidence of the fact, and the daily press proclaims its ascendency.

It is remarkable, too, that this distinguished observer should resort to the mechanical doctrine of congestion to account for hydrocephalus, and other serous effusions, which supervene on venous congestions. "*The probability is*," he says, "*that the effusion of fluid then arises from an impediment to the return of venous blood.*" He then proceeds to state a case in point; as illustrative of the pathology of venous congestion. We shall quote it, as a farther example of the dangerous tendencies, philosophical and practical, of a substitution of the laws of dead matter for those of living organized beings.

"I recollect," he says, "the case of a man whom I saw nearly dying from congestion of the brain, produced by the impediment to the return of venous blood by a ligature round the neck." "A coachman had become entangled in the harness of horses that had run away." "I came up almost immediately." "He had fallen from the box," "and his cravat was drawn tightly about his neck, and that being removed, I gave him some stimulus;" "and when I left him he was nearly well. In this case, the tight ligature round the neck prevented the free return of blood; but effusion had not taken place; and this is what occurs in many cases of congestive fever." (1) (P. 262.)

It is not alone our objection to this mode of identifying the art of the hangman with a natural process; since it should equally follow from the foregoing example that stimulants are the proper remedies for cerebral congestions when nature undertakes the business of their production. Our special object, however, in adverting again to the foregoing doctrines of our author, is to avail ourselves of his conclusions as derived from his observation of nature. In following this guide, he lays down three causes of hydrocephalus internus; 1st. "venous congestion," or the mechanical cause; 2d. "inflammation," or the vital cause; and 3d. "organic disease," or a cause like the effusion itself, consequent on inflammation. He has no "straining" or "filtering" process. As an observer of nature, however, it is strange that Armstrong did not derive a hint from the remarkable analogies between the symptoms of the congestive and inflammatory hydrocephalus, the identity of the effusions, and the exact coincidence in the effects of the same mode of treatment as applied to either form of the complaint. (2)

Dr. Gregory resorts to an expedient, in expounding the pathology of delirium tremens, which is not unusual at the present day in relation to many diseases, and to which we have just seen that Armstrong makes a special reference.

(1) Lectures, &c. vol. i. pp. 256, 257. (2) Ut cit.

"It appears," he says, "to have for its proximate cause a peculiarly *excited state* of the nervous system; but, the occurrence of such symptoms in cases of extreme irritation would lead to the belief that *exhaustion* of the nervous power expresses, perhaps, more accurately its intimate nature." It may be one, it may be the other, or both connectedly.

Dr. Potter, of Baltimore, considers venous congestion of the brain, or inflammation of its membranes, as the true pathological cause of delirium tremens. But, it is well remarked by Dr. Vandervoort that, "the phrenitis of authors is not characterized by the peculiar symptoms which so strongly, and so clearly distinguish this malady."(1) The reason, we apprehend, may be found in the modifying influences of venous inflammation, and the peculiarity of the remote cause in the affection under consideration.

The researches of Barkhausen show a constant turgesence of the cerebral veins, and he places the disease between phrenitis and mania. Still he denies that the disease is inflammatory, "although," he says, "the frequent symptoms of inflammation attending the brain prove that this organ is especially concerned in delirium tremens." (2)

Flourens so constantly excited violent cerebral phenomena in his experiments with alcohol, and such was the state of venous turgescence of the brain, that the lesions appeared as if produced by a mechanical cause. (3) Orfila, (4) and Brodie, (5) found that when given to cats and dogs, alcohol produced apoplectic results, which were preceded by great cerebral excitement. According, also, to Andral, and many others, "we find the immoderate use of alcoholic liquors a predisposing cause of cerebral hemorrhage."(6)

"It is not improbable," says a distinguished Journal, "that it is to this venous congestion, as it may be termed, that we are to *ascribe the gradual effusion of serous fluid* in delirium tremens, in the ventricles, and beneath the arachnoid membrane." (7) (Vol. I. pp. 474 — 491, 521, 549, 568, — 577.)

Nevertheless, we are far from affirming that venous congestion of the brain is ever the entire pathological cause of delirium tre-

(1) New York Journ. of Med. and Surg. Oct. 1839, p. 297.

How often does it happen, that other patients die with symptoms greatly analogous to those of phrenitis, without any other serious lesion of functions, but in whom nothing unusual appears excepting a preternatural fulness of the cerebral veins.

(2) Delirium Tremens, tr. 1828.

(3) Rech. sur les Fonctions, &c. de Système Nerv. &c.

(4) Toxicol. Générale. (5) Philos. Trans. 1811.

(6) Andral's Medical Clinic on Diseases of the Brain, pp. 114, 135. *tr*

(7) Edin. Med. and Surg. Journ. vol. 25, p. 401.

mens. Morbific causes, whether external or internal, which operate with violence upon this organ, or, probably, in minor degrees, doubtless exert a direct impression upon every tissue of the part, and it may be sometimes that common inflammation, more or less modified, shall be developed instead of venous congestion. This appears to have been the experience of Mr. Curling. And, in quoting the opinion of this author, we shall obtain other analogies which show the foundation of the distinction between delirium tremens where venous congestion, or common inflammation, may be a prevailing element of the pathological cause. And here, too, we may premise that these differences will explain in part the disagreement among different observers, and the reason why bloodletting is borne to a far greater extent in some cases of delirium tremens than in others; it being especially the tendency of venous inflammation to depress the powers of life in its malignant, or active forms. (P. 447—448; and Vol. I. pp. 189, 197—210, 219, 222—226, &c.)

"In cases of *tetanus*," says the author to whom we refer, "in which death has been occasioned by the spasms, producing asphyxia, I have noticed that the *sinuses and larger veins are more particularly congested*, than in *other* cases where the injection appears to occupy the *small arteries*. And this *corresponds* with my observation of other affections of the nervous system, as *delirium tremens*," &c. (1)

We have seen that the attention of observers has been especially drawn to the plethoric state of the veins in congestive diseases, and that they have evidently been impressed with the belief that, in some mysterious way this phenomenon has performed an important part in the morbid process. We have seen that Dr. Armstrong, so devoted to this inquiry, imagines that "something may yet be discovered in the venous apparatus of the brain and liver, which shall explain why these organs are more subject to venous congestions than other parts." (Vol. I. p. 478, *note*.) And yet we have just seen that, at other times, he rejects the phenomenon as incapable of solving the problem of death. This doubt probably arose from the fact that the internal jugular veins may be tied without producing a cerebral symptom. He did not penetrate to the relative conditions of the vital forces of the venous tissue. Again, Prof. Alison remarks that,

"Even the peculiarities of that form of fever under the name of *congestive*, are not to be explained by the *mere circumstance* of internal congestion, the *existence* of which in the vessels and *especially in the veins* of internal parts, in

(1) Essay on Tetanus, p. 30.

these circumstances *is admitted*." "It is in vain to seek an explanation of the *symptoms*, as some have done in the *mere circumstances* of irregular distribution and congestion of blood." "In the cases where the congestion in the great veins *fails* to *excite reaction* in the heart, some *peculiar cause* must have operated to *prevent* the heart from being universally excited by the application of the *unusual quantity of its natural stimulus*." (1)

And thus Southwood Smith, alluding to venous congestion:

"It is remarkable and highly characteristic of the intense forms of congestive disease, that its pathology exhibits a *striking contrast* to that of the less severe affections. No morbid appearances are visible in the organs which *seem capable* of accounting for death. There are signs of vascularity; the vessels are *turgid with blood*, and consequently the organs on which they are spent are in a state of congestion. But they *seldom, if ever, exhibit any real appearance of inflammation*, and still less do they contain any inflammatory product." (2)

Dr. Marsh has the same views. "But those who have seen the most intense forms of contagious fever," he says, "and who, at the same time, have observed other diseases of which an overloaded state of the veins is a *leading feature*, will scarcely admit, that venous congestion can satisfactorily account for a series of symptoms, such as those which characterize this worst and most dangerous variety of fever." (3)

And so Mr. Moore: "In puerperal fever, when the patient is rapidly destroyed by the violence of the disease, the morbid changes bear no proportion to the severity of the previous symptoms. *A dubious trace of inflammation, a little bloody serum, or a few feeble adhesions*, are all that dissection, under such circumstances, displays." (4) (See opinions of authors as to the phenomena, pathology, &c. of puerperal fever, &c. as hitherto cited in this section.)

Before venous congestion had again attracted notice in modern times, and in countries where "medicine was yet in its infancy," we find Rush stating on the authority of Humboldt, that in many who died of yellow fever at Vera Cruz there was no apparent cause of death,— "no marks of inflammation were exhibited, nor the least deviation from a healthy state." (5) Still, it may well be, that in some of these cases where the predisposing cause acted with rapidity, and death took place near the invasion of the disease, that no time intervened for sensible vascular changes, or other apparent physical results. (Vol. I. p. 471 — 480.) And thus the humoralist, (positive without facts,) in cases where the disease being prolonged, no one can doubt, from the large experience of others, in analogous instances, that

(1) Outlines of Physiology & Pathology, p. 517.
(2) Treatise on Fever, p. 96.
(3) On Puerp. Fever, in Dublin Hospital Reports, vol. iv. p. 521.
(4) Enquiry into the Pathology, &c. of Puerp. Fever, p. 63, 1836.
(5) Medical Inquiries, &c.

there must have been special developments of venous congestion: —

"In many contagious fevers," says Dr. Stevens, "there is not one symptom of inflammation during the fatal progress of disease, nor one inflammatory spot to be seen after death, to mark its existence, or to induce us to believe that any thing but functional disease had existed in any of the solids; yet these are the very cases, of all others, which are *most fatal.*" (1)

And here M. Andral was probably embarrassed by his humoral doctrine, when he came so often in contact with venous congestions of the brain. Thus: —

"Une grave question nous arrête, c'est celle-ci. Les symptômes qui caractérisent les divers formes de congestions cérébrales sont-ils liés dans tous les cas à l'afflux d'une trop grande quantité de sang vers le cerveau? *Dépendent-ils uniquement* de cette cause?" (2)

Even *disorganization* of the veins has been thought necessary to connect the pathology of purpura with the congested state. Thus Dr. Stoker: —

"The post mortem examinations, which were made in the second case of purpura, of the parts previously occupied by disease, discovered no disorganization which could account for the symptoms detailed in the history of the case." (3)

By referring to page 436, it will be seen that the same objection has been made in phlebitis attended by disorganization of the venous tissue. The difficulty arises, as we have endeavoured to explain, from not appreciating the physiological constitution and relations of the venous tissue.

Looking back to the various affections which we have considered, we are everywhere struck with a coincidence in some of their most imposing phenomena, and we everywhere find one common lesion, — venous congestion, or undoubted venous inflammation. Taking the most simple form in its most local sense we have a type of the whole, by which we may ascend progressively upwards till we reach the strongly marked conditions of venous inflammation, without losing a hold upon some of the analogies which assure us that the common feature is imparted by venous inflammation. Or, where constitutional influences may not obtain, as in varix and venous hypertrophy, there are still present the dilatation of the veins, their disorganization, pain, soreness, &c. to establish their relationship, and to throw a broad light over the common family, however they may be re-

(1) On the Blood, &c. (2) Clinique Médicale, t. v. p. 298.
(3) Pathological Observations on Dropsy, Purpura, &c. p. 35, 1824.

moved in degrees of consanguinity, or in a constitutional sense. "*Ita res accederunt lumina rebus.*" (1) (P. 300, *Zimm.*)

We are now prepared to carry the foregoing pathology into the history of asthma; of genuine asthma, not of other affections which have been confounded with it. (2) We shall call it *congestive asthma.* And here we may premise, that what we have hitherto said in relation to the circulation of the lungs, of the doctrine of remora, (Sec. 2,) and of the powers which conduct the general circulation, should be duly regarded in connection with this subject.

The venous engorgements of the pulmonary mucous membrane, sometimes extending through the substance of the lungs, in asthma, evidently depend upon the same morbid condition of the venous tissue that we have hitherto considered. (3) This is denoted by a variety of circumstances; — by what we have already said of the necessity of some morbid condition of the veins to their dilatation, as manifested in their morbid congestions; by the remote causes, and by the frequent dependence of asthma upon disease of other organs, (4) where sympathy is the medium of development; by the local, and often by the constitutional, symptoms; by the buffing and cupping of the blood, when properly taken for examination, (p. 291;) by the frequent termination

(1) Lucretius, l. 1, v. 1110.

(2) We do not include what is called the "nervous asthma." Ramadge says, "the scientific use the term asthma in as lax a sense as the uneducated." (*a*) Ryan, (*b*) and others, complain in the same way; yet most of them fall into the error.

(3) The proximate or pathological cause of asthma is referred by some writers to a spasmodic affection of the muscles of the glottis; by others to a like affection of the muscles of respiration; and, by a third class, to a similar condition of the bronchial ramifications. Cullen says the disease has a "*manifest*" cause. He conjectures "a spasmodic constriction of the bronchia." But this philosopher regards only the effect, since "it is *excited*," he says, "by a *turgescence of the vessels of the lungs*," "attended by a copious expectoration of mucus." (*c*)

If the spasm exist, it may be shown to be only a symptom. It is from regarding the spasm as the disease, that the special symptomatical doctor prolongs the suffering in asthma by the exclusive use of "nervines," and "supporting the strength" by beef-steaks and porter.

(4) See Bree, On Disordered Respiration, pp. 14, 26, &c.; Guersent, in Diction. de Méd. Art. Asth. Aigue, p. 125; Forbes, in Cyclopædia Prac. Med. Art. Asthma, p. 196; Morgagni, Bonet, Lieutaud, Floyer, Ramadge, Ryan, &c.; all of whom notice the frequent complication of asthma with affections of other organs.

(*a*) On Asthma, p. 1. See his Diagnosis, p. 18 — 36.

(*b*) On the History and Cure of Asthma, p. 5.

(*c*) First Lines, &c. §1385.

in the expectoration of mucus, and sometimes of blood; by the nature of the remedies, these being strictly antiphlogistic.

We shall here stop, for a moment, for the purpose of saying that the *mucus* to which we have just referred, as being a common attendant of asthma, is considered by many of our systematic writers either as the *pathological cause*, or in the highest degree probable, (though generally called a *symptom ;*) just as others regard spasm in the same light. Thus Bree : —

"The spitting of mucus is so *general a symptom* of convulsive asthma, that this matter must have been considered the principal cause of the disease by *modern* physicians, but from the *obstacle* of *some few* cases having occurred, in which there was *no expectoration ;* and from that of others in which it was not considered *equal* to the *effect* produced. But, in the dry asthma, "all pathologists have been stopped in their course, which probably *confused* their views of asthma from manifest causes." (1)

Dr. Forbes makes a remark which is full of pathological import. "The exhalation," he says, "which is commonly attendant, puts an end to the disease for a time." (2) This fact was also stated by Aretæus, in his graphic description of the disease. (3) The rationale is the same as in other inflammatory affections.

We shall not pursue this philosophy farther, but proceed to point out certain facts, which, like the foregoing *symptoms*, may develope the true pathology, and cast a light over the conditions of disease which we have already examined. We may premise, however, that the tendency of venous congestion to develope, or to result in, common inflammation, and to generate some of the products of the latter, in tissues external to the congested veins, will become the subject of farther examination.

Asthma often induces hydrothorax, and more frequently results in organic lesions of the lungs. (4) "The membrane lining the nose and mouth, before and behind, discharges a flux of serum, with which the mouth is occasionally filled." If we examine that membrane, it is rather livid than red.

"It sometimes happens," says Ramadge, "that inflammation of the chest will manifest itself at the close of a severe asthmatic attack."

(1) Op cit. pp. 57, 98, 112, 183. — Also, Davis, on Diseases of the Chest, p. 247.

(2) In Cyclopædia of Prac. Med. Art. Asthma, p. 193. (3) De Diutur. l. 1, c. 11.

(4) Ryan says, that "it has been remarked by almost every writer on the asthma, that tubercles are almost always formed some time before its fatal termination." (*a*) Ramadge gives a pictural view of them. Are these, and the other morbid products owing to "mucus," or "spasm"? Shall we prevent their formation by narcotics, and anti-spasmodics? See APPENDIX No. 5, *on Tubercle.*

(*a*) Op cit. p. 85.

"The same congestions are often observed in the parenchyma and pulmonary membranes which are met with in *asphyxia*." "Extensive adhesion, inflammation, or emphysema of the lungs; œdema of the same viscus; glandular or calcareous obstructions at the root of the lungs," &c. (1)

In a case by Dr. Parry, the only morbid appearance was "a complete suffusion of a damask-rose colour, amounting in some places almost to *blackness*, of the entire mucous membrane of the trachea, and of all the ramifications of the bronchia." (2) Compare this case, which presents the disease in its elementary form, with Ramadge's fifth plate. "A congested or tumified state of the mucous membrane," says Dr. Forbes, "almost invariably accompanies the paroxysms." (3)

In the five elegant plates by Ramadge, where the cases do not appear to have been complicated with any difficulty at the heart, there seems to exist an universal state of pulmonary venous congestion. The dull redness on one side of the lungs in plate 3d, is clearly owing to sub-pleural emphysema, though venous congestion often passes rapidly into common inflammation. This livid appearance is not always stated by authors, as it is held to be unimportant, especially by the anatomists. It must be admitted, however, that dissections have not been numerous, whilst some, reported as asthmatic, belong to other affections.

Asthma is often brought on by organic affections of the lungs, and by various chemical, mechanical, and other irritants, when they may enter these organs. "Stimulating liquors, and indigestible food, are among the worst of the exciting causes." (4)

"The attacks, if not superinduced, are generally accelerated by catching cold." (5) The distinguished Forbes considers cold "by far the most common exciting cause," denies the frequency of "nervous asthma," and maintains that "nine-tenths of the cases of asthma are complicated with some form of catarrh." And, "of all diseases that *predispose* to asthma, *inflammatory* and catarrhal affections of the bronchial membrane are by far the most frequent and effective." (6) So, also, Davis, (7) Ryan, (8) Millar. (9)

And thus Guersent: "On remarque surtout l'asthme dans les climats froids et humides, et dans ceux qui sont très-froids ou très-chauds." (10)

Hoffmann says, "si quod ex causis occasionalibus est quod asthma convulsivum induere potest, certe est externum frigus." (11) — (See APPENDIX, No. 3. on *Cold*.)

Of all this we entertain no doubt; and we ask whether cold

(1) Op cit. pp. 10, 164. (2) Elements of Pathology, s. 474.
(3) Cyclopædia of Prac. Med. Art. Asthma, p. 193.
(4) Bree, ut cit. p. 180. (5) Ramadge, ut cit. p. 174.
(6) Cyclopædia of Prac. Med. ut cit. pp. 196, 197.
(7) Lectures on Diseases of the Lungs, &c. pp. 229, 231, &c.
(8) On Asthma, pp. 39, 78. (9) On Asthma and Hooping-cough, p. 13, &c.
(10) Dic. de Méd. ut cit. p. 106. (11) Quoted by Ryan, ut cit. p. 54.

debilitates and relaxes the veins of the lungs, or constitutes obstructions to their circulation; why not in other organs; why only in particular subjects, and why are they so often liable to its repetitions; why, as noticed by Aretæus, (1) are the young, and pregnant women, the most common subjects; and why, as he remarks, are the young most easily relieved; and, if there be the least foundation for the doctrine of *remora*, why is not the brain, and every other part congested where the lungs are so seriously troubled with venous blood? (P. 230 — 257.) We will say, however, that we believe the cold to operate as an exciting cause of the venous affection, and that the catarrhal symptoms are only a consequence of that condition of disease. It is upon the same principle that asthma is often produced by a heated air. These causes, too, as we have shown, will equally determine venous congestions of other parts, as well as common inflammations; and this is especially true, in all the cases, of a damp, cool atmosphere.

Forestus, in describing an epidemic asthma, attributes it to an epidemic catarrh. But here the pathological cause of asthma was clearly antecedent, and the cause of the catarrh. The latter was no more a cause of the former, than the lymph, or serum, or pus, are the causes of the inflammatory action upon which they depend. In the epidemic by Forestus, the symptoms were violent, *bloody mucus* was expectorated, — "sputum sanguinis in hoc morbo epidemico apparebat," &c. (2) Berger describes a similar epidemic. (3) Millar remarks that, October 1755, asthma prevailed along with inflammation of the bowels. (4) Others have observed similar coincidences, which are important in ascertaining the true pathology of congestive asthma.

In the epidemics, the causes were unusual, and of a malarious nature; such as induce venous congestions, and common inflammations, in other organs than the lungs. For this reason, we may well infer that epidemic asthmas are marked by peculiarities in respect to the exact condition of the pathological cause of the pulmonary congestion, which distinguish them from periodic asthma. Bloodletting was the *remedium principale* in the foregoing epidemics.

"Periodic asthma," says Bree, "is preceded very generally by dyspepsia, which may have prevailed for months or years, before it takes the additional

(1) De Asthmate.
(2) Op. Omnia, t. i. l. 6, ob. 3.
(3) De Circ. Lymph. c. 2, s. 7.
(4) Op cit. p. 11.

form of asthma. But when this disease appears, dyspepsia never fails to be augmented, and to show itself with violence before the fit." "When an asthmatic feels these warnings, he may be convinced that his enemy is at hand." He "believes that the seat of asthma frequently exists in some of the viscera below the diaphragm."

"Temperance," says Ramadge, "becomes, perhaps, a more positive virtue in asthma, than in *any other* disorder." (1) Why?

Every true observer of nature must recognise the truth of the foregoing facts. Nevertheless, there are some writers who have fallen into the error of considering the abdominal affection wholly consequent upon the asthmatic attack. (2) Regarding these conditions, however, in their obvious connection, we see vital causes alone so operating upon the lungs as to produce an engorgement of their veins. In the *periodic* asthma, the abdominal disease, so often antecedent, is slow in its early sympathetic influences upon the vital properties of the pulmonary vessels; but, as soon as the impression is fully produced, the disease is then readily excited, either by a damp, cool air, by heat, a single indulgence in wine, or indigestible food, &c. Will mechanical philosophy explain? It has been often remarked by the moderns that pregnant and puerperal women are very liable to asthmatic symptoms. Hippocrates, in describing the latter, points out a congested state of the lungs as the pathological cause. Thus:

"Si vero morbus paulo diuturnior fiat, patietur sane mulier ea quæ dicta sunt de virgine, cui menses primum comparentes, sursum impetum fecerunt." "Si vero puerperii *purgatio* non exeat per os, sed sursum tamen impetum fecerit, occultabitur, et non procedet justo modo. Et *tussis* corripiet, et *anhelationes*. Et *impleto pulmone a sanguine*, valde *dolebit* latus, et dorsum. Et quum tussiet, *sicca tussiet*. Aliquando vero *spumosum expuet*. Temporis autem progressu sputum apparet *subnigrum*, et *turbatum*. Et *calor pectora* tenet, *magis* quam *reliquum corpus*, nimirum sanguine ipsa calefaciente. Et *febrit* mulier, et venter ipsi coarctatur, et *cibum* fastidit, et vigilabit, et abominabitur, et non superstes manet," &c. "*Non multa vero spes* est de vita ipsius."(3)

"The natural consequence of bad cases of asthma is apoplexy." (4) This is stated by Aretæus, (5) who has a graphic account of the disease. Georget has so frequently found the brain congested in asthma, that he is inclined to consider the disease as having "its organic cause in the centre of sensibility." But, trace the history of the symptoms from their beginning,

(1) Op cit. p. 200.

(2) See Ryan, on Asthma, p. 26; and Percival's Essays, vol. ii. p. 394, &c.

(3) De Morb. Mulieb. l. 1, s. 2, ver. 309—320, 329.

(4) Bree, Op cit. p. 112.

(5) De Caus. &c. l. 1, c. 11, de Asthmate.

and this will always come out a sympathetic result. (Pp. 248, 279, 283, 290, 333, 351, 364, 369, 454.)

Now we have shown conclusively, (pp. 230—257, &c.) in opposition to the common belief (1) that neither asthma, or any other pulmonary affection, constitutes an obstacle to the descent of venous blood from the head; and we have shown that the venous injection of the face is produced by the action of the muscles upon the external jugulars. Besides, "the face, in asthmatic fits is often shrunk and pale;" (2) a symptom often distinguishing venous congestion, whether in its simple form, or as complicated with idiopathic fever. Aretæus speaks of it in asthma, as occurring when the respiration is most laborious. Thus:

"Erecti spirant, et os aperiunt, patefaciuntque, tanquam eo ampliore indigentes. Ipsorum facies præter malas, *quæ rubent, pallida est.*" (3)

The cerebral phenomena will depend as well upon the extent of disease which may be thus sympathetically induced in the venous system of the brain, which may be short of inflammation, as upon any general sympathy of the organ. The phenomena must mainly determine the actual amount of venous disease, after a due consideration of the remote cause and its modifying influences. If apoplexy take place, in most instances there is an extravasation of blood, which we shall endeavour to show is a farther evidence of a morbid condition of the venous tissue. (See Sec. 16; and Vol. I. p. 371—384.) If protracted coma, instead of apoplexy, we have then the morbid phenomena and the physical products which we have already described as attending venous congestions of that organ in various other cases; whilst, as we have shown, the greatest engorgement of the cerebral veins that has been yet artificially produced is not followed by any morbid results.

Insanity also follows asthma. This may be owing to a general lesion of the cerebral powers, which, however, as shown by the phenomena, and by dissections, is commonly associated with venous congestion of the brain; or, in other cases, this congestion has passed wholly, or in part, into common inflammation. When the last is alone present, we have no other proof that it did not exist ab initio, than the symptoms and analogies, which will be admitted by the sound pathologist to be a pretty good criterion. Dr. Bree has the following logic upon this subject:—

(1) See Ramadge, ut cit. p. 50.
(2) Cullen's First Lines, § 1389. (3) Ut cit.

"We conclude, then, that asthma, insanity, and dropsy, have the same cause; for, if insanity and asthma were one disease, and insanity and dropsy were one disease, asthma and dropsy must be one disease." (1) (Vol. I. pp. 68, 434, *notes*.)

Dr. Bree was evidently impressed by the phenomena with an identity of cause; and, we believe in the logic, so far as venous congestion or common inflammation are the pathological causes of the insanity and dropsical effusions attendant on congestive asthma.

Asthma is incident to those periods of life when the vital powers are most active; and if other venous congestions are rife in later years, it must be recollected that the asthmatic suffers constitutional peculiarities which predispose him to a modified form of pulmonary venous inflammation, or a condition of near approximation. In old age, the powers of life are more languid, (or debilitated, if it be preferred,) and "it makes the habit less irritable." "In extreme old age, the disease is almost lost;" (2) although now, if ever, mechanical obstructions, passive relaxations, and passive effusions, should be most likely to prevail.

The pathological cause of congestive asthma becomes determined beyond any proof to the contrary, or any dictum of the mere anatomist, or the doubt of others with whom every symptom is a special condition of disease, or who may reject the clearest analogies, by connecting with the foregoing considerations the operation of its curative means. And here we must once more necessarily turn the attention of the reader to what we have said in another essay upon the philosophy of the operation of bloodletting; especially in its combined relations to inflammatory and congestive diseases. That essay was originally designed as a part of the present; and, indeed, we may make the same affirmation of every other, since every other took its departure from the present, in the form of notes. In carrying them out, as became ultimately our purpose, into a system of medical philosophy, we have endeavoured to maintain the natural relations of the whole, and to render each more or less subservient in illustrating the pathology of venous congestion. This will account for our frequent references to the several essays.

Bloodletting, cathartics, nauseating medicines, blisters, and low diet, are the principal remedies for congestive asthma. The

(1) Op cit. p. 115. (2) Bree, p. 154. Also Ramadge, p. 9

depletory system prevailed with the great masters at the early stages of the art, and modern experience has justified the practice.

"The most probable means," says Cullen, "is bloodletting." (1) Even such as maintain the ground of "*debility*" often commend the use of leeches. Thus Ramadge:—

"There are *many reasons*, which render the local abstraction of blood, in the *majority* of instances, not merely *safe*, but a *highly beneficial* remedy." "I am no friend to the unnecessary abstraction of blood, and would by no means encourage a wanton expenditure of the pabulum vitæ." But, "it would be unwise to reject a *positive good for an hypothetical evil*." (Vol. I. *Bloodletting*, Sec. 2, 3, 5, 7, 9.) He would not venture so far as *cupping*. (2)

"Blisters," says Ryan, "are objected to on the principle that the disorder is seated in the nervous system; (p. 488,) and never was there a more striking instance of the baneful influence of theory unsupported by facts." (3)

This is sustained by Whytt, (4) Percival, (5) K. Hoffmann, (6) and many other writers. Percival found them so beneficial that, with the pathology of *spasm* before him, he says they act "not as evacuants in such cases, but as *antispasmodics*."

Asthma is so often the sympathetic result of abdominal disease, that bark, iron, wine, &c. are sometimes beneficial as means of prevention. These occasional advantages, under such circumstances, have led many, who make no distinction between the disease and its predisposing cause, to the destructive use of those agents after the asthmatic affection has actually taken place. Floyer says of them, that,—

"Steel *stops* the breath," and "ferruginous mineral waters affect the *head* with *drowsiness* and *giddiness*." And as to "castor, amber, assafœtida, volatile salts, or sulphurs, when taken inwardly, they rarify the spirits, raise the effervescence, and drive the windy spirits into the nerves, by which the strangulation is increased." (7)

Sir John, however, is excellent in his peculiar way. He was as much troubled about the real pathology of asthma as any of his successors. Nevertheless, he made a good application of his experience when he saw that tonics and antispasmodics "stopped the breath," and led to "drowsiness;" especially as he experienced these effects in his own person. (Vol. I. p. 296.)

"I found," he says, "I had read most of the modern writers, and hitherto had tried their methods, and hot pectorants and cephalics, in vain. I believed by my ill success in *their* way, that they never understood this disease; and, therefore, turned over some of the old writers, Galen, Ægenita, Ætius, &c. where

(1) First Lines, § 1389.
(2) Op cit. pp. 160, 255.
(3) Op cit. p. 90.
(4) On Nervous Disorders, &c. p. 461.
(5) Essays Med. and Exp. vol. i. c. 4, p. 217.
(6) De Vesicat.
(7) Sir J. Floyer, on Asthma, p. 112.

I found more rational notions, and was directed by them to the use of that medicine which does very much relieve and prevent my fits." (1)

Clutterbuck, after a long life of experience, and with all the past before him, remarks, that "it is quite evident that the impediment in asthma is not seated in the larynx;" and that "in most cases of periodical asthma, the disease may be traced back to *catarrhal inflammation*." He condemns "the whole tribe of antispasmodics, stimulants, and narcotics," and pronounces *bloodletting the most important remedy*. (2)

The foregoing considerations enable us to understand the source of those various modifications which attend the different *species* of congestive fevers. Partly depending on the specific nature of their predisposing causes, the local, as well as the constitutional action, being imbued with their specific influence, and the specific characteristics, (which, however different, are yet so allied as to group together the several species as members of a great family,) being determined by the constitutional affection, the inflammation of the venous tissue imparts yet another general resemblance, — varying the whole from their simple type, and often more or less confounding the specific characteristics under a common aspect. When venous congestion acquires this last ascendancy, and disguises the essential attributes of the constitutional affection, there is no condition of disease which more imperatively demands enlarged views in pathology, a deeper scrutiny of symptoms, or greater moral firmness in the treatment. We even think that cotemporaneous, and well marked local inflammations, associated with these conditions, contribute to the safety of the patient. (P. 221.) They tend to counteract for a while, the depressing influence of venous inflammation, to lull the imagination which sees nothing but "debility," or "putrefaction," in the *prostration* of muscular and organic force, to sustain the powers of life under the influence of depleting remedies, which alone may cure, or to give the last remaining hope which may be inspired by the unaided vis medicatrix, but which may be speedily extinguished by tonics and stimulants. (See *Bloodletting*, Sec. 5, 6, 7, 9, 12, 13.)

(1) Op. cit. p. 15. — Floyer, however, is mistaken as to Galen, who has a strange medley, dictated in this instance by his humoral notions. *See Ætius, Tetrab. Secund. Ser.* 4, *c.* 57.

(2) Lectures on Bloodletting, in Lon. Med. Gaz. July, 1838; pp. 631, 697, 698.

It is upon the principles which we have now stated that we may understand why the typhus of one country, or one season, has been, under equal circumstances of treatment, varied in its phenomena from that of another; (even, indeed, without a contribution from the glands of Peyer;) why epidemic scarlatina and measles are more fatal than the simply contagious; why the intermittents of Italy and of Africa are more pernicious than in other countries. (1) And whence proceeds this important agency of the veins in imparting such malignancy to the intermittent fever? Why do the intermittents of Italy often manifest a resemblance to the congestive typhus? Are not the modifications of inflammatory typhus determined by the local inflammations? But the congestive variety is distinguished by far graver symptoms. And shall we in one case impute the peculiar constitutional disturbance to a simple fulness of a limited portion of the veins, whilst in the other we refer the modifications to a local disease of the highest import?

But, venous congestion, as we have endeavoured to explain, like inflammation, is variously modified by the nature of the remote causes, by constitution, age, habits, by its independence of other affections, or its supervening in consequence of disease in other parts, and by various, and often inscrutable circumstances that may attend its progress. Like certain modifications of inflammation, it may not yield to any extent of bloodletting, but may ultimately, as when attendant on intermittent fevers, require the specific agency of quinine. (See *Bloodletting*, sec. 11, 12.) And why should bloodletting be as useful as bark is injurious in one modification of venous congestion, and the latter, with proper auxiliaries, (2) a specific for another modification? (Vol. I. pp.) 289, 303, 304.) We have an imposing example of this principle in the extraordinary control, as stated by all Italian writers, which is exercised by the bark over the apoplexies which distinguish the intermittents of the European peninsula. But, even here, the integrity of general principles is preserved, and the pathology of venous congestion still denoted, by the necessity of

(1) See the Anatomical Researches of Torti, Morgagni, Lancisi, Baglivi, Bailly, Burnett, Pringle, Cleghorn.

(2) We do not regard the bark as acting, in these instances, in the least on the principle of *tonics*, in the common acceptation of their operation. Morgagni, Epis. c. 1. ep. 4. s. 10; ep. 49, s. 30, and the Italian authors generally. Also, Morton's Pyretologia.

antecedent bloodletting and other evacuants. Nor should we neglect saying, that the fact which we have now stated confirms the importance of studying with the greatest care the nature of the remote causes of disease.

The same variations of treatment are equally necessary in the different modifications of inflammation. And here we may state the remarkable fact, which appears to identify the pathology of venous congestion with that of inflammation, that it is with great rarity that bark is adapted to either condition except when associated with fever of an intermittent type, or clearly induced by causes which generate intermittents. (Vol. I. p. 305, *note.*) We may again illustrate this by another example from a distinguished source.

"A lady," says Sir B. Brodie, "laboured under an inflammation of her leg. The whole leg was swollen from the toe to the knee; the skin being red, painful, and tender. These symptoms had existed for several weeks; the usual remedies had been employed, and no amendment had taken place. At last, I observed that the symptoms varied considerably; that sometimes the redness, pain, and swelling had nearly subsided; that, at other times they were as strongly marked as ever; and that these variations always took place on the alternate days. She was now directed to take the sulphate of quinine. The effect was immediate, and a few days completed the cure." (1)

"When erysipelas occurs with the remittent form of fever," says Dr. Armstrong, "it is surprising how fast the bark stops it." And yet he had just said, that in common erysipelas "the bark and wine system is of all the most fatal practice generally." (2) It is the result of M. Bailly's researches, that, in the pernicious fevers of Rome, the use of bark, in conjunction with depletion, is indispensable even when the fever is complicated with the most active inflammation of important organs. (3) And so other Italian authors.

Maillot always prescribed large doses of quinine, along with depletive remedies, at the onset of the Bona fever, which was generally attended by phrenitic symptoms, that were doubtless the result of venous congestion. (4) We may say, however, that bark and even mild stimulation, are sometimes suited to inflammations which spring from other causes, though only under some very peculiar circumstances; whilst the whole history of venous congestions, in their early stages, whether sim-

(1) Lectures on Local Nervous Affections, p. 47.

(2) Lectures on Chronic and Acute Diseases, vol. ii. pp. 101, 102.

(3) Traité des Fièvres Intermittentes, &c. p. 265, &c.

(4) Recherches sur les Fièvres du Nord de l'Afrique. See, also, M. Piorry, de l'Irritation Encéphalique des Enfans, in Répertoire Méd. Chir. Brux. Août, 1837.

These facts should be considered in connection with what we have said of bloodletting in fevers, vol. i. p. 303 — 305.

ple or combined with idiopathic fever, places them amongst the worst of the lædentia. (Vol. I. pp. 222, 231, 239—248.) Again, bloodletting, especially the general remedy, is more uniformly important, or beneficial, in this affection, than in inflammations. (Vol. I. pp. 130 — 156, 219, 338, 345, &c.) Armstrong presses the necessity of bloodletting in simply congestive fevers; but, "where any poison," he says, "produces a congesto-inflammatory form of disorder, the patient cannot bear general bloodletting." (1) We cannot doubt that this is sometimes true, for reasons formerly assigned, (Vol. I. p. 146,) and from the accuracy with which Armstrong studied nature; but, in its general sense, and when applied to the early stages of disease, we believe it to be a capital error. The distinguished Graves, who can "cure a pneumonic attack by moderate bleeding and without injuriously weakening the strength of the patient," with the paramount aid of tartar emetic, remarks of certain congestive affections, "if tonics be long continued, the persons using them are apt to fall down suddenly in a fit of apoplexy." (2) This may be a small circumstance in the way of objection; but since we are now most concerned about the real pathology of these affections, we may say by the way, that the cerebral veins are found stuffed with blood in these cases, whilst the arteries are either empty or have only their usual proportion of blood. Nor is it irrelevant to our present inquiry, to ask whether the *tonics* produced any mechanical obstruction, or *debilitated* the vessels? (P. 178.) Again, in neglected, or protracted cases of congestive inflammatory diseases, where the vital forces are sunken, bloodletting may in either case, aggravate disease, whilst it may be removed by tonics and stimulants.

It is manifest, therefore, that the occasional effects of tonics, or, we should rather say of the Peruvian febrifuge, in removing venous congestions, so far from invalidating the philosophy of our pathological cause, appears strongly to confirm it. The bark is equally, or more curative of intermittent fever, and intermittent inflammations. It operates upon exactly the same principle in removing the analogous venous congestions. The *rationale* is to be found in the impression produced upon the vires vitæ of the venous parietes, by which an inflammatory condition, like that of the common intermittent inflammation, is overcome.

(1) Lectures, ut cit. vol. i. p. 413.

(2) Lon. Med. & Surg. Journ. vol. ii. p. 75, and vol. iii. p. 103.

Can any thing be clearer than our analogies? The same remote causes, which establish the distinction between intermittent and other fevers, and which so modify their associate inflammations as to render the bark curative in either case, whilst it would be detrimental where other causes operate, have, in like manner, so modified the inflammation of the venous tissue as to bring it under the sanative power of the same agent, but which, in other cases not possessing this modification, would be pernicious. It is upon exactly this principle, that, as we have seen, (p. 485,) opium is more or less beneficial in its sedative doses in the cerebral congestions which are produced by an excess of alcoholic liquors, whilst it is more or less injurious in the various modifications that are induced by other remote causes.

Nor can we refrain from dwelling upon the remarkable influences which are determined upon venous congestions, as upon idiopathic fevers and inflammations, by the exact nature of the predisposing and exciting causes, especially by the former, and which reflect so strong a light upon the true pathology of congestion. Indeed, there are reasons to believe, as we have amply shown, that venous congestion is more influenced than inflammation by the nature of the remote causes. With this consideration, however, must be connected the nature of the organs upon whose venous tissue the influence of the poison may especially fall. The same modifications of the pathological cause constantly happen in acute phlebitis, as denoted by the symptoms, &c. It is not the same when induced by the lancet, as in uterine phlebitis; and it differs from both when generated by injecting morbid animal poisons into the veins. Nor can we doubt that differences will arise in the last cases corresponding with differences in the nature of the morbific agents so employed. This is clearly the ground of the philosophy which induced Magendie to affirm that he "could give rise, at his pleasure, to *pneumonia*, *scurvy*, *typhoid fever*, &c., not to mention a number of other affections which, so to speak, I called into being before you." (Vol. I. p. 397.) There are, indeed, scarcely any two cases of inflammation exactly alike, and the differences become more and more remarkable according to the differences in the remote causes. Cantharides excites a modification of the disease which differs from that by a mustard cataplasm; and this is especially shown by their effects upon internal inflammations. A scald is different from any other modification of inflammation, as shown

by its constitutional effects; often destroying life when only superficial. And so of other local irritants. (Vol. I. p. 495, &c.) Yet all belong to a common genus, and are intimately allied.

If we compare the observations which have been made upon the mephitic gases and analogous agents, by Haller, Morgagni, Portal, Nysten, Halle, Magendie, Bartholin, Thenard, Pechlin, Ramsey, Pringle, Bancroft, Louis and Hippocrates, we shall be fully impressed with the differences which prevail in their morbific properties, and the coincident modifications of the vital powers. What can demonstrate more clearly the precise impressions to which these powers are liable, according to the nature of the morbific agent, than the influences which are produced by the virus of smallpox, and kine-pox; especially if it be true that the virus of each was originally identical, and that the differences have been established by those vital actions which each may generate anew. Again, vaccinate a healthy subject, and he will be, probably, for ever protected against the former disease. But, although the susceptibility of the vital properties to the specific influence of smallpox be thus extinguished, the virus will at all times produce an eruption in some modified condition. If vaccination be performed when another disease is present, especially of the skin, its influence upon the vital properties is more or less resisted by their already altered state, and the subject remains partially susceptible of smallpox; and so he is, also, of a repetition of the kine-pox. This is only equivalent to the resistance which is made by disease to curative agents. Vaccinate again, and we may extinguish the susceptibility to both. In these cases is seen the play of different causes; all acting in collision by their different impressions upon the forces of life. (See Vol. I. p. 560, *note*.)

Nevertheless, the alliance amongst the different modifications of venous congestion is very great, as it is with the various modifications of inflammation; and the same general principles of treatment hold good in all. Nor do we deny a strong alliance between the different species of idiopathic fevers. They appear to be fully distinguished from each other by the combined circumstances of each; though certainly less so than the contagious affections. In the latter, as smallpox, measles, scarlatina, hoopingcough, &c., the vital properties are modified in a way which is strictly sui generis. Thence, they are always capable of producing a specific virus, by which the same disease is propagated

from one to another. In these instances, also, the properties of life generally undergo determinate changes, return spontaneously through uniform alterations to their natural state, and for ever after resist the influence of the same causes.(1) This also implies an identity of the morbific agent in each of the species, since the vital properties are always altered in one unvarying manner. And, as no animal product has ever been imitated by art, and as there is every reason to believe that the laws of life terminate in results wholly different from those of physics, we cannot suppose that the contagious diseases are ever propagated by any other causes than such as are generated by those precise modifications of the vital powers which constitute the essence of the diseases. (Vol. I. p. 469.) It is plain enough, however, that other causes may predispose the properties of life to be specifically acted upon by the contagious virus; since measles, &c., spring up epidemically. For the same reason, also, all those affections which have for their causes the products of laws which govern inorganic matter, can neither be regarded as contagious by the philosopher, nor shown to be so by the man who doubts every thing but his senses. In the latter cases, however, the same principle is farther involved, as in the former; since we think there is proof that the atmosphere of a febrile patient may render the properties of life more susceptible to the operation of the *predisposing* causes either of that, or of any other disease, and may clearly operate as an immediate exciting cause when the system is thus predisposed. It is in this way, especially, that the fœtid dejections in typhoid and other malarious fevers have suddenly developed a corresponding disease in a transient visiter. The learned reader will readily recall some remarkable examples; and, we are disposed to think that the case of Dr. Geary, which we have mentioned in our first volume, page 473, is of this nature. The laws of life and the laws of chemistry are as wide as the poles from each other. No organic action can form the chemical combinations of dead matter. It cannot be done even by vegetable organization, which is fundamentally concerned with the elements of inorganic substances. The morbific agents, therefore, which are supplied by the forces of chemistry, being never generated by the vital forces, the diseases which are the result of those agents can never propagate themselves. (Vol. I.

(1) Hydrophobia, inflammations from healthy animal poisons, &c., are exceptions, if included among contagious complaints. They furnish examples, however, of specific modifications of the vital properties.

p. 451—453, *note.*) We must, therefore, regard only in the light of assumption all speculations like the following:—

"In a great majority of cases of typhoid fever," says Professor Alison, "we *know* that a matter, similar in its effects on the human system to that which excited the disease, is ultimately evolved in a large quantity from the blood, making the disease contagious; i. e., the morbific poison in one way or another is *multiplied in the blood of the living body.*" And this is one of the three "*facts*" which he says "may be stated in favour of this form of the humoral pathology of fever." (1)

But, we can in no way establish conviction so well, as by showing how fact after fact has ultimately triumphed in bringing the most enlightened minds to a tacit acknowledgment of the great principles which we have just stated. We find, for instance, Dr. Armstrong slowly renouncing his firm belief in the contagion of typhus fever, upon which he had erected important hypotheses of a practical nature;—that

"The opinion clung to me closely, and I parted with it gradually, if not with regret," and that, finally, "having discovered my error, the only reparation I can make is by such an acknowldgment." (2)

And turning to our own still greater philosopher, we hear him in the same honourable and humble manner,

"Begging forgiveness of the friends of science and humanity, if the publication of that opinion, (contagiousness of yellow fever and plague,) has had any influence in increasing the misery and mortality attendant upon those diseases. Indeed, such is the pain he feels in recollecting that he ever entertained or propagated it, that it will long, and perhaps always, deprive him of the pleasure he might otherwise have derived from a review of his attempts to fulfil the public duties of his profession." (3)

In speaking of the "practice of the Spaniards and Portuguese, in burning the clothes of persons who died of consumption," and of the analogous acts, "sanctioned by the advice or orders of public bodies in the United States," he says "they are, in both countries, marks of the superstition of medicine." (4)

And yet another, justly honoured in the walks of science. In 1822, Dr. J. B. Beck, in his report on the "fever at Middletown, Ct." remarks, that,

"In looking around the city for any domestic cause which might have produced them (the cases,) I found none that was adequate." "There is nothing

(1) Outlines of Physiology and Pathology, p. 521; 1833.

(2) Lectures on Acute and Chronic Diseases, vol. ii. pp. 113, 114—130.

(3) Lon. Med. and Physical Journal, vol. xv. p. 271; 1806.—Also, Rush's Med. Inquiries and Observations, vol. iv. p. 270, 1805; and New-York Med. Repository, vol. vi. p. 135—150, 1803; where this change of opinion is more fully set forth.

(4) Medical Inquiries and Observations, vol. ii. p. 80.

about this place, (the ship-yard, &c.) which even the most distempered imagination could convert into a source of infection. Indeed, nothing seems capable of explaining the facts on this subject, but to admit, *with the exception of the first two cases, which occurred anteriorly*, the existence of malignant fever, in Middletown, was owing to the presence of this vessel from the West Indies. Otherwise it is unaccountable why, in a place where there is a total absence of all the causes contended for by those who maintain the doctrine of domestic origin," (1) &c.

In 1823, we hear from the same writer, that,—

"It was with the deepest solicitude, that we watched the origin and progress of the late pestilence, which desolated the fairest portion of our city; (the yellow fever of 1822.) And during its course, facts so abundant and striking were developed, as to produce a positive and complete conviction upon our minds, that *contagion had nothing to do* in the propagation of the disease." (2) (Vol. I. p. 451, *note*.)

Armstrong finally *ridicules* the doctrine which he had abandoned "with regret." He also denounces it "as the most cold, the most cruel, the most calculating doctrine that ever was advanced." And, as to "the quarantine laws," they "are perfectly absurd." But what is more remarkable, he falls, at once, into the greater error of confounding the "marsh intermittent" with typhus fever. Had he studied those "black-letter books" for which he expresses "contemptuous views" on the very page where he renounces one doctrine and takes up the other, he would have found in Hippocrates all that would have been necessary to have satisfied him fully of their distinctions. Nevertheless, he

"Beseeches us to take nothing for truth but what our own observation proves to be so; and believes that the doctrine of contagion is taken up by medical men as it was by himself, from the prejudices of education."

But, what strikes us as liable to exception on an occasion so solemn, and in a teacher of powerful influence, and after having not only positively affirmed that he had "entirely altered his opinion of the contagion of typhus fever," but having also devoted some twenty pages in the way of proof, and derision of the doctrine, is the evidence which he affords at the close of the chapter of his having "parted with the doctrine gradually, if not with regret," in the declaration that, "it remains in my mind a problem to be solved whether typhus fever is ever contagious or not." (3)

Turning again to our "American Sydenham," who has been thus so justly distinguished, we find him prognosticating, as the re-

(1) New-York Medical and Physical Journ. 1822, p. 166.
(2) Ibid. 1823, p. 473. (3) Lectures, &c. vol. ii. pp. 113, 128, 129.

sult of his conversion, "the immense benefits which await *this mighty achievement of our science* upon the affairs of the globe." It was not, however, in any respect, with either of the distinguished individuals, an "*achievement af science.*" Science had no part or lot in the matter. It had been resisted to the last; till conviction became the unwilling result of a series of most obstinate facts. And yet, it is truly remarkable, that such illustrious advocates for principles in medicine should not have felt the force of their philosophy, and have permitted science to have anticipated their specific facts. Must we forever await demonstration, where principles apply with their fullest force?

In all directions, we continue to see the doctrine of contagion as it respects typhus, and yellow fevers, and the plague, abandoned, and quarantine laws treated with derision. (Vol. I. pp. 451 — 452, 532 — 533.)

"*Let yellow fever,*" says the venerable and experienced Ferguson, "*be expunged forever from the list of importable and transportable contagions.*" (1)

Even Africa, from her very hot-bed of pestilence, beards the Metropolis of Europe:

"Dr. ——— of Alexandria," says Dr. Valentine Mott, "stated to us, that he sent the clothes and mattrass, of a person who had died of plague, to London, and that a quantity of the discharge from the charbons and buboes was mingled with it purposely. It arrived safe, was taken home, but no disease communicated by it. Hospital pupils, who are extensively employed in dissecting subjects dead of the plague, never contract the disease." "From the facts which I collected at Cairo, Alexandria, Smyrna, and Constantinople, in each of which places the disease existed, and in the first of which I saw a number of cases, my belief is, that it is not contagious." (2)

In addition to what we have said in our first volume, as to the existing testimony against the supposed production of disease by putrid animal exhalations, we may say that the opinion is controverted even by Chisholm; (3) whilst Professor Dunglinson, after much personal and accurate observation, concludes that, "as a cause of malarious fever, putrid animal emanations cannot, we conceive, be invoked." (4) If this be true, what becomes of the bearing of injections into the veins of putrid animal matter, as an exemplification of the origin or pathology of "adynamic" or any other fevers. (5) (Humoral Pathology, Sec. 9, 10, and 11.)

(1) Med. Chir. Rev. Lon. Jan. 1840, p. 306.
(2) American Journ. of the Med. Sciences, Aug. 1839, p. 373.
(3) On Contagion, in Edin. Med. and Surg. Journ. 1810.
(4) American Med. Intelligencer, 1837, p. 162.
(5) We see that Duchaletet's array of facts, although backed by M. Andral, (Vol.

We have dwelt upon this subject on account of its importance to fundamental principles. The precise alterations of the properties of life, which constitute the different species or modifications of fever, require as much precise causes or modifications of those causes, as the more uniform alterations of the same properties require an uniform morbific agent in smallpox. In stating these principles, the reader should bear constantly in recollection that we are allowing nothing for the contingent influences of local congestions, inflammations, &c., which more or less obscure the phenomena of idiopathic fever. And we may now remark the reason *why the morbific agents which are generated by chemical forces are so constantly varied, and establishing so many shades of difference in fevers of the same species; whilst those which result from the vital powers are distinguished for their greater uniformity, and, therefore, establish the changes in life in a uniform manner.* The forces of chemistry operate, as it were, at random, and slight variations in the proportions of only two elements may form the most opposite combinations. One may be atmospheric air, the other a powerful acid. And we may carry these two results to a full exemplification of our principles, — one of the combinations so affecting the organization as to constitute health, the other destroying organization. On the other hand, the forces of life operate only through a fixed and highly elaborated system of matter. The nature and uniformity of the latter gives determination to the actions of the former, whilst we find in this fact an equal proof that the powers which animate the organized matter are founded in the same principle of consistent adaptation. Nor will we neglect saying, that the considerations which we have just made completely establish an absolute distinction between the two systems of forces, and we may engraft them upon our essay on the Vital Powers.

In proportion, therefore, to the affinities which may exist among the morbific causes of idiopathic fevers, and those local inflammations and congestions which may depend upon external causes, there will be, cæteris paribus, approximations in the modes in which the vital forces will be altered. The at-

i. p. 524, is impugned much in the same way that Bancroft suffered. This may have its temporary influence, and the fabric which they, and others, have reared, may, like the sun, be occasionally obscured in a mist; but there it will stand, sooner or later commanding universal homage.

tending circumstances of one climate will promote in a certain way the dissolution of vegetable matter, and the recombination of its elements in a manner that may be nearly uniform. This being morbific, will determine, as expounded by Hippocrates, nearly an uniform alteration of the vital properties. (P. 427.) Or, if it be preferred, the same agencies will determine causes of some other nature than the supposed miasmatic, which will bring about coincident results. There is no difference in principle. At other times, the morbific causes may be varied, and there will be corresponding variations in the changes which they establish in the powers of life. In another climate, the results of decomposition and recombination are varied by other agencies, and, as they differ from those of the former, so will the influences differ which they exert upon life. Thus the fevers of two climates may be greatly different from each other, or they may be connected by close resemblances. The phenomena indicate the distinctions, and carry us back to an investigation of the remote causes with which the science of pathology must, if we may say so, ultimately begin. We need not remark how ably this subject has been investigated by our distinguished countryman, Dr. Miller, and subsequently illustrated by our learned professor Joseph M. Smith. But, if high commendation be due to the philosophy which marks the investigation of the remote causes of disease, at this enlightened era, what measure of honour shall be meted to him of old who anticipated all that is now known of that important subject, who has made it the groundwork of all his writings, and which gave such a direction to his practical views as to obtain for him, through all subsequent time, the solitary distinction of the "*senex divinus*"?

Considering the differences which prevail among fevers, inflammations, and venous congestions, it is remarkable that the general principles of treatment are nearly the same, respectively, in each of the denominations. This necessarily proceeds from certain degrees of coincidence in the morbid alterations, whilst these affinities arise from a principle in the forces of life by which they are adapted to equal influences from a vast variety of agents, and by which the organized system is thus maintained in one uniform condition; and, although the properties of life are liable to alterations by the same agents when operating in excess, or from others more directly morbific, they must still manifest approximations to their natural actions, since their essence cannot

be changed. There are, also, great classes of remote causes which have analogous influences upon the properties of life, and when any number of either of the great divisions establish morbid alterations, the changes will be more or less alike, and manifest themselves in some of the specific or modified forms of fever, or in those of inflammations or venous congestions; the latter being only an inflammation of a particular tissue, whose natural properties differ so much from those of other tissues as to result in phenomena, when the seat of inflammation, which differ from those of other tissues when affected in like manner. (P. 178, Vol. I. p. 542, 568 — 575, 626, *note.*)

SECTION XIII.

CONDITION OF THE VARIOUS TISSUES OF A CONGESTED ORGAN IN THE PRIMARY STAGE. — INCIPIENT SEAT OF VENOUS CONGESTION.

The derangements which attend the organic functions of parts which are the seat of venous congestions are sometimes consecutive to the venous disease, and at other times antecedent, and one of its predisposing or exciting causes. Whenever the venous affection takes place, it necessarily exerts an influence more or less profound upon the whole organization of the part. For this reason, the various phenomena of the brain, in all its congestions, are more or less modified; and constitutional disturbances arise as well from the general lesion of the brain as from the direct influences of its congested veins. We apprehend, too, that the influences upon the general constitution of the congested part will vary according to the modified form of the pathological state of the affected veins, as well, also, through the antecedent condition of the organ, whether as it respects its normal or modified state; and, that we shall find in this explanation a reason why venous congestions contribute much to that modification of the cerebral powers by which apoplexy is so readily determined at certain periods of life.

Bichat, who has now and then a brief allusion to venous congestions, had evidently reflected more upon the subject than is apparent from his writings. He has a paragraph in relation to our present inquiry, which shows his opinion as to the suscepti-

bility of the venous tissue. We shall quote it, as a light beaming from a profound observer upon this intricate part of our subject.

"As to the influence," he says, "that the other organs, when affected, exert upon the veins, we know but very little. As they are everywhere disseminated, like the arteries and the nerves, it is difficult often to know if it is *the vein itself* or the *organ* which is the seat of the *sympathetic* phenomenon."

He says, however, that the veins are not "frequently" the seat of inflammation, and "therefore we know but little of the influence they exert upon the other textures." But, an important remark follows in the next sentence, which evinces what we have said of the exuberance of the vital forces in the venous tissue, and the sympathetic influences they are capable of exerting upon distant parts. Thus, —

"When we transfuse substances into the vessels, we have often seen acrid and irritating ones upon being introduced *into the veins*, produce sudden convulsions in different muscles." (1)

Here is an important fact, which we must stop, for a moment, to examine in its different aspects. When the irritating matter is injected into the arteries it produces no such effect as is ascribed to the veins. This is an interesting additional proof as to what we have stated in relation to the vital properties of these two systems of vessels, and the relative influence of morbid states of the veins upon other and distant parts. It shows, also, that a morbid state is suddenly developed in the venous tissue; and, if it be capable of affecting so profoundly the muscular system, what must be its influence upon the organic viscera; and if great upon the latter, how apparent it becomes that corresponding derangements may be readily and suddenly established in distant parts of its own system, and thus resolve the whole mystery as to secondary abscesses. (Pp. 248, 279, 283, 290, 333, 351, 364, 369, 454; and Vol. I. pp. 125, 131, 134 — 138, 515 — 519, 524, 539, 568 — 575.)

In all venous congestions of the liver, of any moment, the secretions of that organ are changed in their natural character; and, whenever the venous affection exists in severity, the formation of bile is greatly suspended. This function, however, may be often immediately restored by an abstraction of blood that shall impress the system decisively. Nature now comes in with her mode of depletion to finish what art had so well begun. And, without again adverting to the proof of our doctrine which is supplied by the curative effects of bloodletting, we may claim

(1) General Anatomy, &c. vol. i. pp. 424, 425.

from nature a corresponding evidence. So, in the aggravated forms of hepatic congestion, especially if the mucous tract of the bowels be involved in the same condition of disease, irritating cathartics, and even calomel, particularly without the preliminary loss of blood, will often aggravate the hepatic affection; and this we hold to be another proof of the vital dependences of venous congestion. If the brain happen, also, to be simultaneously the seat of congestion, the irritation may be propagated to that organ; and, as we have known, one cathartic after another, each rendering the last more difficult in its purgative effects, will end the life of the patient. (Vol. I. p. 273.)

Allowing, therefore, what we have now said, it follows that the early constitutional results, which attend simple venous congestions, are determined not only by the venous affection, but by a general derangement of the congested part.

The most important of the chylopoietic viscera, in latitudes south of 40°, are particularly liable to venous congestions in their simple form, and as complicated with idiopathic fever, whilst the other abdominal viscera are remarkably exempt. No physical principles can resolve the phenomenon. We look for the cause in the greater endowment of the former with the vital properties, and, consequently, their greater susceptibility to the action of morbific agents. (Pp. 333, 335, 362, 363.) This, too, may be equally affirmed of common inflammation in relation to these parts. Here, also, it is probable that the morbid process generally begins in the universal organ, and that the venous affection may be an ulterior result. The entire system, too, of the most essential organs (the stomach, intestines, and liver,) is often simultaneously involved in a state of congestion. This is another remarkable phenomenon, which can be explained only upon the principles just stated. We find among these organs strong sympathetic relations, through which they all conspire towards a certain result, viz. the conversion of food into chyle. Of the physical media of connection, none is so remarkable, though not the most important, as the vena porta, ramifying at one extremity upon the intestinal tube, and within the liver at the other. The mucous membrane is another important medium of sympathies; and from these anatomical and vital relations, therefore, and the analogy of their functions, it would seem probable, that when one of them becomes diseased, the other should suffer.(1) This

(1) We see this principle prevailing remarkably amongst other cöordinate parts,

we often find to be true. In like manner, also, where the venous system of one organ becomes the seat of congestion, it would seem not less probable, especially from the well known disposition of the venous tissue to rapid and diffuse inflammation, that the congested state would become coextensive with the whole. For here, also, the veins are not only bound together in an intimate and peculiar manner, but the sympathy which grows out of their anatomical relations is farther established by the relations amongst the organs in which they arise and terminate. But, since the liver is composed "of a solid and granulated substance," it is scarcely probable that its veins will acquire the same enlargement as in the other organs; and this may not be obvious on dissection, since the blood may gravitate after death into the inferior branches. This, from the form and position of the liver, we apprehend is the reason why some observers, like Kiernan, and Annesley, have found the hepatic veins most frequently congested. (P. 420.)

We shall have seen that venous congestion passes readily into common inflammation. It is highly probable, therefore, under the circumstances just mentioned, that we should often meet in connection with the congested veins, more or less of common inflammation (p. 217, *note*,) in those parts which have been most congested, and which are most highly endowed with the vital forces. Such is very often the fact. The irritable mucous membrane of the bowels, and of the stomach also, becomes inflamed, whilst the more indolent liver remains affected in its venous system only. It is exceedingly important to understand this complication, which is so common in all miasmatic countries. The symptoms of muco-gastritis, or of muco-enteritis, may be so allied to the congestive, that where the cadaverous appearances denote the presence of the former, the superficial inquirer may pass the whole to the account of common inflammation.

How will the foregoing remarks apply to active inflammation of the visceral veins? We think only so far as they are comparatively susceptible of active and sub-active inflammation. To the latter they are very liable; to the former but rarely. Where, therefore, a mode of disease is only seldom induced, we shall not so often witness a corresponding sympathetic development in other parts of the same tissue, as when it is the seat of an affection to which it is far more liable. Thus, the mammæ are

as between the uterus and mammæ, in their natural, preternatural, and morbid states; and this, too, where there are no special anatomical relations.

variously affected during utero-gestation, or after parturition, or at the beginning of menstruation; but, in other abnormal states of the womb they are but little liable to sympathetic influences. (Vol. I. pp. 157—160, 474—479, 492, 568—572.)

Where is the incipient seat of congestion in the affected veins? There appears to be much ground for believing that the inflammatory action begins in the capillary veins, and that it is subsequently propagated to their trunks. This appears to be probable from the extent in which the venous system of the part is generally and simultaneously involved. This also corresponds with what we have said of the natural function of these vessels in relation to the varying proportions of transmitted blood. (Sec. 10.) When the larger veins are the seat of accumulated blood, as in varix, they are commonly isolated. Nor does venous congestion affect the *largest* series; but, it is generally limited to some complex vital organ, where we may suppose that the capillary veins are more highly endowed with the properties of life, than in "non-vital" parts, and where remote causes, external or internal, may therefore operate upon them with greater intensity, or any general derangement of the organ may develope in them the morbid action. The capillaries are the instruments of vital actions, and of all diseases. This is obviously true of the arterial and excretory systems, and analogy, as well as the general office of the veins, show that the vital forces are more strongly pronounced in the venous capillaries than in the venous trunks. M. Cruveilhier, as we have seen, regards them as the instruments of disease in cancer, and has more or less infused the doctrine of capillary phlebitis into the pathology of other affections; whilst many others consider them the seat of erysipelas. (See *Appendix* on *Erysipelas.*) And here the researches of Douglass, (1) and the analogous ones of others, are worthy of much consideration; since they appear to have ascertained that the visceral abscesses which ensue upon phlebitis in some remote part depend upon inflammation of the capillary veins; where, also, it should not be neglected, the venous trunks are in a state of dilatation. The same conclusions are indicated by what we have said of uterine phlebitis consequent on parturition.

Why the "non-vital" parts are most frequently the seat of active phlebitis, seeing that their tonic forces are less strongly pro-

(1) On Phlebitis, particularly as connected with Secondary Abscess.

nounced than in the organic viscera, we have already endeavoured to explain upon physiological principles; in connection with the particular circumstances which attend the operation of morbific agents.

SECTION XIV.

VENOUS CONGESTION PASSES INTO INFLAMMATION OF OTHER TISSUES.

It is a well determined fact, that venous congestion passes readily into common inflammation, (as we have called it in contradistinction to venous congestion, p. 217 *note*,) and that they often coexist in the same organ.

"We would particularly remark," says Annesley, "that congestion (venous) of the liver very often gives rise to inflammation of the organ." "Congestion may suddenly terminate in the inflammatory state after but a *brief* duration." "The connection existing between congestion (venous) and inflammation is well illustrated by some of our cases, and the passage of vascular turgescence into inflammation is shown by them." "A knowledge of *both* states of the disorder" he considers "important, inasmuch as a *suspicion even of congestion* ought to lead us to the adoption of such remedies as should prevent the supervention of consequences so much to be dreaded as inflammation of the substance of the liver." (1)

Mr. Twining makes the same remark in respect to the remittent fevers of India, in which the venous congestions of the liver, spleen, brain, lungs, stomach and intestines, and the root of the mesocolon, "pass rapidly into inflammation." (2)

Our friend, Mr. T. R. Colledge, senior surgeon to Her Britannic Majesty's Commission in China, informs us that in the intermittents and remittents of the southern coast of that country, the venous system, especially of the brain, is much injected, and this is the principal morbid appearance where the patients die early. There is then no cadaverous mark of inflammation. But, when life is more prolonged, the morbid vascularity is shifted wholly or in part to the capillary arteries. He was so strongly impressed with the uniformity of this succession, that it was communicated to us without inquiry, and simply as an interesting anatomical fact.

This tendency of venous congestion to pass into inflammation is often adverted to by Dr. Armstrong. Thus,—

(1) Diseases of India, vol. i. pp. 343, 348, 350, 403, etc.
(2) Clinical Illustrations of the Most Important Diseases of Bengal, &c. vol. ii. p. 291.

"If stimulants be administered too freely during or after the employment of depletion, they may excite the heart and arteries too much, and produce either a simple fever, or one complicated with inflammation; the last of which is of the *most frequent occurrence*, probably because the parts which had before been preternaturally distended with *venous congestion, are very readily roused into inflammation.*" (1)

Now, we ask, why is all this so? Will any mechanical hypothesis explain it? We mean, *intelligibly*. Here the reader should revert to Dr. Armstrong's mechanical rationale of the relief of venous congestions by exciting the force of the general circulation. (P. 271.)

If, then, there be no obstruction to the free current of venous blood in congestion, and the causes which determine venous circulation prevent all accumulations beyond what may arise from the natural or morbid dilatations of the veins, how does inflammation so readily and so rapidly supervene on venous congestion? Not, of course, from the irritation of mechanical distension, since there is no obstruction. Besides, whenever even varices are tied, they produce no inflammation of the adjacent tissue, unless it begin in the veins. So, also, of ligatures upon the jugular, or other veins. (P. 248—257.)

If our doctrine of venous congestion be well founded, the rationale is apparently obvious. The congestion passes into common inflammation partly by continuous and remote sympathy, arising from the inflammatory action in the veins. Besides the evidences of strong sympathetic relations betwixt the venous and arterial systems which we have endeavoured to exhibit, it might be inferred from their continuity and common office, that when the tissue of the capillary system of one series becomes inflamed, it would stimulate the action of the other, and where, as in the arterial capillaries, the actions of life are mainly carried on, this stimulation would ultimately result in the same modified action as follows other analogous causes of inflammation. Precisely the same principle is seen in operation in relation to the veins in the normal state of the circulation, being only modified according to the natural function of the veins. This function consists in receiving the blood from a series of the arterial capillaries; and the veins, being endowed with properties for the fulfilment of this office, dilate and contract in instantaneous obedience to every varying change in the quantity of blood which is presented to the venous radicles. But they do not

(1) On Congestive Typhus, sec. 6, p. 120.

take on the same actions to which the arterial capillaries are liable in venous congestion for the obvious reason that their natural function is wholly different.

As soon, therefore, as the supposed influence is established upon the capillary arteries, a reaction of sympathy takes place in the veins, by which their morbid state is overcome; when their contraction follows, and the "balance of the circulation" is more or less restored. The animal system is full of parallel examples in its diseased conditions. It may be illustrated by the effect of blisters in relieving internal inflammations and congestions, and we see the principle in the subsidence of chronic gastritis when phthisis supervenes. Nevertheless, as we have endeavoured to indicate, an enlargement of the veins may remain, corresponding with the increased quantity of blood which is now transmitted; though, for reasons also stated, it is more apt to be expelled from the veins in proportion as those vessels are restored to their normal state. (Pp. 145—146, 252—255, 267, 369.)

If such be not the principle by which a contraction of the congested veins is produced, can the phenomenon be explained by the existing theories? They suppose the veins to be in a state of "passive relaxation," that they are "distended beyond their power of contraction," and they commonly superadd to those conditions some obstruction to the venous circulation in the congested part. How, then, is such a state, or either of these conditions, to be overcome by the additional quantity of blood which is discharged into the veins when inflammation supervenes; especially if the doctrine be true that the arterial current moves with diminished force? In any event, however, when inflammation begins, the distension of the veins, whether from simple relaxation or obstruction, should increase somewhat in the ratio of the triply increased quantity of blood, (p. 145, *note* 5;) or, rather, the resistance increasing in proportion as the tension of the veins is increased, there should be a recoil upon the capillary arteries, and these influences, by increasing irritation, should extend in an increasing ratio till a very different train of phenomena would ensue from what has ever attended any process of disease.

The predisposition to common inflammation of which we have now spoken is, also, in numerous instances promoted by the rising force of the general circulation. The circulatory organs are often greatly prostrated during the unresistsd influence of venous congestion. But when, by stimulants, or by the abstrac-

tion of blood, no matter which, the energy of the heart and arteries is increased, the renewed force of the circulation, operating upon the predisposed arterial vessels, may suddenly determine an active inflammatory state in those vessels, whilst the congestion of the veins may as suddenly yield upon the principle before mentioned. (Vol. I. pp. 132, 134, 138, 145, 189, 200, 209, 213.) It is in this way that we must account for the phenomenon of "seeing the blood-vessels, after venesection, in a common phlegmon, travel back again, from a tendency to mortification, to the red colour and pain of common inflammation." (1)

At other times, the influence which is propagated to the capillary arteries is short of producing inflammation, which, however, may sensibly increase the action of the larger vessels; as we often find to be especially the case in congestions of the brain and liver, where the carotids and abdominal arteries pulsate with great violence, although the general circulation be prostrated. "*Quod pulsatio in precordiis manifestat;*" (2) *or, as Hippocrates has it,* φλεβῶν σφυγμοὶ παρ ὀμφαλὸν. (3) And here it is worthy of remark, were there any foundation for Armstrong's partial doctrine, that the rising force of the circulation overcomes the venous congestion, (p. 271,) that it should yield under the foregoing circumstances; whilst, on the contrary, it is actually increased by this very contingency, unless absolute inflammation supervene. This is shown on dissection; the veins being in a turged state, whilst the corresponding capillary arteries have no sensible vascularity.

For the foregoing reasons, it happens that inflammation, in various degrees, may coexist with venous congestion in the same part; the latter yielding, however, in the ratio in which the former supervenes. These views, we think, tend to explain the philosophy, why, according to Dr. Hodgkin,

"Some have considered fungoid disease principally arterial, and others venous. I will not attempt to decide to which class of vessels they are most nearly allied," &c.; but, "it seems probable that accidental or fortuitous circumstances have the principal share in determining to which class of vessels these capillaries should most incline." (4)

(1) Rush's Medical Inquiries and Observations, vol. iv. p. 300.

(2) Aretæus, de Acut. &c. l. 2, c. 8. (3) De. Morb. Pop. l. 4, ver. 129.

(4) Lectures on the Morbid Anat. of Serous and Mucous Membranes, vol. i. p. 287.

Venous congestion of the liver and alimentary canal in connection with inflammation of the intestinal mucous membrane is a common phenomenon; and forms an obstinate, and often formidable condition of disease. In miasmatic countries, where

The disposition to a state of common inflammation is frequently evinced by its supervening in the congested part after the congestion has been overcome by depleting remedies; especially when stimulants have been exhibited "to build up the strength," or animal food prematurely permitted.

When inflammation ensues upon venous congestion, a comparatively mild disease may be thus constituted. Armstrong notices this fact, even when the change is effected by means of stimulants. (Vol. I. pp. 145, 202, 222—232, &c.)

"If the remedies (bloodletting or stimulants) be efficient, they produce a condition widely different from the original one of congestion. In short, they establish a new condition in the system, which may at once lead to convalescence, or be followed by a simple or an inflammatory excitement of the heart and arteries." (1)

the predisposing causes have been of a malarious nature, a tendency to the disease is apt to remain for a series of years, and the subjects, of course, liable to repeated attacks,—especially when the *exciting* causes are not carefully avoided.

It may begin in the liver in the form of venous congestion, the symptoms being commonly insidious at first, or, in the mucous tissue of the bowels in that of inflammation, or both conditions may spring up simultaneously. The former is generally antecedent, especially when malaria has been the exciting cause.

This class of abdominal affections possess, in America, the highest interest. The complications of the venous with the mucous inflammation impart an obstinacy, and a difficulty in treatment, which are comparatively rare in simple muco-enteritis. We rarely saw the former during five years in Montreal, often in the city of New-York, especially for some years before the appearance of the cholera; but since the subsidence of that disease, venous congestions have become less frequent and less obstinate in the city. In western New-York we have seen the worst conditions of the foregoing complication. It is especially incident to children. It demands bloodletting more imperatively than simple muco-enteritis, although the symptoms may be less strongly pronounced from the mucous inflammation being more or less disguised by the congestive. The lancet is far preferable to leeches, especially after the age of one or two years, and at all ages, if there be general arterial excitement. Such has been, for many years, our almost uniform practice. Without this remedy, most others, especially cathartics, are very apt to aggravate the whole condition of disease. Calomel, in one or two moderate doses, may be generally given soon after bloodletting; but its subsequent repetition demands great caution. If the dejections still contain much mucous, this medicine is apt to exasperate the disease. One of its bad results is that of frequently arresting, or nearly so, the biliary secretion, and increasing that of mucus. When carried much too far in these cases, it sometimes produces large dejections of mucus resembling hydatids. The only remedy that can then save life is bloodletting, or leeches to the anus. (See pp. 111, 275, *notes*.) Senna is pernicious in the foregoing cases, and so is rheubarb. Castor oil is wholly the best; but the best of all is to administer but little medicine internally, to repeat the bloodletting, or apply leeches, or blisters to the abdomen, if the symptoms be obstinate, keep the patient horizontal, and confine him to a diet of toast-water, and this in moderation. The disease will generally yield, and often rapidly, under this treatment, when it will be prolonged or rendered fatal by any other.

(1) On Typhus Fever, sec. 6, p. 119.

In any event, should inflammation supervene, the chances of relief will be often greater in the hands of such as forever mistake the prostrating effects of acute disease for "debility." The energies of the vital powers are now up to an encouraging point, (p. 221,) the evidences of inflammation are more strikingly pronounced, and the system under that influence which sustains, without faltering, the bloodletting which now takes the place of tonics and stimulants. (*Bloodletting*, Sec. 3, 5, 7, 9, 11.)

SECTION XV.

ACTIVE AND PASSIVE INFLAMMATION AND CONGESTION.

"The term DISEASE OF DEBILITY is much too vague for scientific discussion." — ALISON. Trans. Med. Chir. Socy. Edin. 1824, p. 381. (See pp. 145, 175, 243, 261.)

PERHAPS we have already said enough on the common distinctions which are made of inflammation into an active and passive kind, by the few who recognise the former. (1) The term *passive* implies an absence of action, and is intended by those who use it to inculcate that mechanical condition of the blood-vessels to the refutation of which we have devoted one of our essays. If what we have there said be well founded, there can be no such thing as passive inflammation. Referring, therefore, to our Essay on the Theories of Inflammation, we shall limit ourselves mainly, at present, to the bearing of this subject upon venous congestion; and, although we shall be especially concerned about the philosophy of common inflammation, in the sense in which we have employed this term, (p. 217, *note*,) the same principles will equally apply to the philosophy of venous congestion.

Although we have before us many eminent authors who speak of an active and passive state of inflammation as things manifestly in absolute opposition, yet they do not appear to designate the distinction. The difference is supposed to be denoted by the results of opposite remedies, although in that variety which is assumed to be *passive* and the antipodes of what is denominated *active*, and where, too, the capillaries are supposed to be blocked

(1) It should be said, however, that asthenic inflammation is rarely admitted in America.

up with coagulated blood, each class of remedies run into each other, if properly administered, in almost every case. The identical vital signs, and the same morbid products in both cases, are left out of consideration. In the *active* kind, the redundancies of serum, of mucus, lymph, &c., are sometimes allowed to depend on a vital process, although the instruments of action are supposed to be in a paralysed state, blocked up, and without action. Nor is it the least peculiarity of this doctrine, that it supposes the various effusions to be in the ratio of the mechanical obstruction, and that, as this increases, the grosser secretions will be most likely to appear, and in their greatest abundance. (*Essay on Inflammation.*) In the *passive* kind, another law is supposed to prevail, which has the merit of being more consonant with the supposed pathology of inflammation. Here we have a consistent mechanical philosophy, and the various physical products are regarded as mere "filtrations from the blood." We would not be so unjust, however, as to say that this philosophy is not frequently carried out in the former case; and it is with the consistent philosophers of this school that we have especially held our argument in our "Theories of Inflammation." The absolute pathology, therefore, with those who hold the doctrine of *active* and *passive* inflammation, is essentially the same; the distinction reposing alone upon the varying results of treatment, or as the antiphlogistic or stimulant plan may happen to succeed in any given case, where a judicious combination of both, or that which had been neglected, might have succeeded better. (Vol. I. p. 292, &c.) For this reason, such as have never "wetted a lancet" restrict their pathology of inflammation to the condition which is demanded by the mechanical rationale.

It strikes us, however, as being not a little remarkable as to the philosophy which predicates opposite conditions of the *same* disease of some differences in the details of treatment, that it should deny the presence of inflammation in many cases, as in tubercle for instance, where the vital signs, and the morbid products, are similar to those of acknowledged inflammation, and where the depletive treatment, in the active stages of the affections, is the only successful practice. (See APPENDIX 5, *Tubercle.*) In our 11th section on the Philosophy of Bloodletting, we have examined a branch of the passive doctrine, where many observations which we then made are more or less applicable

to our present inquiry, as are also certain parts of our Essay on the writings of the distinguished French philosopher.

M. Andral is the only eminent observer who has provided us with a tangible exemplification of the rationale of *active* and *passive* inflammation. True, he discards inflammation as a disease; but he has substituted another having all the signs of the exploded affection, and his method of demonstrating the opposite conditions must be allowed to be the most intelligible that has yet been offered. We are also justified in again referring to our author, by the distinguished rank which he holds in the anatomical school; and since, especially, his researches in morbid anatomy have contributed largely towards exploding altogether, in certain quarters, such a disease as inflammation, and more universally in extending the laws of physics to the processes of life. The first step in the revolution was to abolish the term inflammation, and to substitute that of *hyperæmia*. "I shall employ," says M. Andral, "the term *hyperæmia* to designate the increased quantity of blood, *without any reference to the* CAUSE *which produces the accumulation*." This is doubtless an improvement upon Hunter. The term hyperæmia is also indiscriminately applied to certain healthy conditions of the body; and has the farther peculiarity of presupposing that every inflammation must have for its essential pathological cause an "accumulation" of blood in the seat of the affection. We may illustrate, by a quotation, our author's views of one branch of hyperæmia, which is known in common books by the name of inflammation. Thus:—

"This supervention of acute hyperæmia is one of the most frequent causes of the sudden deaths which occur during the progress of various chronic affections."

To render this condition more intelligible to the uninitiated, and to distinguish it from *healthy* hyperæmia, which "passes by insensible gradations into the pathological," our author sometimes calls it inflammation." (1)

We are now interested about this *morbid* state of "hyperæmia," which our author, following the usage as to inflammation, divides into "*sthenic* and *asthenic*," or two opposite conditions of disease. Now, in the first place it is perfectly contradictory to all that is known of physiology, and to the laws of causation, to suppose that the same phenomena may arise from opposite

(1) Patholog. Anat. vol. i. pp. 10, 11, 15.

states of the vital properties. (1) Unlike the physical powers, however, these forces are acted upon by foreign agents, (Vol. I. pp. 30, 31,) and their nature is more or less affected; and this, too, according to those shades of difference which naturally distinguish the same forces in the different tissues. There may, therefore, be various modifications of a common action. Yet it is always the same individual, only changing its complexion; and, whilst it presents a generally uniform aspect, its near identity in this respect cannot be denied. Modifications of a general principle of treatment may be determined by inscrutable peculiarities in the condition of the vital forces, where the assemblage of symptoms in the different cases is almost precisely the same; and, since it is mainly upon such variations of treatment that the distinction of passive and active inflammation has been founded, we may at once show the fallacy of the induction by referring to what we have said of the curative effects of the bark in pleurisy, enteritis, &c., where miasmatic, or some other unusual remote cause, has thrown its influence upon the affected properties. (Pp. 471, 504, and Vol. I. pp. 242—246, 303—305.) Here, every phenomenon may be exactly the same as in the common forms of the disease, in which bark would be pernicious. And yet, according to the philosophy which establishes the distinction of active and passive inflammation, the foregoing modification should be ranked with the latter division. But, however varied may be some of the remedies in the different modifications of inflammation, the general principles of treatment, as we have endeavoured to show, are substantially the same when the phenomena are analogous. It is only in the foregoing sense that the precept of Santorinus can be admitted, — "prescribe si placet, eodem eadem morbo remedia, et quæ uni profutura, alteri inimica experieris." (2) The favourable action of local or conconstitutional stimulants is no proof that there was not an increased action of the instruments of disease. It only proves its effect in *altering* the vital properties, as in the cases of inflammation just referred to, when the increased action of the vessels

(1) We concede that Bichat thought otherwise. But he saw that he was at war with nature, and asks, "*how can the same phenomena arise from two causes exactly opposite?*" (*a*) When he speaks of passive hemorrhages, &c., as contradistinguished from active, he loses sight altogether of his favourite vital forces. But, he does not make the mistake of Andral, of supposing that the blood exudes through inorganic pores. (Vol. i. p. 182.) (2) De Hæmorrhoidibus, s. 43.

(*a*) General Anat. vol. ii. p. 77.

subsides into their natural state. The vital properties had been altered in *kind* rather than *degree*, and stimulants alter them back again. (P. 349, *note*.) Therefore the frequent rapidity of those changes where inflammation may suddenly affect the most debilitated subjects, and be as suddenly removed by bloodletting; (Vol. I. p. 216, 292 — 293;) or, if now and then by stimulants, the favourable changes are too sudden to admit the belief that there had been a simply debilitated state of the powers of the affected part. Where real debility exists, the process of restoration is always slow.

Consider the principle in other aspects. The virus of small-pox and of cow-pox are supposed to be essentially the same, and yet how unlike in their constitutional effects, their eruptive characteristics, &c. Here the distinction has been established by inoculating the cow with the former, and is perpetuated, though it cannot be produced, by the human species.(1) (Vol. I. pp. 650, 697 — 698.) And again, in another respect: If a robust and a feeble person be inoculated with either virus, shall we have an active inflammation in one and a passive in the other? And so of measles, scarlatina, &c. The various and peculiar properties of medicines which have the closest affinity to each other, also illustrate, in their action upon the vital properties, the principle about which we are now concerned. There are, for instance, no two cathartics which are alike in this respect. (Vol. I. p. 560.)

We have seen that bloodletting has been employed extensively by Woodall, Lind, &c. in scorbutic subjects. (Vol. I. pp. 666, 668.) Here, the inflammation or congestion, of the gums, the spleen, liver, &c., grows out of, especially, a peculiar derangement of the alimentary canal, and is essentially maintained by the same cause. Whatever, therefore, will most successfully restore the functions of the stomach, will be most likely to remove the sympathetic developments. Change of food, change of air, agreeable moral influences, and, perhaps, even tonics, are among the remedies. But here, as in the foregoing case, the restoration from formidable disease, and great prostration of strength,

(1) We would not, however, prejudge this question; although when these diseases are considered in their known relations to each other, and the foregoing modifications are properly regarded, our argument remains unaffected, even though the diseases be individually sui generis.

is too rapid to admit the principles which concern the doctrine of debility. (1) (Ibid. p. 671 — 673.)

"If the hyperæmia be kept up," says M. Andral, "solely by passive dilatation of the vessels, stimulants will then act beneficially by restoring to the over-distended vessels the power of reacting on their contents." (2)

But, is not this equally true of bloodletting in the ordinary conditions of inflammation? In both cases, the power of contracting is suspended or counteracted, but is "restored to the over-distended vessels" by opposite methods. How, then, does bloodletting thus "restore the *power* of reacting on their contents?" (Vol. I. p. 127 — 128.) This we have just endeavoured to explain when speaking of stimulants, and more at large in the first sections of our Essay on the Philosophy of the Operation of Bloodletting. It happens, however, that the same treatment, as we have formerly explained, will remove either modification of the disease, though not with the same certainty. Though one system sometimes succeed, the other may have succeeded better. Again, take a simple form of inflammation, such as scalds; which however is shown by some of its vital phenomena to differ from the simple type. Here, the two opposite modes of treatment, local it is true but not the less direct, are constantly attended by success. So, in certain modifications of ophthalmia stimulants and sedatives appear to have an equal effect; and, what especially shows the close relation of the supposed opposite conditions, is the fact that in the same inflammation of the eye leeching and stimulants may be employed in conjunction, stimulants in the morning, and sedatives in the evening, and vice versa; whilst cathartics, and a moderated light, are beneficial in all the cases. Chilblains may be as well (if not better) cured by leeching and sedatives, as by local stimulants, and, of the latter, cantharides, (specific in its action,) is one of the best. Brunonians must have sometimes succeeded in removing all sorts of acute inflammations by active stimulants, notwithstanding the general havoc; and the more potent the poison, short of death, the greater was probably the success. The violence of the impression altered the morbid condition of the properties. We have familiar examples of this in the popular, though often fatal, treatment of colds by hot gin sling; a method, by the way, which originated with Van Helment, "whose patients," as asserted by Lobkowitz, "never languished long, being generally killed or

(1) See Andral's Patholog. Anat. vol. i. p. 35. (2) Patholog. Anat. vol. i. p. 35.

cured in two or three days."(1) (Vol. I. p. 660, *note.*) This practice is still pursued by some eminent physicians. (2) Fothergill,(3) Haygarth, (4) and Pringle,(5) treated rheumatism by bark. Fothergill says that the disease is prolonged by bloodletting.

"Such are the nature and principles of the febrile action," says Senac, "that at times nothing seems necessary to the cure of an intermitting fever, but *some sudden and striking change.*" "But, if remedies not well established do not *hit the mark*, and destroy the fever as it were by a *single blow*, they are *sure to render it more obstinate and dangerous.*" (6)

For a full illustration of the foregoing principle, and, indeed, in all its connections with our present subject, we refer to our first volume, p. 410—414. Mackintosh has left an evil in balancing the accounts between the depletive and stimulant plans of treating fevers; (7) and Travers, who, like Mackintosh, belongs to the antiphlogistic school, has unguardedly done the same mischief in relation to erysipelas. (8) (Vol. I. pp. 296—297, 300, 302—305, 308.) Nevertheless we are bound to present the statement by Dr. Graves, however adverse to general experience, that he treated an *epidemic* erysipelas

"With *wine*, quinine, and opium, and, with the exception of a single case, successfully. None in his senses would think of using general or local depletion." "The inflammation was characterized by the ordinary phenomena of true erysipelas." (9) Mr. Bullock reports a like success from the stimulant plan of St. Thomas Hospital. (10) (Vol. I. p. 302—303.) Was the disease of the *intermitting* character, or how shall we explain the success? Is not the credit due to the bark, and would it not have done better without the wine? (Vol. I. pp. 279, 283, 296, 302—305.)

How many still adhere to the stimulating plan of treating inflammatory and congestive typhus, erysipelas, Fothergill's sore throat, (11) &c.? Do not M. Louis and his school treat gastro-enteritis by tonics and stimulants with partial success, losing

(1) Zimmermann, on Experience in Physick, vol. i. p. 84, *note.*

(2) See Clutterbuck, on Pyrexia, &c. p. 85, 1837.

(3) Works, on Weather and Diseases, p. 105; and on the Bark, p. 240.

(4) A Clinical History of Diseases, &c.

(5) On Diseases of the Army; part 3, c. 2, p. 161, *note.*

(6) On Fevers, pp. 161, 162.

(7) Elements of Pathology and Practice of Physick, vol. i. p. 28.

(8) Further Inquiry into Constitutional Irritation, &c. p. 145.—Haller lost 128 ounces of blood during an attack of Erysipelas; *Zimmermann, ut cit. vol. i. p.* 304.

(9) In Lon. Med. Gaz. Jan. 7, 1837, p. 523, and Jan. 14.

(10) Ibid. March 4, 1837, p. 863.

(11) Aretæus (*a*) describes this disease; as does also Aetius, (*b*) who depended on bloodletting.

(*a*) De Caus. et Sig. Acut. Morb. l. 1, c. 9.—(*b*) Tetrab. Serm. 8, c. 45, p. 428.

only "one third;" and does not a branch of it in Great Britain concur in proclaiming the dangers of spilling the precious fluid in pneumonia? (Vol. I. p. 283, &c.) Shall we give you another example of American experience and philosophy (Vol. I. pp. 316, 327—328, &c.) in the treatment of inflammations attended by great prostration?

"This feeble, empty pulse of scarlatina maligna," says S. Jackson, "has been supposed to be the consequence of *extreme debility* of the *vital powers*, and to require the sustaining energy of stimulants and tonics. I have never witnessed from their employment, more, even when lavishly administered, than a transient effect on the circulation, and by augmenting the morbid irritation of the cutaneous and mucous surfaces, and thereby confirming their congested state, they have increased the vascular exhaustion, and have enfeebled to a greater degree the action of the heart and pulse."

Nevertheless, our author allows, upon the ground of experience and philosophy, that there may exist "the necessity of resorting, at the same instant, to a compound and opposing treatment;" and as to the doctrine of "exhaustion, &c. from loss of blood," he remarks that "Dr. Marshall Hall has written professedly on this state; but he may be suspected of pushing his favourite doctrine too far, and of seeing exhaustion from depletion where it truly does not exist." (1) (See our Essay on *Bloodletting*, Sec. 11.)

In all our remarks upon the pathology and treatment of disease, we have reference to those early stages of its existence when the powers and actions of life are fairly within the province of art; and it is here that we shall forever find, that common diseases in all countries, at all seasons, all ages, and in all constitutions, not already destroyed, demand the same general principles of treatment. It may be, as we have said, that moderate stimulation by alcoholic liquors must be employed in some cases of inflammation or venous congestion, at or near their invasion, in conjunction with the depletive system, to sustain or exalt the prostrated vital properties; or, as more frequently happens, a specific febrifuge of tonic properties, like the bark, may be equally important. (Vol. I. pp. 222, 224, 231, 303—306.) Dr. Mackintosh again departed from his usual philosophy, when he said, that,

"*Daily* experience convinces me that there are diseases which require a combination of bleeding and *stimulants*, and that it is not inconsistent with sound notions in pathology to bleed first, in order that *we may be able to stimulate*, and to stimulate for the purpose of *enabling us to draw blood*." (2) And so Drs. MacDowal, (3) Duncan, (4) and others.

(1) Principles of Medicine, &c. pp. 491, 495. (2) Op. cit. vol. ii. p. 241.
(3) In Dublin Journ. Med. and Chem. Sciences, vol. vi. p. 182.
(4) Edin. Med. Chir. Trans. vol. i. p. 649.

The latter proposition is not unfrequently true, the former rarely so; for it cannot be allowed that bark operates in intermittents as a stimulant, however its action, like stimulants, may exasperate other analogous conditions of disease. Zimmermann, than whom none is more fully aware of the modifications of a common disease, remarks that,—

"*Nothing is more uncommon than to see nature forsaking her ordinary routes. A pleurisy, which should require wine and theriaca, would be a greater rarity than a child with two heads.*" (1)

Mr. Hey, after a series of disasters in treating the congestive forms of puerperal fever by tonics and stimulants, takes advantage of his mistakes; and although

"There is a vast difference in the puerperal fever at different times, and in different situations and circumstances;" and though "in some cases it appears like a phlegmonous inflammation, whilst in others it destroys with more rapidity than the plague," he came at last to the unhesitating conclusion, founded on a large experience, that "the means of cure, (bloodletting, mainly,) *are precisely the same in both;* but, in the *latter* their measure is *greater and less limited*, and the period within which they must be employed, is far more circumscribed." (2)

And such exactly was the experimental practice of Dr. Denman, and such his ultimate conclusions; and such, too, were Dr. Gordon's. (See Vol. I. p. 312.)

It has been understood from the earliest ages of the art that means of treatment, which are generally more or less pernicious, will sometimes cure, partially at least. For this reason it was said by Celsus, that "it is of great importance whether one be *rightly* cured at first, or otherwise;" and he was equally sensible that "he who has been unsuccessfully treated through malpractice may be yet effectually cured by opposite remedies." (3) And so Hippocrates, (4) Ægenita. (5)

The plague is the same disease in all countries. (Vol. I. p. 321.) The vital forces are, essentially, alike affected in all places. This is equally true, individually, of yellow fever, typhus, erysipelas, smallpox, and all other diseases: (Vol. I. p. 598,) and, it has been one of our peculiar satisfactions in this essay, as

(1) Ut cit. vol. i. p. 174. (2) On Puerperal Fever, p. 183.

(3) L. 1. *Præf.* p. 19; l. 3, c. 2, p. 114; l. 7, *Præf.* p. 405. — When Celsus speaks of the different ways of curing according to the nature of the climate, (*a*) &c. he evidently alludes to the extent, and other partial modifications, of the remedies, and not to any difference in the general principles of treatment. And so Hippocrates, (*b*) and Galen. (*c*) (vol. i. pp. 304, 307, 308, 335.)

(4) Locis in Hom. (5) De Re Med. l. 3, c. 78.

(*a*) L. 1, Præf. p. 8. — (*b*) De Aer. Aq. et Loc. 1 and 57. — (*c*) L. 2, c. 7; l. 5, c. 14.

well as elsewhere, to show ourselves sustained in these opinions by some of the most accurate observers of nature. Nor can we repeat the sentiment, without ever sharing it with those philosophers who either first taught us the logic of nature, or who strengthen our convictions that we are on the only safe road. And here, as applicable to what we have just said of fever, we may cite the conclusions of another of those sound understandings:—

"If we observe the progress of infectious diseases, we shall find, *universally*, that the *same effects* follow the application of the *same causes*." "Thus, in affections whose causes are palpable, nature works in the regular order of causation, and the effects, *in every instance*, are such as experience and observation of the order of *natural phenomena* would lead us to expect." (1)

Nevertheless, we would not neglect the qualification which we have before stated as to variations in results that may depend on constitution, variations in remote causes, &c. Amongst the idiopathic fevers, however specifically distinct in certain modifications, there is also, a general alliance. (P. 502—513; Vol. I. pp. 519, 547.) The same may be affirmed of inflammation. Beddoes goes so far as to say, that—

"I now think myself entitled to draw a more precise conclusion respecting the connection of fever with inflammation. *In whatever organ this is detected, its symptoms appear at all stages of the disease alike; nor does the head offer the smallest peculiarity in this respect.*" (2) See APPENDIX on *Analogy*.

In a general sense this is certainly true of the early stages, allowing for those contingent influences which grow out of peculiarities in the remote causes, the varying states of febrile action, (3) or from coexisting venous congestion, (4) or from the natural modifications of the vital properties which distinguish every tissue, and even tissues of the same order. (Vol. I. pp. 90—91, 468, 570—576, 479, 495, 589—593.) Here we shall find a ready explanation why, according to Cleghorn, "the tertians of Minorca personate all distempers;" (5) why it was said

(1) Review, in Lon. Med. and Surg. Journ. vol. i. p. 527.

(2) Researches on Fever and Inflammation, p. 111.

(3) Important influences are often determined upon the local action by the constitutional affection; and it may therefore be necessary to treat the case with an especial reference to the system at large.

(4) Acute inflammations are often remarkably masked by the presence of venous congestions in the same or some other part. We have seen the joints greatly swollen from rheumatic inflammation; when, from being accompanied by an unusual degree of hepatic congestion, the pulse was quite feeble, 70, and the pain slight. As the congestion gives way in those cases, the pulse rises, and the pain increases.

(5) Epidem. Dis. of Minorca, c. 3, pp. 134, 137.

that "the yellow fever of Philadelphia appeared to be an epitome of all diseases;" (1) and why the scurvy is said to assume "all sorts of shapes." (Vol. I. p. 667, and *note*.) But, Cleghorn adds, that "so far as he has been able to learn from authors, there is a *great analogy* among intermittents *in all countries*."

But, when the structure of parts become deranged, or the properties of life are verging towards an extinction, pathological principles are on the decline, and therapeutics is more or less afloat, and without compass, on the broad ocean of experiment. Nor will we neglect saying in this place, that here it is that morbid anatomy begins, and has reared its pathological fabric on the ruins of organization. Again, the profound observer whom we just quoted remarks, that —

"It has been supposed, that in consequence of some unknown revolution in the human system, the genius of epidemic fevers has of late years become inflammatory, in place of nervous or putrid. But, an unbroken series of observations, if the nature of the thing do not speak plainly enough, proves that these disorders, however they may differ, like all other epidemics, in frequency at different periods, preserve one uniform character; or, if they vary, do not fluctuate in the supposed manner." (2) (Vol. I. pp. 289, 300, 332.)

The supposed distinctions, in a fundamental sense, have grown out of the various complications of epidemic fevers with inflammations and venous congestions; but, however modified by these local conditions, the true observers of nature have continued to recognise the general resemblance, and to find their conclusions supported by the results of treatment. Their principles have not been shaken by a dissolved state of the blood, or at finding nothing but turgescence of the veins where they had expected arterial vascularity, or disorganization. (Vol. I. p. 451 — 460.) They do not believe with M. Louis, Dr. Hall, and some others of the school of morbid anatomy, that the same general assemblage of symptoms may denote opposite states of disease. (3) (*Bloodletting*, *Sec.* 11.) M. Andral carries this doctrine so far as to say, that —

"It is a *law in physiology* that, in every organ, the diminution of the quanti-

(1) Rush's Cleghorn, *ibid. note.*

(2) Beddoes, ut cit. p. 173. — The treatment of typhus fever, in Europe, passed from one extreme to the other, and back again, in less than forty years. (Vol. i. p. 279 — 280.)

(3) It is worthy of remark, that, although the school peculiar to M. Louis, allows this extraordinary inconsistency in nature, it nevertheless maintains that medicine is capable of mathematical certainty. This, we may excuse in France; but our friends in Great Britain and America know better.

ty of blood which normally it should contain, produces functional disturbances, as well as the presence of an *excessive quantity* of blood. And, what is more, in *both cases* these functional disturbances are *precisely similar*. Let *impoverished* or *too thin* blood traverse, for instance, the cavities of the heart, *palpitations* will be the result, just as if *too much* blood distended it." (1)

Is this *pathology?* Shall it be regarded as the "law" by which our author reasons in his physiological investigations, and in their application to disease? Shall "laws in *physiology*" be established on such foundations? Is not this system of law-giving, as we are attempting to show, the result of a partial observation of nature? No induction can follow the comparison of one or two prominent symptoms, especially where, as in the foregoing case, the essential "functional disturbances" and all the absolute phenomena resulting from the important pathological conditions, and all the remedies, are totally *unlike*. The humoral doctrine is also at the bottom of the foregoing philosophy. (Vol. I. pp. 524 — 525, 616 — 632, 690 — 698.)

The general principles of treatment, then, cannot vary, respectively, in the foregoing affections; least of all, can opposite principles prevail at different times, and in different climates. It has been so from the days of Hippocrates. Otherwise, medicine would consist only of a series of observations, adapted to existing contingencies. (P. 207.) There could be no principles, and, of course, no science. Medical learning would be useless, and experience would only suit the present occasion. A new system of treatment must be devised for every climate, every constitution, and every reappearance of the same disease. And yet they, who would represent medicine as "still in its infancy," and "without chart or compass," — nay more, that the laws of disease have no stability, believe that a medical science may be constructed upon a mathematical basis. (P. 207; and Vol. I. p. 293 — 309.)

It is not nature who is inconsistent, or who operates by different laws. Art may give her this appearance; but still we say that "nature can never deceive." (2) It is owing to the consistency of nature that medicine had long since become a noble science; difficult, and concerned about all other sciences, and therefore taking the lead of all others. A science of principles deduced from the phenomena of nature, and which, with the

(1) Andral's Medical Clinic on the Brain, p. 130.
(2) Stokes' Theory and Practice of Medicine, p. 13.

facts that are known, conduct us with remarkable certainty to others that are unknown. It is here that well founded induction enables us to see farther than the senses, and to learn from a single vital phenomenon, from the expression of the eye, the existence and nature of those latent changes which too many can only see when seeing is useless, and bring upon art and philosophy the derision of the crowd. (Vol. I. p. 332 — 333.)

"He who proceeds upon a rational method, though things do not turn out as they should, must not immediately run from one medicine to another, so long as that continues which was there at first." (1) But, "it is not to be imagined that he should know the remedies of diseases who knows not their original causes," "though some think it unimportant what is the *cause*, and only concern themselves about the cure." (2) (See introductory extracts to essay on Morbid Anatomy.)

What are falsely called "exceptions to general principles," (Vol. I. p. 626,) are nothing more than variations in phenomena. These modifications may denote only variations of a common action, arising, especially from differences in the remote causes; or, they may depend, as we have already said, upon the same action affecting different tissues, or upon the morbid condition of one organ affecting another, or all others. And, although there be one great leading principle in the treatment of such cases, there are other subordinate ones founded upon the modifications. These are to be nicely balanced, that the *governing principle* may be properly directed. But it is only men of learning and close observation who can apply these distinctions. All others will look upon the variations of symptoms from their usual state in any one disease, or the differences in the results of an exact, methodical practice, as denoting different pathological conditions, or as constituting "exceptions to general principles;" and they will, therefore, instead of bleeding more, or bleeding less, in different cases of puerperal, or typhus, or yellow fevers, erysipelas, &c., and at different seasons, either take away, according to the "numerical system," exactly the "pound of blood," or change to bark and wine. It were wholly better, in the end, that the "numerical system" should obtain, and that the *pound of blood* be always taken. (Vol. I. p. 330.)

"Shall we say what we think? The physician who sees all the circumstances of a disease; he who sees only a part of them, and another who sees none of them, or rather, sees his own prejudices, must necessarily be of differ-

(1) Hippocrates, Aph. s. 2, a. 52. (2) Celsus, l. 1, *Præf.* pp. 7, 17.

ent opinions; and yet they will all swear by their *experience.*" (1) All are equally successful; at least, so they have it.

Whilst the industry of the multitude is thus employed, it is the part of the philosopher to select what is useful, and separate truth from error.

Returning to where we began this investigation, we find that M. Andral illustrates the doctrine of asthenic hyperæmia by two principal facts.

"In the first place," he says, "I presume few persons will refuse to admit as an instance of asthenic hyperæmia, those sanguinous congestions of the pulmonary parenchyma which so constantly occur *during the last moments of existence.*"

This is one of the fruits of morbid anatomy, as cultivated by the "anatomical school," and of the new philosophy in respect to inflammation; for the better introduction of which the foregoing term has been invented. We need scarcely say that the phenomenon of which our author speaks is neither owing to an active dilatation, nor to relaxation of the vessels, but is throughout of a mechanical nature, appertaining mainly to the veins, and taking place whilst the vital actions are becoming extinct. Again he says,

"At a certain period of long protracted diseases, when the patients lie constantly on their backs, even though the lungs may have presented no previous symptoms of disease, the pulmonary circulation becomes impeded, and the lung itself more or less congested, and after death is found *gorged with blood and serum.*" (2)

After what we have said of venous congestion, it will hardly be expected that we should go into a refutation of the foregoing doctrine. We shall only say that these diseases would be less "protracted," and result more auspiciously, were antiphlogistic remedies substituted for the treatment which our author's theory inculcates. Our author, however, supposes the latter phenomenon is owing to gravitation, and is purely of a mechanical nature; thus defeating the example, himself, as an illustration of disease. Upon these premises, however, our author comes to the conclusion, that, —

"When we purpose treating a congestion on the stimulating plan, the question to determine is, not whether the hyperæmia is acute or chronic, but whether it is sthenic or asthenic." He then proceeds immediately to state the foregoing examples as instances of its asthenic nature.

(1) This was remarkably exemplified in many quarters in respect to the cholera asphyxia.

(2) Op. cit. p. 37.

Such, then, is all we have from philosophy as to the fundamental distinctions of sthenic and asthenic hyperæmia, or active and passive inflammation. Has morbid anatomy supplied anything farther? Has it not, on the contrary, disclosed apparently the same state of the blood-vessels, the same lesions of structure, and the same morbid products, in each of the supposed conditions? As to the vital phenomena, and the means of cure, those we have already considered at large. "No study of morbid anatomy," says a leading Review, "can teach the transitions of disease from sthenic to asthenic." (1) (See next Essay.)

When speaking of the Theories of Inflammation, we deferred the consideration of the present inquiry for this place, that we might connect it with our pathology of venous congestion. In the former essay we cited the opinions of the most distinguished medical philosophers, since morbid anatomy became a methodical pursuit, as to the rationale of the inflammatory process. We may now add a summary view of their induction as to the fundamental distinctions between the supposed conditions of active and passive inflammations. We take it from Mr. Earle, who is the most consistent of his powerful school.

"It thus appears, that all the vessels of an inflamed part are dilated; that, while the motion of the blood is *arrested* in some, a greater quantity than usual is *forced* through others; and that *every shade of variety between active and passive inflammation* is owing to the *degree* of *distension* which is consequent upon the *primary stagnation*, which is *wholly attributable to the rapidity and vigor with which the circulation is being carried on at the moment when the stagnation is first established.*" "In proportion, therefore, to the feeble and languid state of the *general circulation* will the distension of the supplying vessels be less; as the *force* which should propel the blood through the collateral channels is insufficient for that purpose, the blood will remain in them, and thus give rise to an extension of the obstruction, and *consequently of the inflammation.*" (2) (Vol. I. pp. 225, 268, 292—293.)

Here we have active and passive inflammation running into each other, by every possible "shade." But, when we come to the extremes of the latter, as represented by our author, we meet with the very cases in which bloodletting is most indispensable; especially when venous congestion, as is often the case, contributes to the languor of the circulation. Were, also, the foregoing doctrine founded in nature, *active* inflammation should occur only where the general circulation is vigorous, and the *passive* form where it is languid; or, as is admitted, "the former must

(1) Brit. and Foreign Med. Rev. vol. iv. p. 307.

(2) Mr. Earle, in Lon. Med. Gaz. vol. xvi. p. 109.

be most commonly met with in the young, robust, and healthy; and the latter in the aged, feeble, and sickly." But, since the exceptions to this rule, as denoted by the analogous effects of bloodletting, are so numerous as to constitute a rule which places the whole upon a par, it is manifest that there must be a fundamental error in the premises, and that the pathological condition of all the cases is essentially the same. (P. 152 — 153.) Mr. Earle, however, thinks the hypothesis "is peculiarly recommended to notice by its extreme simplicity." That is to say, since all the theories in medicince are intended to guide our hand in practice, we have nothing more to do in the treatment of inflammations than to bleed the robust and stimulate the feeble; or, at most, to observe this rule in the ratio of existing strength. (P. 117—119.) We have just seen that M. Andral lays down a similar rule. We have endeavoured to show in our essay on Bloodletting, by facts and by principle, the fallacy of assuming either real debility, or a temporary prostration of strength, as a ground of practice in inflammation. We might state a multitude of individual cases, and even of general doctrines in relation to inflammations of particular tissues. Of the latter, we have seen that bloodletting has been deprecated in active phlebitis, in pneumonia, erysipelas, &c.; and we may add that the distinguished Dr. Parr, taking the ground of debility, remarks, —

"Perhaps bleeding is more seldom necessary in *enteritis* than in any other inflammation, for its *rapidity tends to mortification*, and should it not at once relieve, it soon proves fatal." (1)

And thus Dr. Good: "If bloodletting do not succeed, we shall *assuredly hasten the stage of gangrene*, and abbreviate the term of remedial operations. And hence, unless free bleeding can be employed early, and the constitution evinces a *tolerable portion of vigour*, there is no inflammation in which the lancet is less likely to be serviceable, or may become more mischievous." (2) And so Boisseau. (3)

Now we ask whether there be a well authenticated case on record where gangrene of the intestines has supervened on enteritis after early and copious bloodletting; and whether, also, an instance can be produced that may invalidate the practice recommended by Dr. Elliotson? (See Vol. I. p. 301.)

It is affirmed on high authority, and sustained by a few followers, that —

"In the treatment of pernicious pneumonia, we must keep in mind that blood-

(1) Parr's Med. Dic. Art. Enteritis. (2) Good's Study of Med. Art. Enteritis.
(3) On Fever, p. 346.

letting cannot cure a disease which will certainly return after a few hours with *fresh violence;* and of which experience has long since demonstrated bark to be the only effectual remedy?" (1)

Allowing the bark to be advantageous so far the affection may be of the intermittent kind, and taking the authority of authors for our guide, all the Italian of any note, and most of the modern, contradict the foregoing statement as to bloodletting. The "fresh violence" of which our author speaks is owing to causes which we have already explained. (Vol. I. pp. 132, 145, 146, 149, 155, 185, 189, 200, 209, 218, 220, 224, 228, 229, 291, 311, 316.)

From having neglected the considerations which we have now stated, Taylor has given us "An Exact Account of Two Hundred and Forty-Three different Diseases to which the Eye and its Covering are exposed;" and Rowley, "A Treatise on One Hundred and Eighteen principal Diseases of the Eyes and Eyelids."

Finally, which ever doctrine of inflammation may be considered true, that which ascribes it to stagnation of blood, and a suspension of all action, or the other, to increased action of the bloodvessels, opposite conditions cannot be supposed to give rise to the same phenomena without involving a repulsive contradiction. It is either always active, or always passive; always motion, or always stagnation. The few variations in symptoms and physical results, or the endless adaptations of remedies, depend on the varied influences of a thousand causes, which produce as many modifications of the vital properties, that are shown by the phenomena, in their various relations, to belong to a common genus whose fundamental requisite is identity of constitution. The varieties, as such, are easily comprehended when it is considered how the same vital forces are naturally modified in every part of the organization. It is certainly less remarkable that bark should be sometimes useful in inflammation where the action is essentially the same as in other cases where it may be injurious, than that urine should be innoxious to the mucous membrane of the bladder, whilst it would immediately excoriate the mucous membrane of the the lungs, or any other part.

So great is the opposition to the lancet, in certain quarters, in inflammatory, and especially in congestive diseases, and more particularly when attended by prostration of muscular strength, or of the forces of life, that we *do not mean to be misunderstood* upon this subject. In the first place, then, we repeat that our

(1) Diseases of the Chest, p. 248.

suggestions in relation to bloodletting, whether they regard its importance, or the philosophy of its operation, have an especial reference to the early stages of disease. What may turn up during its various ravages, what may grow out of disorganizations and the mechanical effects of effusions, no one can surmise. The practice must then vary according to the new order of organization; and even so where there may apparently happen only an absolute exhaustion of the forces of life; or, stimulants may be temporarily necessary at the invasion of disease, where bloodletting may be not less indispensable? And this leads us to speak especially of typhus fever; the true typhus we mean. Bloodletting is the great remedy in its early stages, but may be destructive in its more advanced. Tonics and stimulants must now often take its place, however local congestions and inflammations may exist; and, as we have said before, we have sometimes administered them successfully, in neglected cases, to a great extent. In these conditions we have sometimes exhibited a quart of brandy, within thirty-six hours, along with a fair proportion of wine, and Huxham's tincture of bark, and strong broth; the patients, perhaps, being comatose during the time. But, had we seen these patients at the invasion of the disease, we had saved the necessity of moribund treatment.

Here, however, let us observe, that it has been in the true typhus alone that we have witnessed any remarkable advantages from the *stimulant* plan in the advanced stages of fever. In all other cases, we have generally found the issue fatal, where wine, &c. has appeared to be *necessary* to raise the forces of life; and this is true not only of our own experience, but of that of many of our medical friends. From this fact, therefore, we derive another important proof of the individuality of typhus fever, and that it depends upon remote causes different from those which establish other febrile affections. Those causes modify also, the inflammatory affection of the venous and of other tissues, so that wine and other stimulants become curative in its advanced stages, just as the Peruvian bark is sooner or later adapted to the febrile condition, and the modified inflammations and congestions, of intermittents.

Having began this argument with M. Andral, we shall now, as on former occasions, (Vol. I. pp. 524, 525, 627 — 632,) produce his own testimony against his doctrine of sthenic and asthenic inflammation. We have already, indeed, cited the fol-

lowing excellent philosophy, in part; but we wish to have it in connection with our present subject. In speaking of the different conditions of hyperæmia, he says,—

"We *know not* what the *peculiar modification* is which the texture of an organ undergoes, so that in one case it allows the *blood* determined towards it to escape from its vessels; in another it forms *pus*, or exhales only a *thin serum;* whilst in a third it becomes *indurated*, *softened*, or *ulcerated*. But, there is *one common link which unites* these different alterations, and hence it is, that under apparently the *same causes*, we *often* see them *produced indifferently, and not unfrequently replaced one by the other*." "But, *in all this series of phenomena* we can perceive, *throughout the whole course of the irritation, one constant lesion* namely, the hyperæmia, and a *succession of morbid alterations in the organic action* of the tissue affected, producing *alternately, hemorrhage, cessation of all secretion*, exhalation of *serum*, and of *pus*, and, lastly, a *recurrence of hemorrhage*. I might also adduce here the well known fact, that, in many cases of inflammation of serous membranes, which present *no perceptible difference in their nature*, duration, or *intensity*, *pure blood* is sometimes found effused, sometimes a simple *albuminous fluid*, and sometimes perhaps false membranes, &c." (1)

It appears to us, therefore, that if the principles involved in the foregoing facts are not of universal application where the same or nearly the same facts occur, and if our construction of the modifying influences of remote causes in respect to some of the details of treatment, which may appear to contradict the universality of principles, be not in harmony with the foregoing physiological variations of "one constant lesion," medicine must surrender as a mere empyrical art.

Whenever M. Andral reasons upon the abstract phenomena of nature, he appears as a sound philosopher; and his opinions are so far of the highest value. In this respect he differs widely from his distinguished countryman, M. Louis, whose special merit consists in supplying examples of the worst havoc of disease. We have thus adverted to our author for the purpose of availing ourselves of his critical observations of disease which have a special connection with many of our investigations. We shall close this section with a few extracts of important and various import, whilst the tendency of the whole is to contradict the hypothesis of "sthenic and asthenic hyperæmia." We confine ourselves to what relates strictly to venous congestion, and we doubt not that the entire philosophy, in a physiological sense, will be allowed to be fully applicable to common inflammations.

The following case is a fine illustration of what we have said of the sympathetic influences which prevail amongst the veins

(1) Patholog. Anat. vol. i. p. 54.

of different parts, when they become the seat of congestion in any one organ. (P. 248, 279, 283, 290, 333, 351, 364, 369, 442, 454; and Vol. I. p. 134 — 138.) It has, also, an important bearing upon what we are about to say of spontaneous hemorrhage in our next section. We place it here as a subject for general reference.

"There are other cases where cerebral congestion recognises for its cause neither *intense febrile disturbance nor an inflammation;* but where it is connected with other congestions which occur in different organs, and which *alternate* one with another. *Sometimes* this tendency of several parts to hyperæmia is connected with a *general* state of *plethora;* sometimes the latter *does not* exist, and there is observed a *succession* of *local plethoras*, which cannot be explained by the *apparent constitution* of the individual. We have seen a woman who presented a succession of hyperæmias. She began by complaining, for fifteen days, of a violent headache, accompanied by continual dazzling, vertigo, disagreeable noise in her ears. She staggered in walking, as if she were drunk. These signs of cerebral congestion *disappeared* at the *same time* that there came on an *abundant epistaxis*, which was renewed for several successive days. (Vol. I. p. 275.) Scarcely did this epistaxis cease, when she began to feel pains towards the loins, the lower extremities became numbed, and there soon appeared a menorrhagia which lasted for thirty hours. The patient was 37 years of age, but had not her menses for the last three months. It was near the usual period of their return that the menorrhagia took place. After this phenomenon, there was no more appearance of hyperæmia towards any organ for a month. At the end of that time, the menses did not appear; but, without cough, or producing dyspnœa, some blood was *expectorated* in a quantity large enough to fill the fourth of an ordinary glass. *After* this hæmoptysis had ceased, *signs of cerebral congestion* reappeared, and they continued with variable degrees of intensity for twenty days. At the end of this time, a *new scene* presented itself. The cerebral symptoms *disappeared;* but the patient voided pure blood from the *bowels*, and this new flux went on for twelve days. Twice or thrice every 24 hours she felt a *slight colicky pain*, which caused her to go to stool, and every time she went she passed from a fourth to half a glass of blood. On some days even the loss of blood was still more considerable. This internal hemorrhage lasted till the time at which her menses should appear, but they did not. They came the following period, but in the form of a hemorrhage, *so copious* as to render bleeding *necessary*. From *this period* her health was re-established; menstruation became regular," &c. (Vol. I. p. 249, *Hey.*)

Now is it not apparent, in connection with what we shall say in our next section, that the whole pathological condition of the foregoing case was one of venous congestion, exemplifying sympathetic developments of the disease in various parts of the venous system; that the essential proximate cause was venous inflammation upon which the hemorrhage depended, and which had been imperfectly instituted by nature to overcome the disease, but which was finally conquered by a more decisive effort, how-

ever a corresponding attempt was simultaneously made by art when nature no longer wanted its assistance? (Vol. I. p. 275—278.) And is not this case a fair example of hemorrhage from what is called "asthenic hyperæmia"? Here is another exactly parallel case, with nothing but strongly marked venous congestion. We introduce it to illustrate the pathology of the foregoing, and to show the sympathies which prevail in the venous system, and how rapidly one congestion may follow another through this principle; since, we suppose, it will not be contended that, in the following case the congestion, either of the brain or the lungs, was of a mechanical nature.

"We shall now cite a case," says M. Andral, "in which the congestion, *primarily* seated in the brain, was then removed to the *lungs*, and became immediately fatal in consequence of the hemorrhage *it produced*. A man 50 years of age, entered the hospital with *all the signs* characterising *great* cerebral congestion. This was not removed by bleeding. (Vol. I. pp. 199, 201—202, 238, 345.) *All at once* the patient was seized with extreme dyspnœa, which continued to increase, and he died at the end of four hours. On opening the body we found the cerebral mass gorged with blood. *No other* lesion in the brain. But, in the two lungs there were found hard and black masses, which presented all the characters of the lesion known by the name of pulmonary apoplexy." (Vol. I p. 249—254.)

Our author has then a few words upon the philosophy involved in the foregoing cases; and his remarks tend to the conclusion that the congestions or inflammations which spring up in plethoric or debilitated, or exsanguious, subjects, are *exactly of the same nature*. Thus:—

"What is the influence exercised, either by a state of general plethora, or by a contrary state, on the production of cerebral hyperæmia? It is undeniable that, in many individuals, a state of general plethora coincides with the appearance of the symptoms indicating the existence of cerebral congestion; but it is far from being so in all the cases. There are even individuals in whom this congestion appears at the very time that they arrived at a *remarkable state of anemia*. (Vol. I. p. 292—293.) We have just seen, at the *Pitié*, a woman labouring under cancer of the uterus, and very much exhausted in consequence of the great hemorrhages she constantly had from the uterus; yet, she has just died from cerebral hemorrhage."

Our author regards these, and analogous congestions in a mechanical sense, notwithstanding the light which he has thrown upon the pathology by the case which we have stated at page 348. The following accurate observations by our author contribute farther light to our subject.

"The duration of cerebral congestions varies according to each of their *forms*; and even in *these forms*, there are still, in this respect, *very great* differences. (Pp. 478—479, 485, 506—508.) Sometimes the congestion returns incessantly,

and that during several months, and several years. We have seen individuals who, for almost all their life, were tormented with it. It not seldom is suspended for a shorter or longer period, and then returns under the influence of causes which are appreciable or not. Several authors have spoken of intermitting apoplexies, which, in their return, observed the same regularity as the fevers of this name, and presented themselves with the same types." (Pp. 478, 502—504.)

"Cerebral congestion terminates most frequently in a favourable manner; but, its return may be dreaded, or, at some future period, a more dangerous disease. (P. 532 *Celsus*, and Vol. I. p. 304, *note*.) It sometimes happens, that certain phenomena, *particularly fluxes*, *coincide with its disappearance*, and appear to contribute to it. (Vol. I. p. 275—277.) We have seen a woman, who, for about a month, was tormented with vertigo and other symptoms, which caused us to apprehend an attack of apoplexy. Repeated bleedings, a blister to the neck, exercised no remarkable influence on those symptoms. (Vol. I. pp. 165—166, 201—202, 235.) At last, she was seized on a sudden with copious diarrhœa. She had, in 48 hours, fifteen stools, constituted principally of bile. At the end of this evacuation, which was scarcely accompanied with any colic, the cerebral symptoms disappeared."

Our author then adverts to the celebrated case by Lancisi, (Vol. I. p. 275,) where a threatened attack of apoplexy was cured by the immediate loss of more than eleven pounds of blood from the nose.(1) He then adds that,

"It would seem that cerebral congestions should be one of the diseases against which copious bloodletting must certainly succeed. And so it happens in a considerable number of cases." (2)

It is unfortunate that he does not state in the foregoing case the quantity of blood that was abstracted. But, from what we may gather from his numerous cases, we may well conclude that the quantity was not very "copious." Nature accomplished what art had failed of doing; and, we apprehend the following to have been the rationale of the cure. Hepatic congestion, or, as we would call it sub-inflammation of the venous tissue of the liver was the primary condition of disease. The same pathological state of the brain supervened sympathetically, as seen in the first of the foregoing cases from M. Andral. The repeated bleedings so far overcame the hepatic affection as to be followed by that secretion of bile which always takes place redundantly when hepatic congestion begins to yield. This is nature's mode

(1) The translation which we quote has it "eleven *ounces* of blood." Andral is right,—"*onze livres*." (*a*) We may say here, that we generally avail ourselves of the best translations, when we give an English version, in preference to our own.

(2) Andral's Medical Clinic on Diseases of the Brain, p. 121—127, *tr.*

(*a*) Clinique Médicale, t. v. p. 292, 1834.

of extinguishing the disease; and having fully effected this result in the present case, the brain was not only relieved by the natural depletion, but by withdrawing either the original source of its congestion or a cause which contributed to maintain it, and by those other salutary influences which are exerted over a diseased organ when another passes from a state of disease to that of health.

The foregoing cases, which were attended with hemorrhage, are important to our next section; where we shall also again advert to the philosophy of M. Andral as to natural effusions of blood.

SECTION XVI.

PHILOSOPHY OF SPONTANEOUS HEMORRHAGE, AND ITS APPLICATION TO THE PATHOLOGY OF VENOUS CONGESTION.

The inquiry upon which we now enter was begun in our first volume, page 371—384, which should be considered in connection with the remarks which follow. We endeavoured to show there, that spontaneous hemorrhage is rarely the result of ruptured vessels, unless there be erosion of their coats; that it generally depends on venous congestion, though the terminating series of the capillary arteries are the immediate instruments by which the effusion is effected, and not the capillary veins, which we do not believe are ever concerned in processes analogous to secretion. Physiology is against such a conclusion; and for like reasons the capillary veins, however they may be the seat of inflammation, are never its instruments. (1) The rationale is the same in hemorrhage from congestion, as it respects the direct instruments, as in hemorrhage from common inflammation. Indeed, in the former case, the proximate cause is incipient inflammation of the cellular tissue; and here again we have another proof that similar results, especially of a physical nature, depend upon similar organic processes.

In confirmation of this view of the subject, we may advert to the statements of the best observers as to the appearances of va-

(1) Nor does the blood ever take a retrograde course either in the arteries or veins.

rious organs in subjects who have died of hemorrhages. Such, we have already seen to be the case in sanguineous apoplexy, where venous turgescence is the great characteristic, whilst the capillary arteries in the vicinity of the hemorrhagic seat are more or less injected. Bichat, who saw almost every thing, was impressed with the belief that the veins were directly connected with the phenomenon, and inquires "*is not apoplexy a sudden rupture of the venous capillaries?*" (1)

"In sanguineous apoplexy," says Dr. Philip, "a preternatural distension of the larger vessels is conspicuous, while the brain itself is often nearly or altogether of the *natural colour.*" (2)

Dr. Cheyne states that "turgescence of the veins always appears in dissection, in sanguineous apoplexy." "This is probably the most *important*, as it is the most unvarying appearance." (3)

So, Portal, (4) Craigie, (5) Rochoux, (6) Andral, (7) and many others, especially as deduced by Ferrario from more than 10,000 cases. (8) The veins sometimes appear

"As if forcibly injected with a strong solution of indigo, and the sinuses gorged as if ready to burst, while the *arterial system* is almost undistinguishable." (9)

According to Dr. Bright, the vascularity, in sanguineous apoplexy, "gives a general dusky or purple hue, and is marked by a great augmentation of the veins running into the sinuses." (10)

Bichat says, "it is in subjects that have died of asphyxia, apoplexy, &c. that the venous ramifications can be best observed." (11) So Flourens. (P. 490.)

So, also, in hæmoptysis, whether the effusion be from the sur-

(1) General Anatomy, &c. vol. i. p. 412.

(2) Exp. Inquiry into the Laws, &c. p. 279.

(3) Cases of Apoplexy, pp. 22, 24, 41.

(4) Sur l'Apoplexie, pp. 13, 63, 330, 335.

(5) General Anatomy, &c. p. 403. (6) Sur l'Apoplexie, p. 144.

(7) Clinique Méd. t. v. See his numerous cases.

(8) Statistica delle Morti Improvvise e partic. per Apoplessia, dell 'Anno 1750 al 1834.

(9) Case by Dr. Johnson in Med. Chir. Rev. Dec. 1822.

(10) Medical Reports, vol. ii. p. 690.

(11) Op cit. vol. i. p. 380. — We have attentively examined almost every distinguished writer on the subject of apoplexy, and are prepared to say, that, so far as they have given any attention to the state of the cerebral vessels, there is nearly an universal coincidence in their facts which go to establish the prëexistence of cerebral disease, and the inference that the sanguineous effusions are its consequence, and depend on capillary action. But, what is especially important to our purpose, whenever the state of the vessels is mentioned, and by many authors it is generally so, the veins are said, in sanguineous apoplexy, to have been preternaturally full, whilst there is generally an absence of vascularity in the capillary arteries, unless it exist in the vicinity of the part which is the seat of the effusion.

face of the bronchial tubes, or from that of the air vesicles. In this affection, (1) and in hemorrhage from the bowels, either venous congestion, or inflammation, of the mucous membrane is the usual appearance; but the former condition obtains in a vast proportion of cases. True, where the hemorrhage is large, the vascularity may be subdued; but here the authority of Andral will not be questioned. "*This pale state of the bleeding tissues*," he says, "*cannot be admitted as proof that no congestion had existed either before or during the hemorrhage.*" (2) In purpura hemorrhagica, scurvy, cholera asphyxia, and petechial fevers, it is manifest in various parts. Cazenave is disposed to think "with some English pathologists, that purpura must *necessarily* be preceded by a congestion of the venous system." (3)

"It has been maintained by some writers," says Plumbe, "and amongst others, I believe, by Dr. Mills, Dr. Parry, and Dr. Combe, that venous congestion is *necessary* to the production of petechiæ." Dr. Plumbe agrees that this is probably true "in the petechiæ of low fevers." (4)

But, we have already gone over the ground of proof which establishes the dependence of this affection upon venous inflammation. It is important in its analogical connections with the whole of our present subject, as it is with the general pathology of venous congestion. If anything could add to this view of the subject, it would be those artificial demonstrations, where venous inflammation is excited by irritating injections into the veins, and which are followed by extravasations of blood. (P. 476.)

Dr. Hope has given a plate representing the connection of hemorrhage from the liver with venous congestion. We are told by Mr. Twining, that in venous congestions of the spleen, there is a general disposition to hemorrhage. When it proceeds from the stomach and bowels, he refers it directly to the splenic vein, an opinion which has long prevailed. We apprehend that the same sympathetic results obtain in these cases as in congestive puerperal fever, purpura hemorrhagica, the secondary abscesses which supervene on acute phlebitis, &c. Delaroche, in

(1) Ramadge is clearly right in the view he has taken of the pathology of his Majesty's disease, and that instead of the supposed "rupture" of any vessel in the mucous coat of the stomach, "the sanguineous effusion was owing to a state of *venous congestion*," and "was poured forth from the *vasa brevia*." — *On Asthma*, pp. 308, 310.

(2) Patholog. Anat. vol. i. p. 53. (3) Diseases of the Skin, p. 374.

(4) Diseases of the Skin, sec. 5, c. 1, p. 274.

speaking of the hemorrhoidal flux, states the general result of morbid anatomy in all hemorrhages: —

"La théorie méchanique et *hydraulique*, qui a existée pendant des siècles et qui ne dérive certainement que de quelques faits particuliers, doit faire place à *une autre qui s'accorde mieux* avec la *théorie des hemorrhagies en générales*, et qui *résulte de l'observation* des phénomènes de la maladie, *ainsi que des overtures des cadavres*." (1)

Before proceeding farther with this investigation, we may say that the extravasated blood is generally dark when it depends upon venous congestion. This arises from the remarkable fact that the universal blood is commonly rendered dark before congestion has reached that point when hemorrhage ensues. The colour of the blood may therefore be taken, in a general sense, as a proof of the presence of venous congestion. (See APPENDIX I.) This is the reason why it has been often said, that, "*in natural or spontaneous hemorrhages the blood which is discharged appears to be chiefly venous.*" (2) The cause of this dark colour we have endeavoured to explain in part, in our first volume, p. 427 — 446. See, also, Vol. II. pp. 38, 41, 43, 48.

"That excellent anatomist, Dr. Macartney," says Cooke, "states that in typhus fever the morbid appearances are not those of common visceral inflammation. Venous congestion of particular organs is the principal lesion, according as the head, lungs, or abdominal viscera, are engaged in the disease."

The general statement which then follows is a full summary of the experience of the best observers; and the different effusions which occur, not only illustrate the pathology of venous congestion, but go to establish the dependence of the simultaneous hemorrhages upon a secretory process. There is, according as one organ or another is "engaged in the disease,"

"1st, Fulness or distension of the vessels of the brain, especially of the veins, and some water effused on the surface, and into the cavities of this organ.

"2d. The same species of congestion of the lungs, and different degrees of effusion into the cavities of the pleura and pericardium.

"3d. Venous congestion in the liver, spleen, or alimentary canal; sometimes a *bloodshot* appearance or *spots* of extravasation *in* the mucous coat, more particularly in the stomach and first coils of the intestines. These congestions were *always of a purple or venous* colour." He adds that common inflammation may supervene on these conditions. (3)

"Venous congestion," says Dr. Armstrong, "is generally found attended by effusions of mucus, serum, or blood." And again, "local *obstructions* are often

(1) Traité des Hémorrhoides, 1812. (2) Wardrop, On Bloodletting, p. 17.
(3) Cooke's Morgagni, vol. i. p. 522, *note*.

connected with and relieved by spontaneous hemorrhages, which, in such cases, *seldom diminish the strength.*" (1)

Does not analogy, in all respects, suggest the presence of inflammation in the congested veins? (Vol. I. pp. 143, 156, 165, 187, 189, 193, 198 — 202, 275 — 277.)

Malcolmson affirms the same as to the connection of extravasated blood with "distension of the veins," in beriberi. (2) And so, in all the congestive fevers from plague to intermittent, as has been already shown, (Sec. 7.) It is remarkable that so great a philosopher as Jackson, after saying that the hemorrhagic effusions take place from the "distended veins," should impute them to "venous paralysis," and "a loss of cohesion or vitality in the blood." But mark: "The venous paralysis," he says, "was apparently the effect of *over-excitement.*" (3) (Sec. 7, &c.; and Vol. I. p. 453 — 456.) In cancerous affections, Cruveilhier supposes that the hemorrhage takes place directly from the capillary veins, which he regards as the instruments of the disease. (4) They are only concerned, however, in being one of the tissues that is the seat of inflammation; and what shows the probability of the fact, and the singular development of higher degrees of inflammation by the lower grades, in parts either continuous, or remote from each other, is the fact that phlebitis of the arm has frequently followed carcinoma of the breast. (5) Suppression of the hemorrhoidal flux, it is well known, is apt to be followed by effusions of blood from other parts. There can be no other rational interpretation than what is supplied by the analogies in relation to sympathy; and here, analogy points, also, to the veins as the probable source, whilst anatomy confirms what reason had suggested.

The florid colour of expectorated blood, or wherever external hemorrhages are small, is evidently owing to an intermixture of the blood, or to its free contact, with atmospheric air. If retained in the lungs, according to Laennec it is always very dark. (6) In dysentery, there is generally both inflammation and venous congestion of the intestine, (Sec. 14.) Here, too,

(1) Boot's Memoir of the Life and Med. Writings of Armstrong, vol. i. p. 132, &c.; and Armstrong on Congestive Typhus.

(2) Ut cit. p. 222. (3) On Febrile Diseases, vol. i. pp. 73, 76, 82.

(4) Patholog. Anat. livrais. 4, p. 4.

(5) Ramsbotham's Lectures on Women and Children. Johnson, in Med. Chir. Rev. vol. xxix. p. 264. Sir C. Bell, Graves, Stokes, &c.

(6) Diseases of the Chest, p. 188.

the blood is often quite florid, partly for the reason just stated, in part on account of the decided inflammatory state of the mucous tissue and the general arterial excitement, and in part from the action of other secreted fluids upon it. But it is just otherwise in almost all other intestinal hemorrhages, even though common inflammation be present in various parts. Where the affection is most purely congestive, and capable of constitutional influences, as in purpura hemorrhagica, the colour of the blood is most characteristic, whether it be secreted, or taken from a vein or artery. In pulmonary apoplexy,

"The colour of the diseased part is of a deep black red, similar to that of a clot of venous blood. The neighbouring veins are often filled with concrete and half-dried blood." (1) Laennec says it is "of a very dark red, exactly like that of a clot of venous blood, or altogether black. The veins are sometimes filled with a firmly coagulated and half dry blood. *From its exact resemblance to the effusion that takes place in apoplexy*, I have thought the *name* of pulmonary apoplexy *very applicable* to it." (2)

"Sous le rapport de la lésion anatomique qui la constitue," says Andral, "l'apoplexie pulmonaire n'est pas, selon nous, une maladie particulière et différente d'une simple hémorrhagie de la membrane muqueuse." (3)

When blood is effused into the substance of the liver, Prof. Alison states that it has the appearance of a "coagulum of venous blood." (4)

Capillary hemorrhage is more generally supposed to arise from common inflammation, or some analogous process, having no dependence on venous congestion. However this may be sometimes true, it is comparatively rare. This we have already shown anatomically; and the same conclusion will follow from an investigation of the symptoms. In most instances there is not that degree of pain which commonly distinguishes inflammation of other tissues; and the absence of other severe symptoms in cerebral hemorrhage has induced most writers to believe that the extravasation depends immediately upon a rupture of some vessel. (Vol. I. p. 371, &c.) The same comparative mildness of symptoms generally obtains in hæmoptysis, hæmatamesis, &c. and has led extensively to the doctrine of rupture. The pulse is generally alike affected in hemorrhage and in venous congestions without hemorrhage. It has rarely the same hardness as is usual in common inflammation, or the same frequency, being often pre-

(1) Dr. Davies, Lectures on Diseases of the Heart and Lungs, p. 218.
(2) Diseases of the Chest, p. 188. (3) Clinique Méd. t. iii. p. 109.
(4) Outlines of Pathology, &c. p. 392.

ternaturally slow. If depressed, as is frequently the case, it often rises in volume and force as the effusion goes on, just as it does in venous congestions under the influence of bloodletting. (Vol. I. p. 275 — 277.) The precursory symptoms in hæmoptysis are freqently not sufficient to engage the attention of the patient. The same is often true of venous congestions till a sudden and violent explosion ensues. (P. 219.) But, venous congestion passes so readily into inflammation of the adjacent tissue, or becomes the exciting cause of it in some other part, that we often find the latter condition of disease supervening on hæmoptysis, and affecting either the mucous or other textures in the lungs. Hemorrhage now ceases, although the local and general circulation become exalted. It is replaced, perhaps, by a mucous or mucopurulent discharge, or other more serious results of inflammation.

And so of hæmoptysis, if the hemorrhage be not sufficient to overcome the disease on which it truly depends, or art should prematurely restrain the effusion. (Vol. I. p. 153.) We believe, however, that in hæmatamesis the congestion has oftener passed into inflammation of the mucous tissue; or the latter, in a sub-active state, has existed from the beginning more frequently than in hemorrhages from any other part. But, in most of these cases there is hepatic congestion; but, whether most commonly primary or consecutive, we have not been able to satisfy ourselves on account of the long duration of the complaint in the instances with which we have met. It is obvious, however, that the incipient condition of disease is sometimes that functional derangement, merely, which constitutes the most simple form of indigestion, and which exerts its sympathetic influences, according to its degrees, or as they may pass on to gastric congestion or inflammation, and to the existing predisposition of the liver; just as we have seen the same influences constantly developing cerebral congestion. Nevertheless, we have found hæmatamesis to have occurred most frequently in individuals who had been sometime antecedently, perhaps many months, the subjects of intermittent or remittent fever. We speak, of course, of hæmatamesis in its purely local aspect.

In sanguineous apoplexy, the prëexisting disease is generally of short duration. Here anatomy, as we have seen, is all sufficient; but we may say that the symptoms are those of venous congestion of the organ, and not of common inflammation. We have fully investigated the modifications of venous congestion to

which this organ is liable, according to the nature of the remote causes, age, &c., and it is here that the modifications of this disease may be especially appreciated. In one case it is apt to result in hemorrhage; in another and when purely local as in childhood, serous effusions are the usual product; in a third, as when combined with idiopathic fever, it soon passes more or less into inflammation of the adjacent tissues, with various other results.

Other attendants, parallel with those of venous congestion, might be stated. The blood, even from arteries is dark, and it is apt to flow slowly in venesection. If properly taken for examination, (p. 291,) it is generally covered with a buff, and cupped; but these characters are rarely so well marked as in an equal extent of inflammation of other tissues.

Our proposition, therefore, is, that venous congestion and an incipient state of common inflammation, (or congestion giving rise to inflammation in tissues which involve the capillaries of the congested veins,) generally constitutes the pathological cause of spontaneous hemorrhage, which is an effort of nature to relieve the morbid condition of the venous tissue.

From the manner, in which we have spoken of *hypothesis* in medicine, it might be well for us not to attempt the rationale of the process by which the effusion of blood is determined. It is a question, however, which like all others, rests upon facts; and, since their multiplication would reflect no farther light, we shall endeavour to reason upon what is known. (Vol. I. p. 298.) We think, too, that the same affirmation may be made of inflammations and venous congestions; though we doubt not that there will be no end to the demand for "rigorous facts" and "mathematical demonstration;" whilst others will resist with difficulty the influence of habit, or the authority of opinion. But, however "*we may build facts upon facts till our pile shall reach the heavens, it will still tumble to pieces till it be cemented by principles.*" How far the recent pursuit of morbid anatomy has improved the doctrine of Hippocrates, of Sydenham, and Hunter, and Bichat; how far it has enlightened us, in a general sense, as to the pathology of disease, and how far it has made us better practitioners than the early sages of the art, we have already endeavoured to show; and as "demonstration" is the only terms on which we can hope for a hearing, we shall resume the inquiry in our remaining essays.

Nor is our immediate subject one of a mere theoretical nature. Every part of its philosophy relates directly to the pathology of important conditions of disease, and has an immediate bearing, therefore, upon the highest interests of the healing art. That such investigations, as well as many others with which we have been concerned, will not meet with favour from the mere anatomist, or from him who treats disease according to the "numerical method," we are prepared to expect; but, we believe "our method" must in the end prevail, and we shall therefore cast ourselves upon the stream of time.

We know not that we have ever borrowed an opinion without referring it to its proper source; and lest in what we are about to say it may be surmised that we have derived our hints from the mechanical doctrine of Cullen, we may state that we had either forgotten or were never aware of its existence till this part of our subject was completed in its present shape. A comparison, however, will show that there is but little similitude. Still, there is enough in Cullen to prove that even in his day spontaneous hemorrhage was familiarly considered a result of venous congestion, and that it was held important to attempt its philosophy although at the expense of the fundamental laws of life.

We have seen in our 14th Section, when venous congestion passes into inflammation of the tissues which involve the capillaries of the congested veins, how the capillary arteries probably become excited. Although in spontaneous hemorrhages, morbid anatomy discloses venous congestion, it is often abundantly obvious that the capillary arteries are more or less injected, especially when the hemorrhage has been small. The change of action, however, in the arterial system has not been sufficient, by its sympathetic influence to overcome the congestion. More blood, however, is determined upon the capillary veins, which, being already morbidly enlarged, and their natural tendency to a sympathetic dilatation, when the corresponding arteries are excited, being impaired by disease, (p. 393—397,) the congested capillary veins refuse to receive freely the augmented quantity of blood. The dilatation of the venous parietes may also be supposed to exist at that utmost limit of which they are susceptible through any natural disposition. All beyond, therefore, must be mechanical distension. The blood, partly in consequence, accumulates in the capillary arteries. Here is something like obstruction, growing, however, out of vital action, as when

the blood is determined from the circumference to the centre in the depressing stage of venous congestion, or in the cold stage of idiopathic fever. Were there now no other means of relief, (p. 521,) however little may have been the tendency to active inflammation, it would probably soon become established by the stimulus of distension. If that condition fully supervene, disease in the venous parietes would more or less yield, and the circulation would be liberated. Being otherwise, the secreting series of vessels now feels the influence of this condition, and they either pour out an increased quantity of serum as is most generally the case,(1) or their action is differently modified and they secrete

(1) Dr. Craigie, in stating the anatomical lesions in typhus, says that "the limpid fluid was lodged between the pia mater and arachnoid; and that this fluid was *always most abundant in the vicinity of large venous trunks.*" (*a*)

Müller says, "the existence of exhalant vessels, which even Bichat admitted, and supposed to be open side branches of the capillary vessels, is purely hypothetical." This conclusion is one of the physiological dogmas of the microscope. It is evidently brought forward to support the conclusion that the capillaries have no open terminations. Müller offers no other proof; but he *assumes* that all effusions take place by a mechanical "permeation" through "pores," which he admits have never been seen, not even by the microscope. Müller, with all his excellent philosophy, allows nothing which he has not *seen*, (excepting the "pores,") and compels necessity to yield to the indications of the microscope. Haller, as well as Bichat, and most other observers of nature, comes under the ordeal of that all-presiding, omniscient instrument, since "Haller," according to Müller, "unfortunately adopted, and thus contributed to confirm, the *crude notions* of his predecessors regarding the open terminations of arteries."

Müller considers "*doubtful* the existence of vessels in the substance of the cornea." This, also, is the decision of the microscope. But, "it is well known," he says, "that in inflammation the cornea contains vessels carrying red blood." After reasoning for a while in this manner, he comes at last to the conclusion that "all these facts render it *very probable* that even the cornea, and capsule of the lens, to which vasa *serosa* have hitherto been ascribed, are *really provided with vessels carrying red blood.*" (*b*)

Now our special object in bringing forward this subject again is not so much to show our author's method of reasoning upon facts, and negative results, as to say how untenable must be that conclusion which denies the open termination of capillary vessels, when the very instrument, upon whose failure the induction is founded, is incapable of detecting the *vessels* of the cornea which are admitted to *carry red blood* in their *natural* state. (See *App.* on *Microscope.*)

Bellinus assigned a philosophical reason, for his day, why hemorrhages do not proceed from open terminations of the vessels. Thus: —

"Apertio autem osculorum non datur, cum oscula ejusmodi multa sint, *continuæ enim inter se sunt venæ et arteriæ, et si non sunt continuæ, ipsarum sane oscula semper patent.*" He therefore concludes by anticipating the present theory, — "ut *transudent* sanguis *quasi transeolatus.*" (*c*) But he overlooked the parallel vessels which are destined for nutrition, secretion, &c.

(*a*) Practice of Physick, p. 326. (*b*) Physiology, vol. i p. 214 — 216.
(*c*) De Morbis Pectoris, Art. Hæmoptysis.

blood. In either event, it is the admirable expedient of nature to overcome disease; it being opposed to the wisdom and harmony of her designs to imagine any other final cause where art brings about the same result by analogous means. (1) What may be the contingent influences that determine one product or the other, may, perhaps, be gathered in a measure from what we have said in former sections. Much is clearly owing to the particular nature of the remote causes, much to the precise state of the veins in other respects, and much to the modifications of constitution which depend upon age, especially in relation to certain organs. It is sufficiently known that apparently slight differences in causes will determine remarkable differences in the products of the secreting vessels. (Vol. I. pp. 610, *note*, 628.)

In simple inflammation, unconnected with venous congestions, the pathology being different, and no obstruction existing to the free transit of blood to the capillary veins, the secreting series neither become so much enlarged as in hemorrhage from congestion, nor is there, upon physiological premises, the same action likely to be established. (Vol. I. p. 194—195.) This is confirmed by observation, since capillary hemorrhage is not a common phenomenon, where venous congestion is not concerned in its production. It appears, however, if what we have here and elsewhere said be correct, that, although the morbid products may depend upon inflammation as well as upon venous congestion, the immediate instruments are the same, and their action analogous. Thus we may understand how the doctrine of Seymour (2)

(1) It is useless to object that the products may become a source of greater mischief than the disease which it is its tendency to overcome. The law must operate uniformly in parts that are organized in a common manner, especially in such as are prone to congestion, and where the serous apparatus abounds. Hence we so often meet with effusions of blood in the brain, from the alimentary canal, and the lungs. Nature, however, always provident, has met this contingency by rendering the absorbent system subordinate to its demands.

(2) On Dropsy. — Seymour's doctrine is founded especially upon the frequent complication of hepatic disease with ascites. But, the supposed obstruction has never been shown anatomically, and we have gone over our proof against its existence. We may say, however, that ascites is rare in those instances where hepatic congestions are most strongly pronounced, and that, when connected with disease of the liver, the degeneration of that organ, the vital phenomena, and the success of bloodletting and its associate remedies, sufficiently prove that the organ itself has been invaded by inflammation, which, in a process of time, has involved the peritoneum. The statement of Bichat cannot be controverted, that, —

"It is *ascertained* by the numerous observations of dead bodies in modern times, that the production of dropsies belongs to every kind of organic affection; that the

which refers dropsical effusions to a state of venous congestion, consequent on some supposed obstructions to the venous circulation, is compatible with the laws of vitality, and that these laws supersede the necessity of the hypothesis which he advances, of referring the whole succession of phenomena to physical laws.

Capillary hemorrhage, therefore, may be generally considered indicative of the moment when inflammation is supervening, or is disposed to supervene on venous congestion. It is at this time,

lungs, the heart, the womb, the spleen, &c. can, as well as the liver, occasion them in the latter periods of the alteration of the texture; and that in this respect, they are but a symptom in the greatest number of cases, and a symptom *disconnected with any sort of compression.*" (*a*) (Vol. i. p. 458, *Sprengell.*)

These remarks are, also, more or less applicable to what has been lately said of the connection of dropsy with a diseased state of the kidneys. (Vol i. pp. 676, 681, *notes.*)

A careful attention to all the circumstances attending dropsies, whatever the nature of the remote cause, whether specifically morbific or the stimulus of vascular distension, results in our conviction of the simplicity of nature's operations, and that she has in those affections but one fundamental principle. We certainly need not expect lesions of structure nor vascularity in the peritoneal or cellular tissues, where one is prevented, and the other, if it have existed, (p. 319—327,) may be fully overcome by the effusion. Daily observation evinces the fact that depositions of serum arise from low states of inflammation, or a morbid state approximating, or consequent upon it, especially when this action is under the influence of habit. (P. 319; and vol. i. pp. 180, 186, 190, 270, 621.) Bichat is inclined to allow that dropsies may be sometimes owing to "a defect of action in the absorbents," and from obstructions impeding their circulation. But, that in these cases, the effusion is owing to morbid action produced by the stimulus of distension, (being, as Abercrombie says, "nearly analogous to inflammation," p. 229,) is obvious not only from the valvular structure of the absorbents, but from the following remark of our author. "Nothing," he says, "is more common than to see abdominal and thoracic enlargments of the lymphatic glands in children, without producing serous effusions, even at the *most advanced* periods. In opening the bodies of small subjects, *I have often been astonished at this phenomenon*;" (*b*) and so of the extremities; in which Armstrong agrees, (p. 262.) The salutary effect of bloodletting shows that dropsy is not connected with a "relaxed" state of the absorbents, or other deficiency in their action. Again we see it constantly fluctuating, suddenly increasing, and as suddenly diminishing, just as we observe the like changes taking place in inflammations. Nor are the contents of the absorbents of the same nature as dropsical effusions; nor are the latter alike. (P. 197.)

Finally, we may with as much propriety affirm that bile is strained off from the blood, as that serum makes its escape in this manner. (Vol. i. pp. 180, 581—608.) If there be any dropsical effusions that are not induced by inflammation, or a morbid action consequent upon it, or in other respects analogous, let us have something besides an unmeaning "debility of the vessels," or, what is the same thing, a process of "filtration."

But, it is not so much our object to investigate the pathology of dropsy, as to consider the bearing of facts upon some of our general principles.

(*a*) General Anatomy, vol. i. p. 397. (*b*) Ibid. vol. ii. pp. 104, 124.

that the comparatively obscure symptoms of the latter are succeeded by the more strongly pronounced ones of the former disease. Now we see pneumonia, or a development of tubercles, succeeding to hæmoptysis, (1) gastritis to hæmatamesis, and various chronic degenerations that evidently depend on inflammation. Let us hear M. Andral, watching nature at this important, critical moment.

"It is at the period," he says, "when the hemorrhage ceases, that the most formidable symptoms generally occur. In such cases the hyperæmia has only *changed its form*, but still persists; sometimes assuming *an acute type*, and advancing rapidly towards its termination, whether favourable or fatal; sometimes, also, retaining a *latent chronic* character, and laying the foundation of various alterations of nutrition in the organ where it exists. Thus *hæmoptysis* is sometimes *transformed* into *pneumonia*, *hæmatamesis* into *gastritis*, *menorrhagia* into *metritis*." "These hemhorrages repeated *lay the foundation* for the *development of tubercles* in the lungs, and of that degeneration termed cancer, in the stomach and uterus." (2) *This is an absolute contradiction to the universal doctrine of his rival anatomist, Louis.*

Who will not recognise the foregoing as a portrait from real life? And can it be entertained that the simple rupture of blood-vessels may lead to such terrific results? Will it be believed that no disease of serious moment preceded that hemorrhage upon which pneumonia, gastritis, cancer, supervene, and for the cure of which, when cure may be attained, bloodletting is the remedy? The anatomist tells us there is vascularity, and that it resides mainly in the venous system of the part.

Thence the aptness of the provision, whether the effusion consist of blood or of serum, which interposes at the moment when a condition of disease is about to ensue, which breaks up the organization. Or does inflammation fully supervene in the adjacent tissues, it reacts, as we have seen (p. 521) upon the venous disease, and the congestion is overcome. There is also reason to believe that this very subsidence of the venous affection reacts on the supervening inflammation, and that nature thus completes in a more complex manner her attempted cure. As congestion yields, the transit of blood becomes less embarrassed, and the subsidence of disease in the venous coats removes the morbid in-

(1) M. Louis says that the tubercle is antecedent. But we have shown in another place that he offers no proof of it. — *See Louis on Phthisis, sec.* 233, 240, 274, 394. Structural lesions are his ground of pathological conclusions; (See our *Commentaries on his writings.*) In the advanced stages of Phthisis, hemorrhage is often owing to ulceration.

(2) Andral's Patholog. Anat. vol. i. p. 53.

fluence from the capillaries engaged in the supervening inflammation, not only by withdrawing a source of vital irritation, but through the principle of sympathy, which is elicited as well by changes from disease to health, as by the opposite conversion. At other times, from a variety of causes, the predisposition to inflammation may become so great, or so specific, that its march may be irresistible.

Is it said, in objection to the doctrine of capillary hemorrhage, that blood has been often known to "exude" from the coats of the larger veins? In the first place, how will the mechanical philosophers explain the escape of the globules from the "invisible pores," and this too, where three tissues overlay each other? (Vol. I. p. 686.) If the "transudation," as is generally affirmed, be of this mechanical nature, how does it happen that the blood does not infiltrate between the different coats of the vessels? May we not think, therefore, that it does not in reality proceed from the contents of the larger vessels themselves, but actually from their *tissues?* And, if this must be necessarily allowed, will it not be equally necessary to surmise that the blood escapes from vasa vasorum, which have open orifices upon the outer coat of the veins; and does not, moreover, the physiology of other hemorrhages, and all that is known of other effusions without rupture, demand of the understanding this rationale of the process? Having thus gained these admissions, we inquire, next, whether the phenomenon do not go along with our other proof, to establish the dependence of venous congestion upon disease of the venous coats, and its inflammatory nature? We will not again argue the question whether the blood makes its escape in virtue of its "stagnation and coagulation" in the hemorrhagic capillaries, (p. 187, *note* 2,) as this is not relevant to our present inquiry, and as we have fully explored this philosophy in our Essay on the Theories of Inflammation. We are content that the doctrine should be indulged in the instance before us, so only we obtain a hearing as to the essential pathology of venous congestion. The veins are organized like other parts, and their coats, especially the external, abound with vasa vasorum. We shall not attempt to consider how far the capillary veins may be concerned in the process. They may or may not have an agency; but we have no facts, and analogy is against it, especially as hemorrhage some-

times arises from inflammation where there had been no venous congestion.

Of a similar nature to the effusions of blood which take place, either according to the process which we have attempted to describe, or from the parietes of the larger veins, are the serous effusions that so constantly attend on venous congestions. We have already endeavoured to show by facts and reasoning the importance of this result in identifying the pathology of venous congestion with inflammation. So glaring, indeed, is the analogy, that the best observers, without applying the fact pathologically, refer to the coincidence. Thus, Dr. Abercrombie:

"Whenever such interruption occurs in the circulation of a vein, it appears that the increased effusion takes place from the *exhalant branches of those arteries with which the vein is more immediately connected*, (p. 228) depending probably upon a state of congestion in these parts, which in its *effects* is nearly analogous to inflammation." (1) (Vol. I. pp. 259—260, 268—269.)

M. Andral and his school are powerful advocates of the *mechanical pathology* in the cases which we are now investigating. We had occasion to object to M. Andral's hypothesis regarding the mechanical transudation of serum. (Vol. I. p. 180—183.) We shall now examine the principal argument which relates to effusions of blood from the parietes of congested veins. This we shall do, however, only in respect to the premises upon which the reasoning proceeds; believing that we have sufficiently considered the subject in other aspects. We again allow M. Andral to speak in behalf of the anatomical and mechanical school.

"Pure blood may escape from the *over distended* vessels, just as water transudes through the permeable sides of a vessel *in which it suffers compression. To this source* are to be referred several hemorrhages and dropsies produced by simple transudation in a tissue mechanically congested; and, although these effusions have really *nothing active* in their nature, yet are they considerably diminished, and sometimes *altogether removed by bloodletting*, which, in such cases, acts in a *purely mechanical manner*, by removing *from the vessels* the fluid *by which* their parietes were kept in a state of *over-distension.* These *pathological* observations are quite exemplified in the majority of those cases of hæmoptysis, hæmatomesis, ascites, and other affections, which are connected with organic disease of the heart." (2)

(1) Diseases of the Brain, &c. p. 41.

(2) Andral's Patholog. Anat. vol. i. p. 42.

From facts which we have stated, (vol. i. p. 379—381,) and a multitude like them, it is evident that the venous engorgements of the lungs which are connected with organic affections of the heart, and the hemorrhages which ensue, are in no other sense mechanical results than as the violent action of the heart may be an exciting cause of disease in the venous system of the lungs. And here we may observe the

We have quoted the foregoing paragraph for the purpose, especially, of exhibiting the entire mechanical rationale of the nature of venous congestion, and how its physical results are brought about, as well as the "mechanical" philosophy as to the effects of bloodletting. As to the supposed transudation of blood, it is sufficient to say that the proof rests upon a comparison betwixt an organized living vessel, and a dead, inorganic one. The premises, therefore, being wrong, the hypothesis must fail or receive better support. That this, however, need not be attempted, we shall, as on former occasions, (pp. 542, 543; and Vol. I. pp. 180—181, 524, 525, 627 — 632,) allow our author to refute it himself. Thus:—

"There is yet another species of hyperæmia formed after death, which results from the transudation of the blood or some of its constituent parts through the coats of the vessels. *So long as life continues*, the fluids contained in the several cavities *cannot escape* through the *membranous* parietes of these cavities; but this impermeability does not depend on their texture or organization, so much as on the *vital forces* by which they are animated. When these forces *cease to exist*, the membranes become permeable to different substances which they contained during life."

The body being *dead*, "the bile transudes through the gall-bladder," &c. But, what is here remarkable, our author now raises a great doubt as to the permeability of the blood-vessels, and asks, "can the blood in like manner pass through the parietes of the arteries and *veins*? The following experiment *seems favourable to this supposition*." This experiment, (a chemical

remarkable fact, that dissections do not disclose inflammation, but only venous congestion; whilst no ruptures of vessels are detected. The exciting cause being, in these instances, more or less of a mechanical nature, and the affection of the veins being therefore analogous to the inflammations of other tissues that depend on mechanical causes, it is apt to subside, as in the latter instances, as soon as the exciting cause is removed, or as soon as the action of the heart becomes tranquil. This is the reason why hemorrhages, in these instances, are not apt to be followed by inflammation, or disorganization of the lungs; whilst, also, this consideration shows the modifying power of the predisposing causes of disease. (Pp. 478, 485, 502—503, 514.) Or again, if the subject of cardiac disease suffer also the scrofulous taint, then the venous affection is imbued with the constitutional influence, and may be followed by other pulmonary lesions. Here, as everywhere else, we see the importance of considering the nature of the remote causes.

The suddenness with which the pulmonary congestions sometimes spring up in the foregoing cases, is in favour of our conclusions, and corresponds with what is often witnessed of venous congestions from other causes, as well as in inflammations of other tissues. But there is reason to believe that the affection is often slowly progressive before the violent explosion takes place, and is one cause of the heart's disturbance. In these cases, too, we frequently meet with hepatic congestions, the removal of which is indispensable to the tranquil action of the heart.

one,) is made upon "a vein or artery in a *dead* body." (1) But our author on the same page again concludes in opposition to himself; for in considering the "*cadaveric* transudation of blood through the coats of the vessels," he sums up with —

"The *fact is*, that the serum and colouring matter pass through the vessels, whilst the fibrine remains behind either fluid or coagulated."

Nor is this all, for our author supposes that putrefaction is necessary to the result.

"But," he adds, "as the rapidity of putrefaction is extremely variable, it follows that in some cases *no trace* of transudation from *this cause* can be observed at the expiration of 24 hours." "It will be favoured," he says, "by moisture and a high temperature."

We may therefore safely conclude that our author was mistaken in supposing that "*pure* blood may escape from the over-distended vessels," &c. And we may here corroborate our author by the coincident statements of others. Thus Dr. Hope: —

"Complete transudation of blood may take place within the brief space of *twenty-four hours*, provided *putrefaction* be favoured by a high summer temperature and atmospheric humidity." (2)

And so, Beclard: "Transudation by organic pores has only been *supposed*. But, without knowing the mode in which the perspiratory secretions are performed, (Vol. I. p. 703,) we know that transudation takes place in the *dead body only*, and *then even some* time after death." (3)

And thus Bichat: "These phenomena (transudations) never take place during life." (4)

We have brought together these concurring statements for the purpose, also, of farther defeating the assumptions on which the humoral pathology is founded. (See Vol. I. pp. 451—460, 657, 668, &c.) And we have had yet another motive from having witnessed the perpetuation, and even a dangerous extension, of an exploded doctrine in our latest and best systems of pathology. Thus, Prof. Gross observes, that, —

"Assuming, therefore, that all the vessels are porous, the most plausible theory that suggests itself is that *all hemorrhages*, not dependent on rupture, are

(1) Ibid. pp. 48, 49.
(2) Op cit. p. 151.
(3) General Anatomy, p. 152.
(4) Gen. Anat. vol. ii. pp. 19, 69.

Fodére, and Magendie made experiments to sustain the doctrine of vital transudation. They are amongst the earliest and best that respect endosmose and exdosmose, and similar to that which we have seen of Müller, (vol. i. p. 565, *note*;) but they have the advantage of having been made on the living subject. (*a*) Dr. Bostock (*b*) assigns substantial objections against them; but better ones exist in the very nature of the experiments themselves.

(*a*) Magendie, Journ. de Physiol. t. iii. p. 35.
(*b*) Elementary System of Physiology, vol. ii. p. 469, &c.

caused by a sort of *endosmose, diapedesis*, or *transudation*, by which the elements of the blood are *forced through* the coats of the vessels." (1)

If this be physiology, or pathology, or have any connection with the science of life, or if, on the contrary, the premises relate entirely to the phenomena of inorganic matter, what should be the remedies for hæmoptysis, cerebral hemorrhage, &c. ?

But allowing, in respect to the effusion of blood from the coats of congested veins, that the process is purely mechanical, this construction would establish our doctrine of the pathology of venous congestion ; since we believe, from what we have now shown, that it will not be maintained that such permeability of the vessels can take place without an antecedent disorganization of their parietes ; and all analogy prompts us to refer this to inflammatory action. Even Bichat, who allows, in opposition to himself, of transudation under certain circumstances, qualifies his opinion by observing that, "it might be said that it is the blood which transudes, as in the *dead body*, through the pores that have not power to retain it." (2) This observation leads us to say, once more, that we contend for a purely active dilatation of the veins in congestion only so *long as their tissues remain unaltered*, or their molecules are not mechanically separated by intra-textural depositions. Nor can it be consistently supposed that any of these changes arise at the invasion of congestion, since the veins often acquire with great rapidity very remarkable degrees of dilatation, where no interstitial deposit is at all cognizable; and since, also, this corresponds with the natural dilatations of the veins where no interstitial depositions will be suspected.

Nature, however, is too uniform in her laws to admit the belief, that effusions of blood or of serum may sometimes depend on a passive and sometimes on an active state of the vessels, and again on a simple mechanical exudation. (Pp. 184, 192.)

"There can be no question that the laws, according to which in one case the blood-vessels pour out serum, in another secrete pus, in a third soften, in a fourth produce induration, are fixed and invariable in their operation; and each may possibly be attended with its specific and therefore diagnostic signs." (3)

We may at least infer, that, if the secreting series of the capillaries be the immediate instruments of hemorrhagic action in one case, they are equally so in all ; whilst this is confirmed by all the analogies that relate to any other product. And having

(1) Gross' Pathological Anatomy, vol. i. p. 77. (2) Gen. Anat. vol. ii. p. 15.
(3) Southwood Smith, On Fever, p. 137.

gained thus much, perhaps it would be no great stretch of analogy to conclude that all *morbid* products, pus, serum, lymph, secreted blood, &c. depend, respectively, and at all times upon common modes of action. Nay more, that when varieties take place in those products, they arise from a modification of action which is always necessary to that precise variety. The animal, and still more the vegetable, kingdom is full of illustrations of the principle in their natural habitudes. We have had occasion, for various purposes, to call up the philosophy; and referring to our other essays for what we may have said, we shall only add, that there is nothing fortuitous in the products of organized matter. They may be infinitely varied, but each variety has precise causes, remote and physiological. The vital properties must be so modified in each case that the capillaries, acting in obedience to those properties, shall decompound, and recombine the particular elements and constituents of each product; rejecting all the rest. Otherwise, indeed, there could be none of those astonishing changes occurring in the determinate manner that is necessary to the formation of the specific compounds, and there could, therefore, be no resemblances amongst any of the natural or morbid products. Everything would be confused, and we should have nothing but the riot of the chemical forces. Whenever, therefore, one product or the other may be presented, could we learn either from chemistry, or by other methods, the precise modification that exists, we should know that the vital properties and actions are affected in the present instance, as they had been in all other cases where similar products have been determined. If we attempt any other explanation, we come in immediate collision with facts, and find our hypotheses at war with nature, and only suited to the particular occasion. But, it happens that the properties and actions of life are subject to so great a variety of modifications when altered from their healthy standard, and these perhaps fluctuating more or less from hour to hour, that we see at once the absurdity of humoralism in attempting an analysis of disease by a chemical analysis of the variable products; allowing that chemistry has the supposed connection with the subject. But it so happens, that under the influence of all morbific causes, the vital properties can never undergo any change of this essential nature; and we constantly see this principle carried out till they become extinct. Hence the analogies amongst diseases, and the approximation of

these, according to the nature of the remote causes. It is the great foundation of the healing art; and were it otherwise, medicine would be utterly fruitless, a mere creature of circumstance, one perpetual experiment.

It is owing to the great fundamental law which we have just stated as to the immutability of the essence of the vital properties, that such striking analogies occur even amongst all diseases, and upon which our illustrious countryman must have founded his doctrine of the unity of disease. All true observers of nature see the principle, though they see it operating in connection with others. Here is one who has given us no little aid in our various inquiries: —

"There are," he says, "scarcely two diseases, however opposite in their phenomena when viewed in an insulated shape, that are not linked together by others partaking in the nature of both. At a first glance, the yellow fever and smallpox would seem immeasurably separated and widely distinct in every aspect; yet the plague presents a fair connecting link between them, as the polypus does between the animal and vegetable kingdoms." (1)

In consequence, also, of the foregoing laws of organization, the approximations of morbid conditions are such as to enable us to establish upon a certain combination of phenomena certain general principles of treatment, corresponding harmoniously with the principles through which the morbific agents have induced the adverse changes. The curative principles, therefore, will be liable, in all cases which are not exactly alike, to certain modifications that shall correspond with the apparent modifications of disease; and these are to be learnt, especially, from the vital manifestations. It is true that chemistry, overlooking the fact that vital agencies may *create* out of exactly the same proportions of three or more elements a thousand different substances, and identifying, therefore, the most dissimilar organic compounds, proffers, obtrudes its aid. But the more we examine its pretensions, the more are we convinced that it is upon the wrong track. Even in respect to physiological inquiries, the eye may be deceived by the minute detail of experiment, art may produce results which nature will never recognise for her own; and there are limits to the scrutinizing eye of the morbid anatomist. Where this uncertainty is likely to prevail against any great general principle, it is better to give up our senses than to renounce our understanding.

(1) Dr. Johnson, On the Influence of Tropical Climates on European Constitutions, &c. vol. ii. p. 49.

All our preceding essays, as well as the present, have been constantly concerned about facts and principles which have a direct bearing upon what we have been just saying, and the points of illustration are too numerous for specific references. This is one reason for having omitted an index. But, we have endeavoured to bring the relations of the work together by incidental references, especially in the last volume.

From what we have variously said upon the pathology of venous congestion we may now bring to its interpretation, especially, our essay upon the philosophy of the effects of bloodletting; whilst we cannot but think that we have shown ample proof in our present investigation, that loss of blood operates like all other vital agents, and in no respect in a mechanical sense, unless as it respects some of its ulterior results. The latter, and almost universal construction of its operation has proceeded mainly from the physical theories of life, and the mechanical doctrines of inflammation and venous congestion. It is one hypothesis built upon another.

We have endeavoured to show upon pathological and philosophical grounds, that spontaneous hemorrhage is a critical effort of nature, and that it is often indicative of an incipient termination of venous congestion in an obstinate and disorganizing inflammation; and, when we connect with those considerations the frequent subsidence of the most obstinate conditions of disease upon the supervention of sanguineous effusions which art cannot imitate, the proof appears to establish the various physiological and pathological aspects in which we have regarded the subject.. We come, then, to one, amongst various practical conclusions, more important than the rest, viz. that when hemorrhage proceeds from any vital organ possessing an outlet, and whilst it exists within the limit of safety, it calls for other means of treatment than such as most speedily arrest the effusion, unless, like bloodletting, &c., they reach the pathological cause on which it depends. (1) (Vol. I. p. 272.)

Having now brought our most difficult subject to a conclusion, and looking back on its magnitude and obscurity, and con-

(1) M. Duparcque, in speaking of uterine hemorrhage, condemns the empyricism of those who overlook all but the discharge of blood, and homœopathically regard the symptom as the disease itself. (*a*)

(*a*) On the Functional and Organic Diseases of the Uterus.

sidering how much more knowledge than we possess is important to its full investigation, we can only indulge the hope, that what has been said may be instrumental in leading those great minds that distinguish the present age to explore a disease, which, even in its circumscribed visitations, is more fatal to man than the sweeping epidemics which only now and then desolate the earth.

In looking about for help upon many of our important questions, we have often lamented our almost solitary position. In this emergency we have, as in other essays, gone to the facts and the arguments of the most powerful who differ from us; and where we have thought it practicable we have endeavoured to turn them to our advantage. This may sometimes give an appearance of asperity where none is intended. It has been in no respect our object to become a commentator upon the writings of others, with a single exception; and, therefore, we have confined ourselves to the questions at issue, without often adverting to the excellencies which cast into shade many of what we have considered important errors. If, in the prosecution of this task, we have controverted the doctrines of the best ornaments of medical science, we have done so from our general respect for their opinions, and because we know that they have the magnanimity to approve where truth is our common object: —

"*Quemadmodum Divina nec juvari desiderant, nec lædi possunt.*" (1)

With the exception of our Appendixes, especially that which relates to cold as a remote cause of venous congestion, we have now completed the circle of evidence which we had proposed to ourselves with the single view of illustrating the pathology of venous congestion, whose knowledge is indispensable to any rational or successful treatment. (P. 222 — 223.) We say our circle; for so we had designed it, — the pathology of congestion being the focal point, and our various facts forming the radii. Their concentrated force, it appears to us, entitles the subject to the serious consideration of those philosophers who cultivate medicine in its rational aspects; though with such philosophers we would have been almost willing to have entrusted the result either to the abstract phenomena of the disease, to its remote

(1) Seneca, de Constan. Sap. c. 8.

causes, or to the analogies which are suggested by the effects of remedial agents. But the subject being of acknowledged obscurity, (p. 219,) a powerful array of mechanical philosophy being enlisted against us, we have felt it indispensable to survey the whole ground, and even to lay a broad foundation by our preceding essays. We have flattered ourselves, too, that we might throw some light upon the various topics which form our chain of evidence; since each one is of practical importance in the cause of humanity. The facts, too, which we have embodied in respect to various topics, but possessing relations to each other, may not be without their specific interest. In the wide range which we have taken, it may be thought that more might have been left to inference; but, for any particularities that enlightened readers may consider superfluous, they will find our motives assigned in the language of Zimmermann, at pages 119, 300, 337; and Vol. I. pages 400, 457; who also justifies himself upon the ground that, "there are some readers for whom an author should think over all parts of his subject."

APPENDIX I. TO THE PHILOSOPHY OF VENOUS CONGESTION.

(See pp. 209, 217, 219, 266, 315 549.)

From what we have seen of the prevailing doctrine of "stagnation," it would not be remarkable if some philosophers, who have peculiar views as to this pathology of disease, should not include venous congestion, or "stagnation of blood" in the veins, in their category of pathological "stagnations." Upon this branch of the subject there is one prominent objector, to whose pathology of fever we thought it important to devote an appendix as a sequel to our Essay on Inflammation. It is manifest from what we have there said, that it should be an *a priori* conclusion with our author, that there can be no venous stagnation in any febrile affections. To have allowed "stagnation of blood" in the veins would have been utterly at variance with his doctrinal views of fever and inflammation, which, from first to last, place the essential pathology in "stagnation of blood in the capillary arteries." (P. 209, and Vol. I. p. 127—128.)

After the numerous citations we have made of the recognition, by the most distinguished observers, of venous congestion as an important affection, it would seem almost supererogatory to attempt to show the fallacy of any objections that may have been urged against it; and, more especially so, since Dr. Clutterbuck is the only other writer of consideration, who has thrown any doubt over the existence of this disease. (P. 217.) But as the latter author has supplied no facts, and has made no argument against the universal testimony of morbid anatomists, by which the phenomenon can be explained upon any other principle than such as we have assigned or have endeavoured to controvert, we have nothing more to oppose to the authority of an opinion which we should otherwise regard with deep respect. We will say, however, in relation to the conjecture that the blood consists simply of what is determined upon the veins by the contraction of the capillary arteries after death, that no such appearances take place in parts whose arterial action has not been disturbed, or where there is no ground to apprehend some morbid condition; whilst it should be borne in mind that the venous injection, in truly morbid congestions, generally far exceeds any imaginable quantity of blood that may be accumulated in the arterial capillaries during their inflammatory processes, and that in a vast proportion of the cases of venous congestion, those capillaries are found in their normal state. (P. 225, &c.) Nevertheless, as we have already said, and will again repeat, where determinations of blood occur in the arterial capillaries, whether in normal conditions, or in inflammation, there is a corresponding enlargement of the communicating veins; and this appearance may remain after death, and be even increased by the final contraction of the arterial capillaries, though for reasons already stated, the blood is far more likely to disappear

from the veins than in their morbid congestions. (P. 145—146, 252—255, 267; and Sec. 14.) It is of great practical importance to attend to this distinction.

We do not think that Dr. Clutterbuck's assumption requires any farther consideration; but, with Dr. Craigie it is different, since his declaration is predicated of another important hypothesis which is set forth as a fundamental element of the doctrine of fever. He assumes that the doctrine of venous congestion must be abandoned, as a corollary of his theory of capillary stagnation in fever; this philosophy being just the counterpart of Clutterbuck's. As he stands, however, alone in the philosophical world, and without any display of proof, we should have still passed the subject in silence, were there not some other reasons for speaking, and greater than those which prompted our remarks upon the mechanical pathology of fever. There is a vast class of the silent multitude, who have either given no consideration to the subject, or have carelessly regarded the congested state of organs as a post mortem occurrence. If we may, therefore, succeed, as in our argument upon the pathology of fever, in showing by the premises of our able author, that he contradicts his assumption, we shall, perhaps, have accomplished more than simply neutralizing the authority of his opinion. We have also in view the object of pointing out the importance of examining all parts of an author in their proper connection, and of showing, as we have done on other occasions, how easy a matter it will be to find a true observer of nature flatly contradicting any hypothetical opinions which are not founded upon natural phenomena. We gain, too, the farther advantage of adding some important facts to what we have already stated in our 7th Section. Our author, having laid down his doctrine of capillary stagnation of blood in febrile and inflammatory affections, (p. 209) proceeds to state the following corollary: —

"The views now given also explain the exact nature of the term *Congestion.* That term has been applied, since the distinctions introduced by Dr. Armstrong, to designate a particular kind of fever, in which *it is supposed the blood is unduly accumulated in the veins.* We now perceive that *the veins are really less distended than in the natural state*, and that in congestion the blood is *accumulated in the arteries.*"

The foregoing is an unqualified general doctrine; but, as it occurs, under our author's division of "simple continued fever," he seems to be sensible that it is out of place, and he therefore immediately adds, —

"Though I have introduced these observations in this place, they are certainly more applicable to the third and fourth forms, *typhus*, and *synochus*, of continued fever, than to the one under consideration." (1)

Now let us turn on to what our author says of the *facts* in relation to *epidemic synochus.*

"I have now," he says, "to advert shortly to varieties in its phenomena, *according as one organ is more particularly affected than another.*" — "I examined numerous cases of this disease during the epidemic of Edinburgh, both in the Royal Infirmary and the Fever Hospital of Queensberry house. In general, I recognised the appearances now enumerated, and *especially* remarked the following. Where the upper division of the skull was removed, the cerebral membranes were occupied with *large blood-vessels, filled* with dark coloured blood, generally

(1) Practice of Physic, vol. i. p. 278, 1836.

coagulated. *These* vessels were largest and most numerous on *each side of the longitudinal sinus*, and especially so towards its posterior end. Upon examination they proved to be *chiefly veins*, but, it was remarkable that the divisions of the internal carotid, and basilar artery at the basis of the brain, were equally distended with dark coloured coagulated blood. (P. 549.) "Limpid fluid was lodged between the pia mater and arachnoid, in the intermediate delicate filamentous tissue; and this fluid was always *most abundant* in the vicinity of large *venous trunks*." (P. 481, and Sec. 16.) "The *choroid plexus was always more than usually crowded with distended vessels*."

"The lungs were generally very dark coloured, much gorged with black blood, and clots of this were found in the large vessels."

"These facts, ascertained by dissection, show principally the effects of the morbid process of fever." "They show in this respect that fever proves fatal by the *morbid changes* which take place in the *circulation* of the brain and that of the lungs." (1)

We have made a short quotation, (p. 312,) from our author as to the state of the veins of particular organs in yellow fever. He has much more of the same nature, and, although nearly a repetition, we shall state it, in part.

"In the fatal cases of this disease, (the third modification,) which are numerous, the *veins* and *sinuses* of the head are *generally filled* with dark coloured blood; the arteries in like manner are *either empty* or *filled* with fluid of *this hue*. The *choroid plexus* resembles a clot of black blood more than an organic part." "The mesenteric *veins*, or even the arteries, if not empty, the liver and its vessels, the spleen and its vessels, and most of the intestinal canal, are darkened in the same manner."

"In the second modification," "the superficial veins of the brain are distended with dark coloured blood, and the choroid plexus appears like a mass of clotted blood," and so on. (2)

Our author is copious upon this condition of the *veins*, especially of the brain; and, notwithstanding what we have seen in our first quotation, Armstrong was never more employed in establishing the "distinction" of fever in connection with venous congestion; whilst our author scarcely indicates in the foregoing affections any other morbid appearance of importance, than venous congestion; and, as we have seen, he regards that condition of the brain and lungs, as "the principal effect of the morbid process of epidemic synochus," and that the "fever proves fatal by the morbid changes which take place in the circulation of the brain and that of the lungs." Our author says, theoretically, that, —

"The effect of the poison, in plague, is either to render the capillary vessels unable to transmit their contents, or to render their contents incapable of being transmitted, and, in short, to produce a *sudden*, almost *immediate* retardation of the motion of the blood, in the capillaries of all the organs, in all cases of plague, and in the most intense and virulent, stagnation more or less complete of the blood." (Vol. I. pp. 279, 420, *note*.) This *stagnation* "affects every tissue and every organ of the human body," and its "effect is, that while the capillaries and their arterial division are overloaded and distended with dark coloured blood, the *veins* necessarily receive less than their normal quantity, and these vessels consequently, are comparatively empty and flaccid."

(1) Ibid. pp. 325, 326, 327, 328, 329. (2) Ibid. pp. 209, 210.

We have seen, too, that it is an important part of our author's hypothesis, (p.209) that the stagnation increases in proportion to the degree of excitement that may exist. Now what are the facts? These may be seen by referring to our page, 313. Our author, himself, appears to have had no practical knowledge of the affection; but the only authors, whose anatomical investigation he notices and approves, are the four who distinguished themselves at the plague of Marseilles in 1720, and 1721. (Vol. I. pp. 453, 532.) From these we learn that turgescence of the venous system of important organs was the most remarkable phenomenon, whilst the arteries also contained dark coloured blood. This was the case with the brain, the lungs, and the liver. "The liver," says Craigie, "was in general enlarged, often marked by dark, livid, or purple spots and stripes, and its vessels were distended with much dark coloured blood." (1) These vessels were the veins. The reader will also recollect that it was found by Rigaud, in 68 subjects, that "the *arteries* were almost always *empty*." (P. 313.)

Great varieties, however, are occurring as to the state of the arteries which may communicate with congested veins, the causes of which we have endeavoured to explain, in part, in our 14th and 16th sections. The reader will have observed examples in this appendix, as well as in the Essay, where the arteries were full of dark coloured blood. This fact, in itself, is sufficient to disprove the conjecture of Clutterbuck, that the turgid state of the veins arises after death from the blood passing from the arteries into the veins. Here is a case from the records of hydrophobia.

"Dura mater of a dark hue; the larger branches of the *arteries* tense and turgid with *livid* liquid blood; the *smaller branches*, naturally invisible, *strongly injected* with blood of the same colour. The *veins distended* with blood of the same colour. The four *arteries* at the base of the brain were filled with the same kind of *livid brown* blood, (p. 549,) and so were their branches in the pia mater. Heart nearly *empty*, and so substance of the *lungs*." (2) (P. 239—See a very similar case from carbonic acid gas, page 482.)

We shall add a few remarks to what we have said of beriberi, (pp. 314, 476,) as tending farther to illustrate the pathology of venous congestion, as well as some other subjects about which we have been employed.

Mr. Hamilton, founding his opinion upon the turgid state of the cerebral, pulmonary, and hepatic veins, conceives that the pathology of this disease is to be found in "the obstructed circulation." "The numbness, spasms, and loss of power in the limbs," he says, "which take place in beriberi, I consider principally, if not entirely, dependent upon congestion of blood found to exist in the internal parts, and more particularly in the *brain*, and along the course of the spinal cord." (3)

Mr. Malcolmson, having a different hypothesis, which supposes "spinal irritation" to be the pathological condition, and being obliged to admit the existence of the alleged venous congestion, says, that "the *fluidity* of the blood, which is common in the fatal cases of beriberi, sufficiently accounts for the turgid state of the lungs, liver, mesentery, brain, and *perhaps* the spine;" Malcolmson having made, apparently, but two examinations of the spinal marrow, (Ca-

(1) Ibid. pp. 362, 363, 364, 365. (2) Marshall, on Hydrophobia, p. 92 — 103.
(3) In Edin. Med. Chir. Trans. vol. ii. p. 22.

ses 2, and 10,) upon the result of which his doctrine of "spinal irritation" is especially founded.

Now it is worthy of remark, that in all "the fatal cases" examined by Mr. Malcolmson, we read of no such thing as *fluidity* of the blood; but, whenever there was blood accumulated in the right cavities of the heart, it was *firmly coagulated.* We will state some particular facts relating to the only cases by this observer. Case 2. *Lungs healthy. White coagula* in right side of heart; left empty. Liver greatly enlarged, easily broken down. Kidneys greatly enlarged and soft. Much water between pia mater and arachnoid, and in ventricles. "Perhaps the only case on record of the dissection of an old beriberi patient." Case 10. Membrane of the spinal marrow inflamed, &c. well represented by a plate. "Upwards of two pounds of fluid in the thorax, *lungs healthy, pale.* Heart enlarged. Right auricle and ventricle enlarged and distended with *coagulum,*" &c. Case 22. "Heart pale, *flaccid,* and very soft. Lungs dark coloured, &c. Vessels of *pia mater very turgid,* and lateral ventricles full of water." Case 23. "Pericardium distended with water, adherent to pleura. *Lungs sound.* Numerous blackish spots throughout colon." (1) Nothing else. These are all the dissections, (except two very doubtful ones) and two of these are borrowed.

All writers agree, that venous congestion is the most remarkable cadaverous phenomenon of beriberi; and we therefore call the attention of the reader to the absence of morbid appearances in the lungs in 3 of the 4 cases, and the "flaccid" state of the heart in case 22, that these facts may go along with our others that were intended to disprove the doctrine of remora. (P. 239 — 257, &c.)

(1) On Beriberi, ut cit.

APPENDIX II. TO THE PHILOSOPHY OF VENOUS CONGESTION.

ON THE IMPORTANCE OF ANALOGY AND PRINCIPLES IN MEDICINE.

(See Vol. I. pp. 588, 603, 626; Vol. II. pp. 28, 223, 234, 299, 320, 337.)

"Qui novit, neque id quod sentit exprimit, perinde, est ac si nescerit." — THUCYDIDES.

"Analogy is the great chain of Nature, and the basis of all the sciences." — BACON.

"The phenomena of Nature are themselves a grand series of experiments, and open to the common observation of mankind." — JOHNSON, *Med. Chir. Rev.*

"Without principles, the practice of medicine is mere empyricism." — ARMSTRONG. *Letter to Dr. Boot.*

"If there be an inscription upon my tomb, I would have it that I was an advocate for principles in medicine." — RUSH.

THE science of medicine being founded mainly upon analogy, and a powerful effort being now made to exclude this great fountain of knowledge from the healing art, and to substitute the illusion that "medicine is susceptible of mathematical precision," and should repose alone upon "rigorous facts," the system which we have undertaken would be defective, were we not, as in the case of the microscope, to devote an article to an exposure of the errors of this innovation. Perhaps, however, this would have been sufficiently done by contrasting the opposition to analogy with the method of induction which prevails in the writings of M. Louis, who may be considered the chief of the mathematical school, and where we shall have seen that analogy is at the foundation of all his philosophy. Nevertheless, we are unwilling to leave so important a subject without its specific investigation; and, in effecting this purpose we cannot do it so well as by showing the indebtedness of other sciences to analogy, which are far more susceptible of precise demonstration than medicine. And, that we may concentrate this information, we shall exhibit, especially, as at the beginning of our Essay on Congestion, the general conclusions of those illustrious men who have brought other sciences to their present maturity.

Simultaneously with a disregard of analogy, sprung up the dogmatic exclusion of principles in medical science, and the substitution of the same method which was to prevail in the place of analogy; that is to say, an exclusive adhe-

rence to "rigorous facts." How far, also, this doctrine has been practically respected by its advocates we shall likewise ascertain in the essay to which we have just referred. In the mean time, we shall inquire into the advantages of general principles, and by the same process that we have proposed in respect to analogy.

To avoid repetition, and, at the same time, to lose no weight of testimony in our favour, we refer to our citations at page 215.

In our Essay on Venous Congestion, we have pursued a method of investigation which, at all former eras, has been employed as strictly philosophical, and as conducting to the truth with almost unerring certainty. A part of that method has consisted of inquiries into acknowledged conditions of disease, and of applying this knowledge to a class of affections less understood, but which possess the closest affinities with the former. This system of analogical induction has been brought into no little discredit in a science, which is truly made up of analogies, by an undue dependence on morbid anatomy, and by the induction of many philosophers that the living actions of nature as modified in disease can only be interpreted through changes of organic structure; which have come to enjoy, in consequence, the authoritative appellation of "rigorous facts." How far this is founded in truth, and how far it is subversive of physiological science, we have endeavoured to consider in other places. We are now only interested in showing farther the obligation of knowledge to the light of analogy.

Quintillian says, — "ejus (analogicæ) hæc vis est, ut id quod dubium est, ad aliquid simile de quo quæritur referat; ut *incerta certis probet.*" (1)

"Hippocrates totam suam philosophiam in hoc posuit, quod res congeniæ se mutuo appetant, dissimiles vero a se invicem recedant." (2)

And thus Baglivi: — "Certum siquidem est, quod in cunctis operibus suis analogice Natura procedit." — "Analogysmus ad proficiendas artes sub naturali scientia comprehensas, et *presertim medicinam*, cæteris argumentandi modis opportunior est." — "Analogysmus aperta constat in anatome, in chymia, in botanica, mechanica, et meteoris, aliisque innumeris artibus, ac scientiis, *quæ nec aliter adauctæ*." (3) — And so the poet:

> "Of things above we judge from things below;
> Whence can we reason but from what we know."

"The advantages of analogy extend to all objects that are not sufficiently clear in themselves. A thick cloud is spread over nature. This begins to open itself, and dissipate, the moment we are able to perceive some of the phenomena, together with their connection and causes, so far as we can judge from their effects. We proceed from those parts that are known, to other new ones which present themselves. Analogy, by means of certain general principles, enables us to combine together an infinite number of particular phenomena that are very distinct from each other. We consider Nature by analogy, either by discriminating or by comparing the phenomena, when it is impossible to know her internally." (4)

"If we observe a phenomenon which we never have observed before, it is absolutely impossible for us not to think of the analogous cases which we may have seen; since they are suggested by a principle of association, which is as truly a part of our constitution, as the senses with which we perceived the

(1) Inst. Orat. l. 1, c. 6.
(2) Keill, Med. Stat. Brit. p. 32.
(3) Prac. Med. l. 1, c. 6; et de Analog.
(4) Zimmermann, on Experience in Physick, vol. ii. p. 25.

phenomenon itself; and, if any of these analogies strike us as remarkably coincident, it is equally impossible for us not to imagine that the cause, which we know in that former instance, may be present in this analogical instance, and that they may, therefore, both be reduced to the same class." But, "so prone is the mind to complicate every phenomenon by the insertion of imagined causes in the simple sequences of physical events, that one hypothesis may often be said to involve in it many other hypotheses, invented for the explanation of that very phenomenon which is adduced in explanation of another phenomenon, as simple as itself." (1) (*See remarks on the writings of M. Louis.*)

There certainly "exist proofs, which one may venture to call *demonstrative: but, in medicine, we are seldom indulged with this sort of evidence.*" (2) The greater, therefore, is the necessity of resorting to the analogical process. Organic action is distinguished by an endless variety of results; but whether healthy or morbid, when a certain succession of phenomena are presented from time to time, there can be no doubt as to their dependence on established laws, and the principle will be still farther confirmed when we constantly observe that those results may be varied in a uniform manner by common agents which exert their effect through the medium of those laws. We take not one step in practical medicine which is not directed by analogy. Our principles are founded upon it, and it is the only guide of empyricism. We rely upon the success of depletion in phrenitis, or pleurisy, and we know that stimulation will be fatal in either. The whole certainty reposes, in part, upon the universality and specific nature of the vital laws; and this certainty is equally great, whether those laws concern the vital phenomena of disease, the influence of remedial agents, or the ultimate lesions of structure or other morbid products. They all depend upon common principles, and analogy is even better in the former than the latter cases. But this is only a part of our therapeutical guide. Every prescription is as well founded upon its known effects under analogous circumstances. Therefore was it said by a sound philosopher, in speaking of mental diseases, that,—

"If such examples as those above described admit of a *tolerably certain inference*, it may be extended by *analogy* to other cases in which the morbid phenomena are similar, though the causes and results of the disease may not be so clearly marked. And the morbid changes, in the protracted cases, being such as inflammation induces, afford an argument which cannot be set aside by *any method* of evasion." (3) (Vol. I. pp. 241, 246, 258, 260, 268. *Dr. Hall.*)

Whilst this mode of reasoning is concerned about the great laws of nature, we are employed in a pursuit that is eminently worthy the highest efforts of genius. The basis is solid, and fact after fact will conduct us to analogical principles that shall infallably serve us where some of the usual premises may be wanting. But, on the other hand, where inductions are formed, as we shall see in a conspicuous instance, in another place, from a few observations, or from a solitary fact, that are more the offspring of art and accident, than the ordinary operation of natural laws, then it is that analogy may become the foundation of important errors. And so especially of the "mathematical method;" when carried out to its greatest latitude. (P. 207.)

(1) Brown's Lectures on the Philosophy of the Human Mind, Lec. 8.
(2) Beddoes, on Fever and Inflammtion, p. 192. (3) Pritchard, On Insanity, p. 181.

But, it is not alone in relation to vital actions and their results that analogy is considered an imperfect guide, however it may be constantly employed as the great fountain of light. It has been absolutely neglected in respect to organization. Certain parts, like the crystaline lens, cornea, teeth, nails, &c. have been generally supposed to be inorganic substances; (1) as if dead and living matter were compatible with each other. The serous membranes have been thought to be destitute of vessels, notwithstanding the specific facts which prove their perfect organization, (p. 318, *note;*) and although Chaussier, Beclard, Williams, and others have demonstrated, what might have been left to analogy, a vascular communication between the uterus and placenta, still the communication is denied. It is only recently that analogy has proved its application in respect to a nervous system in worms and many of the lowest animals that manifest sensibility, where it has been found by Mechlis, Otto, Nordmann, &c.; and, if we may trust the microscope of Ehrenberg, it is present in the infusoria. We therefore regard it, says Carpenter, as an indisputable axiom, "that a nervous system, in the form of connected filaments, exists in all animals, (that is, beings endowed with any degree of sensibility and voluntary power,) although its presence may not be detected by our means of observation." (2) Fohmann, too, is said to have injected the lymphatics of the umbilical cord and of the cornea; Mayo, to have shown an interstitial absorption in cartilege; (3) and we doubt not that the application of analogy will yet be shown as it respects the existence of lymphatics in the placenta, the brain, and all other parts which are liable to absorption. (4) Analogy leads us to the conclusion, which art is fast illustrating, that nature carries on her processes in the most obscure as she does in parts of more manifest organization, where they relate to beings whose vital phenomena are alike. This consideration, therefore, leads us to reject the hypothesis that the circulation of the brain and the penis is governed by laws peculiar to themselves, or that the vesiculæ seminales may secrete semen, or the urinary bladder, urine. (Vol. I. pp. 588. 603, 703.) Müller, who is the author of the doctrine in respect to the penis, regards the placental function not only as an instance of "absorption by organic attraction," but supposes that the law in this respect is peculiar to the placenta; leaving every other absorbing process of the living body to be "accounted for by physical laws." "There is," he says, "only one part of the body in which this kind of attraction

(1) This is Muller's opinion, although the cornea is affected with "penetrating ulcers, granulations," &c. So the teeth "are not organized, but are only in connection with an organized matrix.' He "does not believe that a real vascular communication took place" in Hunter's experiments upon the cock's comb. (*a*) When Bichat adverts to the probable organization of the nails and hair, Magendie replies that "the nails and hair are no more nourished than mucous or urine." (*b*) Are the latter, parts of the body? Do they grow like the former; or, like them, are they subject to disease?

(2) Carpenter's Physiolog. Inferences from the Nervous System, &c.

How much more applicable is the latter clause of the foregoing extract to our induction as to open terminations of the secerning vessels. (See vol i. p. 685 — 690.)

(3) Med. Chir. Trans. vol. xix. Denied by Key, Ibid. vol. xviii.

(4) Here analogy is much stronger than in the case just quoted from Mr. Carpenter, since there are few parts in which the lymphatics do not abound, and have an important agency in the organic processes. And here we may found upon analogy a strong argument against the imputed absorbing power of the veins, since their only well ascertained function in any part is that of subserving the circulation of the blood. (P. 546.)

(*a*) Elements of Physiology, vol. i. pp. 214, 254, 383, 409, 412.

(*b*) Bichat's Researches on Life and Death, c. 7, p. 83, *note*.

(organic) certainly exists, and this is the capillary system of the placenta." (1)

We certainly look not for the same simplicity of causes in physiology as in physics. Still we believe that nature is always so precise in her general laws, that it is not less philosophical in explaining the phemomena of organized, than inorganic matter, to refer a connected series of similar results to a common cause; and, whether in health or disease, the identity of cause is established in proportion to the dependence of the effects upon constitutional principles; and the proof increases in an increasing ratio by the multiplication of the phenomena. "We should openly violate the rules of philosophizing, which direct us to assign the same causes for natural effects of the same kind, and not to admit more causes than are sufficient for explaining the phenomena, if we recurred, for the purpose of explaining the varieties, to the perfectly gratuitous assumption of originally different species." (2) "Dicunt enim philosophi natura nihil frustra fit per plura quod fieri per pauciora. Natura enim simplex est, et rerum causis superfluis non luxuriat." (3) "It seems to me," says Bichat, "that the explanation which represents Nature always pursuing an uniform course in her operations, drawing the same results from the same principles, has a greater degree of probability than that which shows her separating, as it were, this phenomenon from all the others, in the way which she produces it." (4)

Such, at least, is the simplicity of Nature, and such the analogies betwixt causes and effects, in the inanimate world; and shall we depart from this principle as we ascend in the scale, and believe that confusion prevails in proportion as Nature unfolds her power; that she then breaks up her great law of analogies, and leaves to chance the phenomena that spring from organized matter? The results, however, being more diversified in the latter instance, it may be often difficult in disease, to separate what is accidental from what is fundamental; and the interpretation of general laws may be less practicable when the whole system is impressed by morbific agents, and the complex operations of sympathy are shadowing forth their modifications, than in instances where the phenomena are more simple. But the relation of the predisposing and exciting causes is always foreign to the changes they produce in the powers and functions of the body. Those may be very diverse, and establish common modes of action; these are always the same, or nearly allied, where the phenomena are identical or connected by strong analogies. The remote causes of disease, even those which are supplied through the medium of sympathy, have no other law in relation to the living system than that of variously impressing the organic forces. But these forces are equivalent to established laws in themselves, and when they are affected in one way or another, a train of corresponding phenomena always follow. When these phenomena are otherwise independent of remote influences, the operation of a common law may be distinctly traced in common results. This is especially exemplified in local inflammations; and turning to one of the specific objects of the foregoing remarks, if we apply our principles to venous congestions, and begin with the uniform dilatation of the veins, and trace that phenomenon along the gradations of its cause, as illustrated by the parrallel phenomena of varix and phlebitis, and the coincident results of

(1) Ut cit. p. 248. (2) Lawrence's Lectures, ch. 9. (3) Bacon, Nov. Org. l. 2. Aph. 2.
(4) General Anatomy, &c. vol. ii. p. 54.

common remedial agents, we shall reach a point at which conjecture ceases, and principle becomes established.

In complex forms of disease, the phenomena which distinguish the laws upon which they depend in different parts may be more or less disguised by contingent causes of which we have spoken, especially by lesions of structure, and even by the influences of sympathy. This is particularly true of compound diseases, as when common inflammation is complicated with idiopathic fever; but more so when the latter affection is associated with venous congestion, and most of all when the three conditions coexist. The power of an analytical mind may then be as necessary to designate the modifying influences of one affection over the other, as to determine the effects of our planetary system upon the periodic return of comets. Halley was considered visionary for a month, when his calculation failed; but it was soon shown that the fault was in the calculation, and not in the law of gravitation.

There is nothing more applicable to our present subject than that which teaches us that, "although throughout nature nothing really exists but individual bodies having simple individual actions, according to a law, (edentia actus puros ex lege;) yet in every theory, that law, and the tracing, finding out, and explaining it, is the foundation of science and experiment." (1) The aphorism is a distinction between true theory and hypothesis. It condemns the speculative philosophy which assigns a cause for every effect, without ascertaining facts and the objections that may exist. But a law being ascertained, it becomes a key to a variety of *forms*, as they are called by Bacon. "In forming an axiom, another mode of investigation must be employed from what has been hitherto in use; one that will apply to the principal and all the intermediate axioms. The induction which will be truly useful in demonstrating the arts and sciences, should analyze nature by rejecting and excluding, as far as is necessary, and afterwards forming conclusions on what may be ascertained." (2) Analogy is allowed its full weight, in opposition to the negative results of experiment.

It will be seen that the foregoing principles have an extensive bearing upon all the subjects which we have investigated; and so far as analogy is concerned, in its specific relation to disease, we may safely affirm that, "as similar diseased conditions commonly produce the same or similar results, we infer from the occurence of the latter the existence of the former." (3) And when we bring to the aid of this proof the analogous results of curative agents, which make their impression on the same forces through which the morbid changes had been induced, we shall have attained all the certainty of which medical physiology is susceptible.

From the foregoing considerations, we reach the unavoidable conclusion, that medicine is a science of principles, as well as of abstract facts. The foundation of the former is the leading principle of Nature, — "res corporeæ admirabili quadam, eaque æterna et constanti regula, gubernantur." (4) It is for this reason, "a single just principle in our science will lead to more truth, in one year, than whole volumes of uncombined facts will do in a century." "Should we build facts upon facts till our pile should reach to the heavens, it

(1) Bacon, Nov. Organ. l. 2. Aph. 2.
(2) Bacon, ibid. l. 1. Aph. 105.
(3) Brit. & For. Med. Rev. No. 3. p. 126.
(4) Baglivi, Op. p. 27.

would tumble to pieces if it were not cemented by principles." (1) "In endeavouring, therefore, to explain any of those phenomena which have hitherto evaded research, it may be anticipated, that if such inquiries be successful, they will not lead to the discovery of any new laws, but unfold the same simplicity of means for performing those operations of the economy which have already been discovered." (2) "Fatemur quidem hanc qualem volumus theoriam in medicina parum prodesse, nisi frequens etiam accedit praxis; quæ tamen si hac duce dirigatur, erroribus, quos alias frustra cavet, minus erit obnoxia;" (3) or even as Quintilian has it, "plusque si separes, usus sine doctrina, quam citra usum doctrina, valeat." (4) "Multi nimium rationi tribuunt, et nihil experientiæ; multi contra faciunt. Utrique æqualiter peccant, unde tot inter medicos contentiones, tot inter theoriam et praxim dissidia." (5)

Aristotle says, that simple experience consists in the resemblance of particular cases; but he denominates those who employ experience without principles mere "automata." Such as unite both he calls men of skill and erudition. The opinion of Senac, as illustrated by his excision of facts, is important upon this question. (See our Vol. I. p. 298.) "The establishment of just principles," he continues, "besides being the proper and natural end of observations and facts, is certainly what confers upon them their highest value." Zimmermann, too, "after having observed diseases with the greatest care, had often found that great medical writers had described all, and sometimes much more, than he had seen. There are few authors, indeed, who will support this comparison; but they who do support it, render our experience the less necessary." (6) "It is wonderful," said Samuel Johnson, "how little good Radcliff's travelling fellowship has done." "It is in vain to send our travelling physicians to France, and Italy, and Germany, for all that is known there is known here. I'd send them out of Christendom,; I'd send them among barbarous nations." (7) "Those who are incapable of generalizing, seek in books for particular details only. Every author, therefore, who does not tell them all they are ignorant of, will appear to them unworthy of being read." (P. 568.)

"Sparks electric only strike
On minds electrical alike."

"Theoriæ munus est rationem reddere phenomenon in morbis apparentium antecedentia cum præsentibus recte comparare; occultos morborum causas, et veros causarum fontes investigare, aliaque id genus explanare, ut medicus, in determinandis indicationibus apertius nec empiricorum more progrediatur." We believe "that all practical researches ought to be built on an induction of facts, and that single objects or events are of little value but so far as they stand related to others." "Those analogies, in which all suggestions and rational conjectures originate, can only be ascertained by an enlarged view of nature." (8) The natural progress of the human mind, says D'Alembert, is to rise from individuals to species, from species to genera, and from proximate genera to remote ones; so that, at each step we form a science, or add a new branch to the science already formed. Induction may not lead to perfect knowl-

(1) Rush's Med. Inquiries, vol. iv. p. 43, &c.
(2) Wardrop, on the Nature and Treat. of Dis. of the Heart, part 1, p. 13.
(3) Friend's Emmenologia, p. 192. (4) Orat. l. 12, c. 7. (5) Baglivi, Prac. Med. l. 1, c. 2.
(6) On Experience in Physick, vol. i. p. 92. (7) Boswell's Life of Johnson, vol. ii. p. 385.
(8) Sir G. Blane, in Med. Chir. Trans. vol. iv. p. 89.

edge; but, "when the probabilities are drawn from experienced facts, to which we have been led by the senses, they have a right to be established as so many fundamental propositions."

Such are the doctrines of men of genius, — the guardians of true science. We have quoted them under the penalty, that "the reader may, and perhaps with justice, censure our profusion of authorities." (1) But, whilst other sciences are culminating beyond the reach of ordinary minds, that of medicine is threatened by many with a dissolution into its original elements. "In contemplating the operations of the inorganic world, nothing is perceived but harmony, regularity, and exactness; whilst, if we regard the phenomena of nature in the animal and vegetable kingdoms, as they are now interpreted, we discern only confusion and uncertainty." (2) Let the present adherents to "rigorous facts," and those who attempt their "interpretation" by the laws of physics answer to the charge. In Bacon's day, the wildest speculations led him to affirm that "medicina in philosophia non fundata; res infirma est." (3) Bacon, however, read and admired the works of Hippocrates, to whom he was not a little indebted for his rational philosophy. (4) True, the same speculation is more rife than ever, but under the disguise of "rigorous facts." It is now with hypothesis, as it was with inductive philosophy, when our fathers flourished; "Hippocrates being the first who joined analogy to a severe logic; whilst those of the empyrics, who openly condemned all reasoning, secretly adopted it." (5) Even "the votaries of medical science, in their zeal for observation, seem to have lost sight of *induction*, — so indispensable to give validity to their labours. We consider it the besetting sin of medical inquiry at the present time." (6) In medicine, says Mr. Morgan, after his *elaborate hypothesis* of inflammation, "theories are truly the soap-bubbles with which the grown up children of science amuse themselves; while the idle crowd dignify these learned vagaries with the name of wisdom." (7) It is the same doctrine as taught by that original empyric, Serapion; but like him, also, the present exclusives mingle hypothesis with their facts, and are the very men who most encumber science with speculations. "They are like unto blind-folded men, propelling a load of theory with strenuous efforts to preserve a straight line, but deviating to the right, or left, or retrograde, as the inequality of the course determine." "Atque hinc fit, quod recentiores morborum explicationes nihil aliud plerumque sint, quam philosophorum fabulæ." (8) Indeed, "it is common to almost all authors to generalize too much the facts observed under *particular* circumstances." (9)

(1) Beddoes, Researches on Inflammation and Fever, p. 5.

(2) Grangier, On the Structure and Functions of the Spinal Cord. *Preface.*

(3) De Argument. Sci. l. 4, c. 2. — "Quod plurima falsa multum tanta probabilia, perpauca evidenter vera contineat, non statim rejicienda est scientia maxime salutaris." — *Keill.*

(4) Ibid. c. 1 and 2. — The following was the ground-work of Hippocrates: "Cognoscuntur itaque demonstratis jam artibus, et nulla est ars, quæ non ex specie quadam rerum videatur: arbitrorque sane ego etiam nomina artibus ex speciebus accipi et imponi. Absurdum enim est, existimare ex nominibus species rerum germinare et provenire: imo etiam impossibile existit. Nomina enim lege quadam naturæ indita sunt: species autem rerum non lege sancitæ sunt, sed naturæ germina existunt; atque hæc quidem, si quis ex prædictis non ad plenum intelligat, in aliis sermonibus fortassis clarius edocebitur." (*a*)

(5) Zimmermann on Experience in Physick, vol. ii. p. 24.

(6) American Journ. of Med. Sciences, vol. xxi. p. 154.

(7) Principles of Surgery, p. 41.

(8) Keill, Tentam. &c. Præf. p. xix.

(9) Bichat's Gen. Anat. vol. iii. p. 40.

(*a*) Hip. de Arte, v. 23 — 34.

In those by-gone times, however, when sound medical principles were in vogue, that man of discernment, Dr. Johnson, was wont to say that "he had, in general, a peculiar pleasure in the company of physicians." (1) He valued their conversation, not for the multitude, but for the intrinsic worth, of their facts, and the wide range of thought to which their generalizations conducted him. The very astronomer and mathematician inculcate our duty. "In the opinion of Lacroix, Laplace, and Condorcet, reasoning, logic and induction, are in medicine not less useful, or less certain, than numerical calculations. Even in Germany, in almost all points, calculation has hitherto only proved what reasoning had already suspected." (2) "Theory," said Laplace, "is only common sense applied to calculation." "Some have pretended," says a distinguished Reviewer, "that the obscure Flamsteed is entitled to a large share of Newton's glory. With equal justice the organ-blower might lay claim to the merit of the music of Mozart. The mere observer is only a higher order of mechanic. His Historia Cœlestis displays only his untiring industry, and the acuteness of his senses. He is but the hand of Astronomy, and may not wear the crown of glory fitted to the head which combines his observations into a harmonious theory." (3) (*See our Remarks on the Writings of M. Louis.*)

The evil does not arise from any greater obscurities in the science of medicine; for here the phenomena, the true sources of knowledge, are far more diversified, and "whenever we have gained a step in physiology, we are struck with the remarkable simplicity in the means which nature employs for accomplishing what had to us appeared a most complicated function." (4) And have not Serres, Meckel, Cuvier, Tiedemann, Raspail, St. Hilaire, penetrated the apparent labyrinth of organized structures, and reduced their primary development to a most simple system?

There is a large class of philosophers who "endeavour to bring all opinions to the test of experiment and strict observation; and if they are found not to stand these tests, reject them from their creed without farther ceremony, although they should be sanctified by all the medical apostles who have written since the days of Hippocrates." (5) (*See our remarks on the Writings of M. Louis.*) They know not the spirit of the maxim, — "ab experimentis omnis scientia naturalis orta, firmiter valenterque procedit;" (6) or, as expressed by Celsus, — "quod vero proprius est, vix ulla perpetua medicinalis ars recipit, idem sunt, quod ii, quos experimenta sola sustinent." (7) "Homo naturæ minister et interpres tantum facit et intelligit, quantum de naturæ ordine, ne vel mente observaverit; nec amplius scit aut possit." (8)

Although "there is much in experiments to captivate, history tells us there is even more to mislead. There is scarcely a physiological absurdity that has not been supported by an array of experiments. They are to physiological science what *cases* have been to medicine, — *teeming with fallacy* and falsehood. Still we esteem experiments most highly; but we depend upon them *only when they chime with the great phenomena of nature.*" (9) There is said, however, to be "in Great Britain a very strong, and proper feeling against that indiscrimi-

(1) Boswell's Life, &c. ut cit.
(2) M. Double.
(3) North American Review, No. 102, p. 147.
(4) Wardrop, Op. cit.
(5) Moore's Medical Sketches, p. 217.
(6) Keill, Tentam. Med. Phys. *Præf.* p. xi.
(7) L. 1, p. 14.
(8) Bacon, in Nov. Organ. l. 1. Aph. 1. Et. de Interp. Nat. Sent. 1.
(9) Dr. Johnson, in Med. Chir. Rev. vol. xx. p. 324.

nate employment of experiment which characterizes some of the continental schools; and the objection applies not only to the cruelty, but to the worthlessness of the practice." (1) Another class, disgusted with the numerous fallacies of experiments, and the conflicting hypotheses erected upon them, have no confidence in any. "Incidit in Scillam qui vult vitare Charybdim."

"There should be established," says Bacon, "a true and legitimate union between the experimental and rational faculty, forever; the undue separation whereof has caused the greatest disturbance in the family of mankind." (2) And let us not forget that the philosophy of Bacon was the philosophy of Hippocrates; and that the idea of gravitation was originally the fruit of a mind well disciplined in medical induction. Whilst, therefore, "we religiously abide by facts, we can do nothing so well as investigate principles." (3) "We ought," says Hippocrates, "to join philosophy with medicine and medicine with philosophy; for a physician that is a philosopher is like a god; ιπτρος γαρ φιλοσοφος ισοθεος (4); and again, λεγε πρατικως, και πρατε λογικως. "Whereupon we must conclude, that all the affectation of despising theory in the medical art must either arise from not distinguishing the erroneous theories which are founded upon imaginary hypotheses, or false data, from those which are founded upon observation and facts; or, neglecting to observe diseases, nature, and what she indicates; or, not being instructed in the method of reasoning truly from such observations on diseases, their causes, and from nature." (5) "A very few atoms of knowledge, added to each other, times without number, have produced the greater part of that enormous mass of bibliography under which our science at present groans and is stifled." (6) The remedy is the reduction of these facts into a few simple principles; or, rather, condensing them into the principles already established by such masters as Hunter and Bichat. We see many signs of an approaching revolution, and have reason to think that not only the encumbrance of superfluous facts will be soon hurled from the science, but "we most confidently anticipate the time is at hand, when all the rubbish of 'anomalous cases,' and 'intractable and mysterious diseases,' will be swept from our periodical literature." (7) (Vol. I. p. 298, *Senac.*) In this hope we are encouraged by the assurance, that "the present age is characterized by an eagerness and a longing for the enjoyment of principles," though "they are themselves but faintly seen, and as yet but imperfectly understood." (8) "An idea must have been entertained some time or other by every student accustomed to reflect on what he heard or saw, of its being possible, out of the mass of ill-explained practical facts in medicine, to draw a clear continuous thread of medical principles," (9) and "we are sanguine enough to believe that medicine is really beginning to assume the characters of an inductive science," and "would be glad to see the science of physiology based upon a more extensive generalization of the *phenomena of vitality* than has usually been thought necessary." (10) "We must concern ourselves with principles. We cannot help it. All men do it in some sort or other; for the mind is not able to keep in view all the particulars of its

(1) British & Foreign Med. Rev. vol. v. p. 99.
(2) Preliminaries, s. 1. § 22.
(3) Beddoes' Commentaries on the "Laws of Irritability."
(4) De Decent. Ornatu, v. 53.
(5) Inquiry into the Method of Improving Medical Knowledge, p. 330.
(6) Mac Divit's Report of the Kent and Canterbury Hospital, 1837.
(7) Marshall on Diseases of the Heart and Lungs, p. 90.
(8) Eclectic Journ. of Med. vol. ii. p. 276.
(9) British & Foreign Med. Rev. vol. v. p. 200.
(10) Ibid. vol. iv. p. 285, &c. 1837.

own experience." (1) This necessity for generalization is the best proof of its practicability; and since the facts are more multifarious in the organic than in the inorganic world, the rule of necessity operates with greater force in demonstrating the applicability of inductive philosophy in the former instance. "Whilst too much attention cannot be paid to facts, too many facts crowd the memory without advantage, any farther than they lead us to establish principles." (2) "The methods of eclectism are analysis and induction; its result, the understanding of indications, with the knowledge of the best modes of fulfilling them. In short, it is the logic of facts, enlightened by the logic of thought. Yet, to many this method is unpalatable. Some are too impatient, some too indifferent, while others are incapable of pursuing continued trains of reflection." (3) "The man of genius knows how to doubt, when he perceives that the reasons why any particular thing ought to be believed are of little value; and, on the other hand, he knows how to act where there is greater reason for certainiy than for doubt. Men of little minds are not susceptible of doubts of this sort; and they who *do nothing but doubt* are incapable of acting like men of genius." (4) "Igitur, ut ad propositum meum redeam, rationalem quidem puto medicinam esse debere; instrui vero ab evidentibus causis." (5) "He, who derives his medical knowledge from books alone, and whose exalted notions have not been moderated by experience, will practice medicine as the philosopher who declaimed on the art of war to Hannibal would have conducted an army; he who has seen much practice without reasoning, as one of Hannibal's pioneers; and he, who joins the greatest natural acuteness and all the powers of reasoning, as Hannibal himself;" (6) and, "although the love of truth is, alone, the predominant passion of a man animated by a genius of this kind, it will be right to avoid being often with men of *weak heads*. The too frequent conversation of these people sometimes brings us down to a level with them, when we are least aware of it." (7)

"It is well," however, "that there should be in physiological science, as elsewhere, a conservative section, who may restrain the movement party from advancing with unsafe rapidity." "Nevertheless, we should like to see more regard paid to physiology, as a *science*, —that is to say, as an expression of general principles, and not a mere collection of facts." (8) And as to medicine and surgery, "it is evident that, as their essence consists in their practical applications, they are to be regarded as arts, and not sciences (?) and as their rules are as yet so little founded upon general principles, and stand so frequently upon the narrow basis of a limited induction, that we must consider them as rather empyrical than scientific;" and yet, "we think it will appear, that by the cautious generalization of correctly-sifted and exactly-observed facts, abstract principles may be developed, possessing a certainty not inferior to those of the so-called *exact sciences;*" (9) and, although "medicine, as an *empyrical* art, has advanced far beyond the science of pathology, and has little claim to be regarded as a *scientific* art," "we trust that we have shown," *throughout our Commentaries*, "that the actions of living beings, whether nor-

(1) Dr. Latham's Lectures on Clinical Medicine, p. 70.
(2) Hunter's Lecturcs on the Principles of Surgery, p. 8.
(3) M. Double, Lon. Med. Gaz. May 13, 1837. (4) Zimmermann. (5) Celsus, L. 1. *Præf.*
(6) Moore's Med. Sketches, p. 59. (7) Zim. vol. i. p. 113.
(8) Brit. & Foreign Med. Rev. vol. v. p. 100. (9) Ibid. vol. vi. pp. 106, 109, 1838.

mal or abnormal, are as amenable to general laws as those of inert matter; and that the discovery of those laws is within the reach of those who search after them in the right track." (1)

It is said by a philosopher, that "in general, experience is considered the simple produce of the senses. The understanding seems to come in for so small a share, that every thing that is intellectual in it is regarded as having as much of materiality in it as the perceptions of the senses. This is what I call *false experience*." "It is a blind routine, directed by no law whatever." But such is not the common acceptation, and "the old man is generally supposed to have thought more than a young one, because he has seen more." But, it is nevertheless true, that "he, who at thirty years of age is not an able minister, an able general, or an able physician, will never be so." And as to the truly great men, Young compares them to Minerva. They come from the hands of nature, as she did from the brain of Jupiter. Dubois says, that "cultivation may improve, but cannot extend genius." Zimmermann considers genius an indispensable element in a physician. "It has been already remarked," he says, "long before my time, that the increased number of years and patients only serve to remove physicians, destitute of genius, farther from true physic. The more their practice is augmented, the more numerous and considerable are their errors. On the contrary, we observe, that by genius, a physician is enabled to penetrate the greatest difficulties, *even from his youth*; and that, with this to guide and support him, he rises superior to every obstacle." Witness, Bichat, Baglivi.

The mighty genius of Newton launched into the great principles of nature, from whence he descended to the particulars of which they are composed. (2) He first erected the superstructure and laid the foundation afterwards; — at least, this may be affirmed of much of his ratiocination. "Omnibus' ornatus excellere rebus," the fall of a stone and the terrestrial revolution of the moon were to him analogous phenomena, — and so of the rest; and yet all experience is adverse to the principle on which motion depends. He was conducted by laws to their consequences; and as the latter were partially ascertained, he was moved on by an augmented impulse through the unknown and boundless field of nature. And when his vast discoveries were finished, many might have been still doubtful, had not their truth been confirmed by the principles from which they were deduced, as well as by the methodical rules of induction. This is a rare property even of genius, and is allied to that Great First Cause who established the laws upon which the particulars depend.

> "Superior beings, when of late they saw
> A mortal man unfold all nature's law,
> Admired such wisdom in an earthly shape,
> And show'd a Newton as we show an ape." — *Pope.*

Pascal created within himself the science of geometry; and when at the age of 16 years he wrote a treatise on conic sections, although prohibited the use of mathematical books, Des Cartes attributed it to a mature and cultivated ge-

(1) Ibid. p. 110.

(2) "The application of a general law to particulars admits only of probability, which may, indeed, as the induction has been wider, and the circumstances of observed analogy more numerous, approach more or less to certainty, but must always be short of it in its nearest approximation." (a)

(a) Brown's Lectures on the Philosophy of the Human Mind. lec. 9.

nius. Flamsteed saw, for the first time at the age of 14 years, an astronomical book, when he immediately calculated an eclipse. What we have said of Newton is more or less true of that "greatest, wisest of mankind," who said "he wrote for posterity, not for his own times." "Ρηιδιον θεοις;"—it was an easy matter for the gods. "Φιλοσοφους εγω ονομαζομαι." (1) "The slow progress of discovery arises from the obstacles which our feeble power of discrimination presents to our mental analysis."

Although we may not hope for a Newton in medicine, we may safely reverse the order of his philosophy, and ascend from particulars to generalization. A few simple facts shall ultimately announce the entire pathology of the most complex derangements. The countenance alone may reveal to the eye of genius what others would regard as an impenetrable labyrinth of disordered actions. The same, also, is true of the modifications induced by remedial agents, and the art of prescribing depends upon their foreknowledge. We proceed in these inductions upon the broad foundation of principles. They may be occasionally disturbed by erratic phenomena; but, like the course of the comet, these will be found to be under the direction of common laws, and strengthen our dependence upon fundamental facts as a sure basis for generalization. One good history of a disease is a permanent legacy to all posterity. The diseases which prevailed in the time of Hippocrates were the diseases of the present age. The portraits which he drew of them seem to have been taken from the subjects with which we are daily familiar, so "impossible is it for nature to contradict herself." The most sagacious physicians think they have seen nature properly when they have seen her as he did. It is from observing that he has described all that we see ourselves, and nothing more, of the phenomena of disease, that he has been designated through all time "the father of medicine," and "the divine physician." It was always nature which he described, and nature *is* the same now as then; "et quia naturæ sunt maxime consentaneæ, ideo prestantissimæ etiam habentur, quas de morbis conscripserit, historiæ." (2) What, therefore, shall we say of the doctrine which is inculcated upon students in medicine, that, "it is not true, as has been said too often, that *facts* do not become old," and that, "the *best work is good only in relation to the epoch at which it appears?*" (3) Friend thinks that Hippocrates has said all that is essential upon the phenomena of disease, and that these phenomena indicate sufficiently the method of cure. With the accumulated knowledge of ages in which Boerhaave was so deeply versed, he frankly avowed that his own aphorisms were inferior to those of Hippocrates; nor "could he wish for the reputation of a good writer when he compared his aphorisms with those of the ancients, and judged himself by them." Lawrence thinks "we have had workmen enough to toil in the mine and the quarry. They have raised and roughly fashioned an abundance of materials." (4) And so Senac. (Vol. I. p. 298.) "I have done my part," says Sydenham, "by mentioning the indications to be considered, and pointing out the time, and manner of doing it; for the practice of physic consists chiefly in being able to discover the true curative indications, and not medicines to answer them; and they, who have overlooked this point, have

(1) Lucian.
(2) Friend's Emmenologia, p. 193.
(3) Louis, on Typhoid Fever, vol. i. *Advertisement.*
(4) Lectures on Physiology, &c. p. 65.

taught empyrics to imitate physicians." (1) "Sydenham was inclined to general histories and rejected particular ones. Friend was of a contrary opinion. Both seem to be necessary. Although nature is simple in her appearance when viewed in the whole, yet she varies in her parts; and it is therefore necessary to be acquainted with her in both." (2) And yet no one knew her better than Hippocrates, though he gathered his information, according to Galen, in small towns, insufficient to support a single physician. But, as Socrates said of another ancient, "he spent his time in inquiring, considering, and consulting." He was, as we have intimated, the true author of the Baconian philosophy; and Zeno, the founder of the stoical sect, borrowed his fundamental principle of observing nature from Hippocrates, as he did his doctrine of the vital principle; though, unlike Hippocrates, (p. 283 *note*,) but in the modern sense, he extended it to all inanimate matter. (P. 117, *and Vital Powers*.) It is remarkable, too, that Hippocrates should have been so well acquainted with nature, since it is probable that neither he, nor other early ancients, made many experiments except with remedial agents, were but imperfectly acquainted with the circulation, and had only an acquaintance with general anatomy. But, with all our proficiency in the latter, we certainly do not excel Hippocrates in the treatment of disease, save in some chronic, and most of them incurable, cases. Nor do we surpass him in the art of divining disease from the phenomena of respiration, which to him, was as the pulse to us. And so of the natural and morbid excretions. Nevertheless, there is abundant evidence in the writings of Hippocrates that his inquisitive genius had neither neglected healthy or morbid anatomy. He describes, in a general manner, every organ of the human body, the arteries, veins, nerves, &c. Almost all our anatomical terms of any practical importance to the physician are familiarly employed in his writings; and from what he often says of the mutual dependencies of all parts, and the influences of one upon another, through the principle of sympathy, we may conclude that he was far before many of the moderns in sound physiology. He has, also, our names of diseases. He distinguishes accurately between affections of the brain and its membranes, and describes the vessels of the head. He knew that the blood traversed the body in a circle, notwithstanding he assigned to the liver the origin of the veins, as he did the arteries to the heart. He confesses his ignorance of the beginning of the circle, but never loses sight of the fact.

"Venæ per corpus diffusæ, ab una multæ germinantes, atque hæc una, unde oriatur, et ubi desinat, non scio. Circulo facto principium non invenitur. Cæterum rami ac germina ipsius, unde pendeant, et quæ parte corporis desinant, et quod una his consentit, et in quibus locis corporis extentæ sunt, *ego declarabo*," &c. (3) (P. 283, *note*.)

He has left us a rich and exact record, which assures us that living organized beings were liable to precisely the same diseases, presented the same morbid phenomena, the same secretions, the same lesions of structure, and that they were healed by the same remedies, more than two thousand years ago, as at the present day. It assures us, therefore, that through all this lapse of time, the human organism has been governed by the same laws, that they have an immutable foundation, and are as wide from the laws which govern the inor-

(1) Preface to Sydenham's Works.
(2) Zimmermann on Experience, &c. vol. i. p. 164.
(3) L. de Ossium Natura, v. 174.

ganic world as the most opposite principles. (Vol. I. pp. 57, 593.) Whoever denies the foregoing statement, must be either unacquainted with the writings of Hippocrates, or unjust to his merits.

But his far reaching mind considered it safer to repose upon the living phenomena of nature, than to subject her to the abuses which were apparently known to his prophetic ken. Indeed, he "censures such as spend more time in studying its more various and less useful parts, than in practical observations, and consequently in studying nature." (1) With what reason he argued, the monument he has left must testify.

Had Hippocrates always toiled at facts, as he tells us was at first his disposition, — had he not perceived their connection, and deduced principles which conducted him to more enlarged and rapid observation, how little would he have discovered, in what obscurity would he have lived, in what oblivion would he have rested! What might have been the fate of medicine and of humanity till the age of Bacon! And here we are reminded that this genius of his race worshipped at the shrine of Hippocrates; and whilst he reprobates all departures from the Hippocratic philosophy, he embarks upon a long and adventurous voyage in which he lays down, as great landmarks in physiology and disease, mirages for islands, and plants upon them the standard of superstition. The speculations of Hippocrates, on the other hand, were incomparably less than distinguish many of the most able medical philosophers of the existing generation.

Sydenham, that other Hippocrates, began the practice without having read a book upon medical science. He justly enforces the importance of observation; but will it be believed that it was by the aid of solitary experience, and this alone, that he mounted to the highest niche in the modern temple of Æsculapius? And that great marvel of recent times, John Hunter, who is declared by some of his countrymen to have been deficient in knowledge, is more justly said by others, —

"To stand alone in our profession. When we think of all that he accomplished, we are almost tempted to regard him as supernatural. His life resembles the transit of a comet, which bewilders while it excites admiration; which dazzles with excess of brilliancy, and then forever disappears. In his immense career, every thing bore reference to one great idea, — *the discovery and elucidation of nature's laws.* How humble do any of the men of the present day appear when placed by the side of Hunter;" (2) "who," says another, "was neither anatomist, physiologist, surgeon, nor naturalist, alone; but, the most remarkable combination of all these which the world has yet seen;" (3) for, "where," says another, "in the calendar of time, shall we look for an equal in the compass, the variety, and the depth of his researches into the mysteries of animal life, or for consequences such as those that have resulted from his labours to universal pathology." (4)

These are rare exceptions in the wide expanse of mind, — "appearing only once during the existence of a world." "*Naturæ, non hominis, voce loquuntur.*" They employed the laws of nature in rearing the great fabric, just as the me-

(1) Sydenham, s. 23.
(2) Morgan's Principles of Surgery, *Introduction.*
(3) Brit. and Foreign Med. Rev. No. 11, 1838, p. 191.
(4) Travers' Hunterian Oration, 1838.

chanic would now avail himself of fundamental principles to accomplish in a day what was once the weary toil of years. But they show us how unjust the derision of "genius" by Louis and certain followers, and that it may grasp at the laws of nature, and fundamental principles, whilst others are seeking for "rigorous facts" in the debris of the body.

Finally, seeing that medicine is especially indebted for the advancement it has made, from time to time, to the culture of principles, and that it has owed much of its retarded progress to an exuberance of "facts," we cannot avoid the conclusion, that "the self-called exclusively practical men are the most arrant of pretenders." (1) They are clamorous for novelty; but, do they seek for it in the medical journals of the day? Let their editors answer. Do they look for it in other monuments of mind and industry? The shelves of the booksellers answer — no.

(1) Beddoes on Fever, &c. p. 255.

APPENDIX III. TO THE PHILOSOPHY OF VENOUS CONGESTION.

COLD, AS A CAUSE OF CONGESTION.

(See pp. 45, 47, 235, 245, 277, 316, 334, 496.)

A low temperature operates in three ways. 1st. When universally applied, it acts as a *depressant;* or 2d. as a *stimulant;* or 3d. when applied to a small part of the body, it seems to be an *irritant.*"

"If a low or variable temperature occurs, it very frequently leads to the production of that state which I call common congestive fever. If water be applied in the same way, it leads to a similar condition, especially if the whole surface be exposed to it, and if the body be weakened. Congestive fever occasionally arises in this way from bathing."— ARMSTRONG. *Lectures on Acute and Chronic Diseases, vol. i. pp.* 148, 152, 153.

"COLD," says that shrewd observer, Dr. Parry, "by determining the afflux of blood to one part, may increase it to another; but, *that fact* being admitted, the *question* still remains, *what is it that determines it to that other part in preference to the rest.*" (1)

This "question" it is our present purpose to answer as well as we may. It will, however, doubtless appear singular, that we should attempt to show that intense degrees of cold may excite inflammation in the internal viscera during its progressive operation, and that in conformity with our doctrine as explained in Sections 10th and 15th, the process should be an active, not a passive one. There is something, indeed, so remarkable in the phenomenon, and so apparently adverse to our whole theory, that we have thought it necessary to devote an appendix to its consideration. Nature operates by universal, not by partial laws; and if venous congestion of the brain which is consequent upon frost be owing to some other cause than inflammation of the venous tissue, or some analogous process, then it is highly probable, in a general sense, that that cause operates in other cases. On the contrary, however, if it be rendered probable that sub-active inflammation is the essential pathology, it should bring no little confirmation to our doctrine, and forcibly illustrate the analogy of nature. (See APPENDIX on ANALOGY.) But, it must be constantly borne in mind that we are now speaking of man, and not of hybernating or other animals. (P. 24—25, *note;* and Vol. I. pp. 650—651, *note*, 698.) Organization and constitution must not be neglected.

(1) Elements of Pathology & Therapeutics, vol. i. s. clviiii.

It is fully admitted that active phlebitis is a common affection, and that the enlargement of the veins depends upon the inflammation; whilst we have shown that the divergent veins, and such as are remote from the focus of disease, though continuous with it, are often greatly enlarged from the same pathological condition, though but feebly pronounced. We have also proved that the enlargement of the veins, in varix, and hypertrophy of the veins, depends upon inflammatory action; and we have shown that dissection proves the same condition of disease in the true venous congestions of the brain, the liver, and various other parts. To this proof we have also added the paramount evidence afforded by the vital signs and the effects of treatment. We have also demonstrated the independence of active phlebitis, and varix, and venous congestions, of any obstruction to the circulation; and, what is important to our present inquiry we have proved that accumulations of blood never take place within the head in consequence of any remora that has been supposed to set back from central determinations of blood. We have thus not only cut off any mechanical rationale of the cerebral congestions which are produced by frost, but have brought the pathology, by various, and the soundest analogies, within the precincts of inflammation. Nor can it be alleged that the results are too rapid, or the general powers of life too much impaired for the pathology which we assign, since we see this condition springing up with astonishing rapidity under the influence of many poisons, and when, also, the general powers of life are as much impaired as in the early stages of that exposure to cold which is to end the life of the subject. The same is not less true of inflammations. And here, too, it is important to remark, that the vital signs evince the beginning of cerebral congestion before even the extremities are frozen. The stupor, &c., incident to this period, are familiar to all. Foderé relates the history of two cases of "apoplexy from cold," which happened in February, 1808. "Alius robustus, apoplecticus cecidit, facie rubida, et obiit. Alter, debilis, asphyxiatus, sicut pisces quos captabat, ad littora stagni cecidit, et ad pagam delatus, etiam interiet." (1)

Here, the cerebral congestion being established, went on to a fatal termination in apoplexy after the subjects had been rescued.

But, this is a subject on which we must rely mainly upon analogies, and morbid anatomy. The indications of the latter will not, *of course*, be doubted.

"The immediate cause of death by cold," says Sir H. Halford, "is apoplexy. The heart is arrested and paralyzed in the exercise of its office, and no longer supplies the brain with arterial blood. Nor is the blood thrown with sufficient force to the extremities. It circulates, therefore, in the larger vessels proceeding immediately from the main spring, and there is *no ingress* for the blood returning from the brain. The large sinuses, therefore, become overloaded, and apoplexy follows." (2)

This doctrine is probably adopted from Portal. After describing a case to which we shall again recur, Portal says, —

"We see by this example that apoplexy produced by extreme cold is sanguineous, or that it is the effect of blood carried and retained in too great quantities in the brain. The exterior veins of the body being contracted by the

(1) Disquisit. de Apoplexia, p. 59. (2) London Med. Gaz. March 11, 1837, p. 904.

cold, the blood cannot return freely to the heart, and the arteries, for the same reason, cannot transmit the blood into the veins." (1)

But Portal evidently saw that this hypothesis would not explain the reason why the brain should be alone the seat of congestion in some of the cases; and, he therefore surmises that "the coats of the cerebral veins are less sustained than in other parts." This, however, is neither physiology, nor will it account for the hepatic congestion in the first of the cases which we shall recite.

We would here premise that we are not employed in considering the proximate cause of death from cold, but simply the cause of the venous turgescence of the brain. The fatal impression is doubtless going on simultaneously in all the organs, not only from the direct effect of the remote cause, but especially so from the specific operation of that cause upon the venous system of the brain and the whole cerebral substance, and the influences that are propagated, in consequence, from the brain to all other parts. (See *Bloodletting*, Sec. 3.) This will appear from our dissections.

Sir H. Halford's theory is founded upon "the three cases" (2) of death from exposure to cold, in which Dr. Kellie ascertained the existence of great cerebral congestion. We shall therefore take these cases to demonstrate our own theory.

Perhaps we have sufficiently shown in our Section 2d, that the phenomenon could not have arisen from the imputed difficulty of a descent of the blood from the brain. The hypothesis is, that the central accumulation of blood was such as to constitute an obstruction to the return of blood from the head. The heart, lungs, liver, and other neighbouring organs should, therefore, have been overloaded with blood. But what were the facts? Dr. Kellie describes with minuteness the state of the brain and abdominal viscera, and leaves the impression that the heart and lungs were perfectly natural; and with the exception of the congested liver and small intestines in the first case, all other organs were in their natural state. "The spleen was rather *empty* and *flaccid*, and the stomach *pale*." In the second case, the liver, spleen, and all the internal viscera were natural, except the small intestines. (3) In both cases, however, "there were a few congested spots of a florid purple colour on the mucous coat of the stomach." "In the first case, they had an *ecchymosed* appearance."

The remaining facts were, also, nearly alike in both cases. We shall *mark* what we regard in connection with the foregoing facts, as especially indicative of common inflammation, and inflammation of the venous tissue, to save the necessity of comment.

"The body, generally, might be described as presenting the appearance of more than usual freshness and soundness."

In the first case, "*very little* blood flowed from the integuments of the head. The dura mater was studded with numerous bleeding points; the *whole membrane* was somewhat congested, suffused, and *heightened in its colour;* and its *sinuses* were *loaded* with dark blood. (Pp. 256, 430.) The *veins* of the pia mater

(1) Les observations sur l'Apoplexie, p. 235, 1811. (2) Only two of the subjects were examined.

(3) For this reason, Haller was wrong. He would make an universal venous plethora within, upon another ground. "In animalibus frigidis venæ perfecte immobiles sunt, et contractili vi omnino destituuntur." (*a*)

(*a*) Element. Physiol. t. 1, l. 2, s. 2, § 7.

were very *turgid* and *extensively* injected, so that the whole membrane had a *more than usual vascular appearance*, and somewhat heightened in colour. Following the convolutions of the brain, there was a milky or *œdematous* appearance, arising from the *effusion of serum* between the arachnoid and pia mater. The *cerebrum itself*, in texture and colour, seemed *perfectly sound*. The choroid plexus presented *no appearance of turgescence*. In the ventricles of the brain, and at the basis cranii, were found between *three* and *four* ounces of *serous fluid*."

"In the abdomen, our attention was arrested by the *very dark colouring* of the *small intestines*. The ilium, in particular, through its whole extent, was *very* RED, and presented a very beautiful example of *vascular injection*. The *stomach* and *colon* had *nothing of this congestion*, or *colouring*; and the CONTRAST between the appearances of these and the small intestines was *very striking*. The stomach, indeed, was *remarkably pale*. On its inner membrane were a *few spots* of a *coffee-ground* colour, and *ecchymosed* appearance. The *liver* was *congested* with blood; the *spleen* rather *empty* and *flaccid*. There was *no deviation* from the *usual* appearances in the other abdominal viscera."

In the second case, "the complexion was more *sanguine* than that of the man. *Very little* blood flowed from the integuments of the head, when divided. The dura mater was not so highly coloured as in the man. Its *veins*, however, *were injected*, and *its sinuses were loaded with blood*. The *pia mater*, and the *veins between* the convolutions of the brain, were fully injected, and *very turgid*. About *three ounces of serous fluid* were found in the ventricles of the brain, and at the basis cranii." (Pp. 197, 557, *note*, &c.; and Vol. I. pp. 180, 581—608.)

"On raising the omentum, it was a very striking coincidence to observe, that here, also, the small intestine exhibited *precisely the same appearance as in the man;* the same *redness, not in patches*, but over *the whole* extent of the bowel, and occasioned by the same *general* and *minute injection* of the vessels, *profusely ramified* beneath the peritoneal coat. The *stomach* and *colon*, too, were in this case of the *usual pale colour*, having *no vestige* of the same vascular appearance. There were on the mucous membrane of the stomach a few *congested spots* of a florid *purple* colour. The pancreas was of an unusually dark flesh colour. The *liver*, *spleen*, and *other viscera* presented *no* uncommon appearance."

It is now important to observe that the bodies were *not frozen*, though we do not apprehend that the morbid appearances would have been otherwise had the degree of cold been sufficiently intense for such a result. "A register thermometer, at Hermitage Hill, had not sunk lower than 34° Fh. The neighbouring hills were next morning seen whitened with snow; but no snow remained on our own roads or lower grounds. This was not, therefore, a temperature capable of producing a frost-bite. But the furious gale which blew, and the sleet and snow which fell, would greatly increase the benumbing influence of even this degree of cold, and impede the exertions of those who were exposed to it. If the struggle were once given up, &c. we cannot wonder that they slept to wake no more." (1) But, a parallel case in the sequel.

(1) Trans. of Medico-Chirurg. Socy. Edin. 1824, p. 84.

Those who have attended to the important facts which we have marked in the foregoing cases, will have seen that every part of a mechanical hypothesis is inapplicable; whilst the doctrine which imputes to the circulation of the brain a peculiar law, independently of the manifest objections as to its violation of the analogy of nature, &c. (Vol. I. pp. 161, 703,) will not account for the inflammation of the small intestines in both cases, nor for the hepatic congestion in the first, besides other peculiarities. (1) The existence of that hepatic congestion in one case, and its absence in the other; the natural condition of the thoracic viscera; the pallor of the stomachs; the "empty" and "flaccid" state of the spleen; each one, *per se*, is contradictory of the supposition that the blood could not descend from the head, and of the doctrine of remora. We must, therefore, as in all our former interpretations of the phenomena of life, have recourse to vital agencies; and the more we analyze these apparent anomalies in morbid processes, the greater is the reason for rejecting all physical explanations. And, just so has it been in all our inquiries into the natural phenomena of organized beings. We have everywhere found that no one can be interpreted by any of the known operations of chemical or physical powers, and have even seen that the very originators and defenders of the physical doctrines have come at last to our own conclusion. (P. 114 — 121, &c.)

We have made some references to other parts of our work, in the detail of the foregoing cases. The reader will see the intended applications, and form his own conclusions. We shall only add, in respect to the morbid anatomy, that inflammation was clearly excited in *different tissues;* and its palpable existence in the small intestine in both cases is strongly indicative, in connection with our various proof of the inflammatory nature of venous congestion, that such is truly the pathological cause of the latter affection. We may say, too, independently of the anatomical demonstration, that the *isolated* and universal nature of the intestinal affection is a farther proof of its vital and inflammatory character; whilst the *pallid* state of the stomach and large intestine, in their relation to the small intestine, corresponds with what is known of the natural irritability of these several parts. Being strongly pronounced in the small intestine, the force of sympathetic influences fell upon this part. The "ecchymosed spots" upon the mucous coat of the stomach may appear to be a small circumstance; but, they are a key to the whole pathology. *Patches* of venous congestion were developed in those places, and the causes, remote and proximate, which produced them there, produced, also, the congested state of the brain, and the analogous condition of the small intestine. It is with disease, as with every product of organized beings. Give but the fragment of a sign, and it may open the door to a magnificent display of nature, where we shall see her always in correspondence with the trace she has left behind. (Pp. 67, 267, 300.) The eye of the patient may sometimes as clearly announce the pathology of extensive disease, as the rib of an unknown animal may enable a Cuvier to fabricate the entire skeleton. And so with the subject before us. A solitary case like

(1) Dr. Kellie also thinks that "the force of Cullen's objection should be admitted, that the observed congestion and effusion are the effects of the *retarded return* of the blood from the head, the *consequence* of a general *immobility* of the *nervous power* induced by the sedative action of cold." It appears to us, before we proceed to consider the intelligibility of this rationale, it ought to be determined whether or no the "nervous influence and galvanism are identical."

the foregoing being established in the pathology which we have endeavoured to assign, should be an index to all other cases of *true* venous congestion. It is well observed by Dr. Kellie, that, —

"If, on the dissection of a patient, who had died of a disease characterized by all the ordinary symptoms of any of the comata, the physician were to discover such appearances as were found in these cases, he would be satisfied, congratulate himself perhaps on the accuracy of his diagnosis, and admire the correspondence between the symptoms of functional derangement, and the lesions discovered in the organs of those functions. In various head cases, I have certainly seen a less satisfactory concordance between the symptoms and the organic changes discovered on dissection." (1) We would apply this remark as well to the state of the small intestine, as to that of the head. Such, even, is our respect for morbid anatomy, in a strictly local sense, so only it were permitted to take its natural direction.

Besides what we have said of hemorrhagic effusions, (Sec. 16, and Vol. I. p. 371 — 384,) the "ecchymosed spots" will derive farther illustration from the following case by M. Portal: which is important, also, in its various bearing upon the foregoing examples.

"A lady, aged 32 years, had been accustomed to bathe in cold water, and to remain for a long time in the bath. Being liable to acute pain in various parts of the body, accompanied by spasms like the clavus hystericus, &c. and failing of relief, she was advised to apply ice to the head, and even to add it to the bath, which she did at first without inconvenience. She then increased the quantities of ice as well upon the head as in the bath, when she sank into a most profound sleep, and her respiration became stertorous. When taken from the bath, she was without feeling or motion, being, indeed, quite dead.

"A large quantity of blood was found between the membranes of the brain and in the ventricles. The substance of the brain, the cerebellum, and the spinal marrow, were sound." In his "remarks," he says "the brain was congested as in other cases of apoplexy from cold." (2)

Probably reasoning from analogy in these cases, Portal recommends bloodletting; the philosophy of which is as sound, after stimulation, however unsuccessful, as its application in the cold stage of fever, or in other congestions of the veins. (Vol. I. pp. 202, 204 — 209, 222, 227, 229, 342, 345.)

It seems not improbable, from what we have said of the antecedent history of the foregoing case, congestion had existed in the brain before the last application of the ice-bath, and that the long continued use of the final bath had only aggravated a disease already formed. This explanation, however, is not necessary; though we offer its practical bearing upon cases where congestions may be supposed to exist, and the propriety of avoiding cold applications to the entire body under such circumstances. We have always found them pernicious; though when made over the affected part, in the highest degree beneficial. We present these facts as also illustrating farther the pathology of the disease. (Vol. I. p. 512.)

In all the examples which we have been able to find, venous congestion of the brain was the predominant symptom, attended either with effusions of blood or of serum. (3) Its operation, in *continued* and *intense degrees* like the narcotic

(1) Ibid. p. 94.

(2) Observations sur l'Apoplexie, p. 234.

(3) For our interpretation of the proximate cause of these effusions, we refer to our 16th. Section, and to our first volume according to the references embraced in that section. Even Dr. Kellie, hav-

poisons, and all those causes that act specifically on the nervous system, is clearly most strongly pronounced upon the brain, (pp. 18 — 23, 28, 478 — 493,) and like them, its sensible results are especially manifested in the venous tissue. It was so in the cases by Rossen, (1) and Quelmalz. (2) Larrey, also, informs us that there was an immense accumulation of blood in the heads of those who perished by cold during the Russian campaign; and that "the greater part of those who died in the retreat were attacked with insensibility, (étourdissemens,) and vertigo; after which somnolency took place, followed by profound coma, and finally death." (3)

Portal, in his "remarks" upon the case which we have quoted from him, observes, —

"We may add to those causes which occasion congestion of blood in the brain of such as perish by apoplexy from cold, that one of the effects which cold produces upon persons exposed to it during winter is, to cause first a stupor, to which succeeds a profound sleep, and finally death. The examination of such bodies has shown congestion of the blood in the *vessels* and *cavities* of the *body*, and *particularly* in the vessels of the *brain*, and even in its ventricles."

Kellie says "the effect of the low temperature on the nervous system in producing a torpid and lethargic state in certain circumstances of exposure to a cold atmosphere is familiarly known by many recorded circumstances. This state seems in its symptoms and progress to bear a *striking resemblance to other diseases of the order comata.* This affinity was remarked by Galen, and has been admitted by succeeding writers." (4)

Affecting specifically, like the narcotic poisons, alcohol, electricity, carbonic acid, &c. the nervous system, we should look, as in those instances for various results, according to the intensity of cold, and the duration of its application. That the subject may be fully before the reader, in all its aspects, we shall venture to prolong this appendix by certain views of its operation which experience has suggested. Mr. Hunter remarks that, —

"When cold is applied either within the powers of a part, to excite heat, or only for so short a time as to give the stimulus of cold, then a reaction takes place and warmth is the consequence. But, if cold is applied beyond the powers of resistance, then a contraction of the vessels takes place, and that contraction is in some degree permanent; but this must be done with caution, for, if continued too long, it will produce debility, and action will be excited which

ing the mechanical hypothesis in view as to the philosophy of the foregoing cases, remarks,

"Where three or four ounces of fluid are found extravasated, we can hardly doubt that effusion was *the effect of some modification of the circulation*, and the *cause* of some of the phenomena which terminated in death."

(1) Compend. Anat. p. 147.

(2) In Haller's Disputationes Med. t. vi.

We shall quote this case, the subject of which was a septuagenerian, — "cujus in itinere, mense Januarii, 1726, constitui frigore vehementi extincti oppressique, cadaver theatrum anatomicum delatum, blandoque calore, ob rigiditatem ejus fotum, præter alia in cunctis vasis sanguifluis, tam arteriosis, quam venosis paulo capacioribus polyposas concretiones longas, teretes, simulque *vasa meningum sanguine turgida, lymphamque viscidam in ventriculis*, præbebat. Illorum igitur plerosque, quotquot gelu excessive intereunt, *ex apoplexia*, perrupto *vel sanguine, vel sero accumulato* in cerebri ventriculis succumbere, vero videtur simillimum. *Ipse sopor*, vel somnus, in quem adeo proclives sunt, ante mortem, *serumque largius cerebri in ventriculis* post mortem repertum, non obscurum ejus rei præbet testimonium."

(3) Campagnes, &c. t. iv. 127. (4) Op cit. p. 93.

will be irritable." (1) And, "the more intense the cold," says Girtanner, "the greater is the accumulation of irritability." (2)

Dr. Hall drew the conclusion from Edwards' experiments that the "effect of exposure to cold, when the temperature of the animal *falls*, appears to be, in a general point of view within certain limits, diminished respiration and *augmented irritability*." (3)

"Cold," says Senac, "ought to be considered an irritating cause. In those parts long exposed to its action a frequent stimulus remains, and the small vessels being contracted in their diameters, the fluids are accumulated in them. In this state of things, the impression of the cold either alone, or assisted in its action by the irritation of the nerves, rouses the vital energy of the part." (4) "This is a point as susceptible of clear and satisfactory proof, as any one in the science of physiology." (5)

Cold, like other agents which affect especially the nervous system, is apt, in moderate degrees, to produce inflammation of the mucous and serous tissues; in greater, to affect the venous.

And, "although it may not always be possible to determine why the impression of cold, in the *most common* cases, produces in one a pleurisy, in another peritonitis, in a third arachnitis, in a fourth, a fifth, and a sixth, inflammation of the mucous membranes of the parenchymata, it is no less certain that these different phlegmasiæ are occasioned by cold." (6) (Vol. I. p. 512.) Numerous experiments, indeed, have been made upon animals, especially in Paris, which show that inflammation is readily produced by the direct operation of cold. "It is a singular fact," says Mr. Palmer, "that excessive cold should produce effects very similar to those produced by excessive heat." (7)

Much undoubtedly depends upon the existing susceptibility of different parts, and something perhaps upon the precise part of the surface upon which the cold may make its strongest impression. Its primary action is undoubtedly upon the skin. (Vol. I. p. 568 — 575.) "Who," says Bichat, "does not know that oftentimes, when the *extremities* are *frozen*, the patient feels an *extraordinary internal heat*?" (8) But, taking its varied results as they spring from moderate and extreme degrees, and from sudden, gradual, short, or long continued application, we have an exemplification of the principle which is involved in the variously modifying influences of different kinds of remote causes. If the reader have any difficulty in comprehending this principle, let him turn to our 12th Section, or ample illustrations may be found in our first volume, or the whole may be seen at a glance, in a brief *note* in that volume at page 610; which, indeed, is a key to the whole philosophy of the operation of remote causes.

We may now understand how far Dr. Good was correct in saying that "cold, when it reaches the proportion at which reaction ceases to ensue, is one of the most powerful sedatives in animal *chemistry*. It effectually exhausts the system of all its *irritable* and *sensible* power, and thus occasions a degree of torpor or sleep, which the will, in many instances, is incapable of resisting." (9)

The influences are of a very complex nature. *Sensibility* is certainly every-

(1) On the Blood and Inflammation, p. 349, *note*. (2) On the Laws of Irritability.
(3) See *note* to Dr. Edwards on the Physical Agents on Life, p. 464.
(4) On Fevers, p. 39, tr. (5) Ibid. Caldwell's *note*.
(6) Brousais' Pathology, p. 302. (7) Hunter's Lecture's on Surgery, p. 73, *note*.
(8) General Anatomy, &c. vol. ii. p. 49. (9) Study of Medicine, vol. iv. p. 419.

where greatly depressed. This property, however, is in no respect necessary to inflammation; but, as we have shown, the division of nerves may prove an immediate exciting cause of that mode of disease. (P. 146—147, &c.) The *irritability* of the surface is also greatly reduced; but, does it thence follow that so also is that of the great internal viscera? It would be an assumption in direct collision with facts. There is, however, something more than a mere diminution of sensibility and irritability in the skin. There is a great and sudden alteration of all the properties and functions of that organ; and, when we consider the vast surface affected, and the powerful sympathies which prevail naturally, and as exemplified in numerous diseases, between the skin and all the internal viscera, we shall scarcely be surprised to find that venous congestions and inflammations spring up in the latter organs when so great a violence is inflicted upon the former. Why the morbid action is determined with great uniformity upon particular organs may be readily understood by referring to the action of various specific agents, the narcotic poisons for instance, (p. 478—493) and to principles which we have considered at large in our first volume, and in a general sense, in that volume at page 568—575. Nor will we neglect saying, that the example now under consideration, illustrates what has been stated at the preceding pages.

Hippocrates, Aretæus, Aetius, and succeeding observers, have considered the winter season as predisposing to apoplexies, especially the transition period from autumn to winter, and from winter to spring. Various authors have continued to confirm this fact; and amongst the latest is M. Andral, in his valuable clinic on apoplectic affections. "The sudden change," he says, "from one extreme of temperature to another ought to be considered as one of the principal causes of cerebral congestions," (1) meaning *venous* congestions. In this instance, doubtless, there are concurring atmospheric causes. But, at other times, the prevalence of apoplexy appears to depend upon intense degrees of cold. Thus, it is stated by Heberden that the cases which occurred in the rigorous winter of 1795 were to those of the mild winter following as 52 to 31. (2) Similar examples will be stated. But we will say here, that we cannot better show the principle upon which cold operates in these cases, than by comparing its effect with excessive heat, which also leads to the same result. Thus, Rush says, in 11 years, the cases of apoplexy in Philadelphia during the month of July, compared with those which occurred in the other months, were as 109 to 59. "*Frigora enim ex caloribus, et calor ex frigore.*" (3) Again:—

"A table," says M. Andral, "has been made, for a period of 20 years, showing that in Holland, cerebral congestions have abounded most in winter, next in autumn, then in spring, and lastly in summer.

"In Milan, for a period of 25 years, cerebral congestions followed, in frequency, the order of the seasons, winter, spring, summer, and autumn.

"At Paris, the statistical researches of M. Talret show that cerebral congestions and hemorrhages are more frequent in winter than in summer or spring."

A table is subjoined by M. Andral, in which 20 cases are put for January, 19 August, 17 April, 15 February, 15 December, 9 September, 7 March, 7 May,

(1) Clin. Méd. t. v, p. 257.
(2) Statistics of Apoplexy are more entitled to confidence than of any other disease.
(3) Hippocrates, de Humoribus, v. 136.

5 November, 4 June, 3 October, 3 July, = 104. Of 20 other cases, 13 took place in January, February and March, 2 in September, and 1 in each of the months of May, June, October, November, December. Our author well says, — "cerebral congestions are produced by both extremes of temperature, and that they are reduced to their minimum of frequency (minimum de fréquece) during a mild and uniform temperature." (1)

We take the following from the three last reports of interments in the city of New-York; and this we do the more so on account of their acknowledged accuracy. There were in

1837	Cases.	1838	Cases.	1839	Cases.
January,	14	"	13	"	14
February,	9	"	8	"	7
March,	16	"	13	"	9
April,	13	"	3	"	13
May,	11	"	6	"	10
June,	11	"	6	"	6
July,	7	"	70	"	9
August,	7	"	8	"	5
September,	8	"	6	"	6
October,	3	"	11	"	14
November,	15	"	13	"	16
December,	15	"	7	"	7
	129		164		116

The months were generally cold for the season when the greatest number of deaths happened, with the exception of July 1838, which was an excessively hot month. (2)

Ferrario supplies the following statistics of 10,432 cases of apoplexy which happened from 1774 to 1834, at Milan. There were in

January,	1176	May,	829
December,	1075	October,	822
February,	1030	September,	718
November,	963	July,	689
March,	956	June,	684
April,	848	August,	645

It results from the foregoing tables that cold is a more powerful predisposing cause of cerebral congestion than any other morbific agent, much exceeding that of heat, or the miasmata which are the accompaniments of heat. (3) This, our author affirms of the climate of Milan.

"Il *freddo* più che il caldo determina tra noi le apoplessie; vedi che nell' anno

(1) Clinique Méd. t. v. p. 255—257.

(2) The proportion of deaths from apoplexy in the city of New-York, as compared with other diseases, from 1805 to 1836, was 1 in 58. 11.

(3) To illustrate what we have said of age in predisposing to the cerebral congestions which result in apoplexy, we shall subjoin the following table. (Pp. 280, 262 — 263, and vol. i. p. 383.

Of the 10,432 cases, 2839 happened between the ages of 61 and 70 years; 2,163 from 51 to 60; 2,026 from 71 to 80; 1,363 from 41 to 50; 778 from 31 to 40; 464 from 81 to 90; 408 from 21 to 30; 224 from 11 to 20; 145 from birth to 10; 20 from 91 to 100; 2 from 101 to 103. Of the 11,731 apoplectics, from 1750 to 1834, there were males 6492, females 5,239.

1767, nel cui gennajo il termometro segnò — 12,0 e piu, è cresciuto il numero delle apoplessie, forse per tanto freddo, in confronto dei cinque anni antecedenti e dei quattro seguenti."

But, he adds, that "*extreme heat*, as well as cold, produces the phenomena (fenomeni) of apoplexy, and sometimes heat appears to be an active agent. Thus, in 1743, more than 5,400 persons died suddenly at Pekin from the heat of the weather, which was considered extraordinary, although the thermometer of Réaumur did not rise above 34°."

Having quoted the philosophy of others as to the modus operandi of cold in producing venous congestions of the brain, we shall now state that of our able Italian author, both as to heat and cold : —

"When the heat of the summer was very great, there arose an *expansibility* of the fluids, the *plasticity* (plasticita) of the blood became *diminished*, and the organism weakened. Hence ensued a disease of *weakness*, (asteniche) &c.

"When the opposite cause, or the rigour of winter prevails, worse effects ensue. Sensibility is diminished, and the perspiration arrested, whereby the *plasticity* of the blood and its stimulating properties are *increased*, the muscular fibre acquires greater tonicity, the action of the heart becomes stronger, and the direct functions are affected with greater energy ; the ordinary consequences of which are venous congestions, and inflammations, and what is still worse, the weakening of those vessels (e spessissimo lo sfiancarsi di quei vasi) from which proceeds the fatal hemorrhage." (1)

Richond observes, "que les temps froids et humides sont propres à déterminer l'apoplexie." (2) He attributes much to a pernicious influence of cold upon the abdominal viscera ; in which he is undoubtedly right. See, also, Foderé, and authors quoted by him." (3)

But, it is by no means cerebral congestions, and common inflammations, that are alone determined by cold. We have seen that crural phlebitis has been known by Ramsbotham, Johnson, Graves, Stokes, and others, to have been produced by "the local exposure of the limb to cold." Abdominal venous congestions are a frequent consequence in climates where fevers of an intermitting type occur. In these cases it either operates in connection with other atmospheric causes, or is more apt to develope an explosion in the fall or winter, when the predisposition has been begun or formed by the other causes at a warmer season. Laennec, Davies, and others, state that "temperature has evidently an effect in producing pulmonary apoplexy ; thus, the sudden and *long continued* impression of cold acts as a cause." (4)

Cold is one of the most universal causes of scurvy ; (p. 475; and Vol. I. p. 670,) a disease in which venous congestion has an important agency. Cold contributes to the development of typhus fever, although there is reason to think, when it prevails in winter, that the important predisposition was formed

(1) Statistica delle Morti per Apoplessia nella Città, e nel Circondario Esterno di Milano, dall' anno 1750 al 1834, pp. 55—59, 63, &c. (See *ut cit.* p. 547 ; and *Hippocrates*, de Dieta, l. 2, v. 384—535.)

(2) De l'Influence d l'Estomac sur la Production de l'Apoplexie, p. 21.

(3) Disquisit. de Apoplexia, p. 50—60.

(4) Laennec, on Diseases of the Chest, p. 196 ; and Davies, on Diseases of the Lungs and Heart, p. 222.

in the summer or fall. Whether cold or heat will manifest its special agency in the production of congestions, will depend upon various circumstances. Either may, apparently per se, predispose to the disease. The incubation is then short, and the attack soon follows. In these cases, the degree of heat or cold is generally severe, or if less so, the exposure is sudden or long continued. Many common inflammations, however, as colds, are excited by mild rather than severe degrees of cold. And this example, by the way, demonstrates the important agency of cold in developing inflammation. The very name of the disease is derived from its exciting cause. It is generally the result of a moderate change from a mild to a cooler temperature. This disease also exhibits the independent agency of cold, as in its effects where the high temperature of a room is suffered to go gradually down, or in passing suddenly from such an apartment to another where the temperature is lower; and it manifests the cooperation of other and more important causes, in the prevalence of influenza. When heat or cold are aided by the influence of other remote causes, these last diversify the nature of the inflammation, whether of the venous or other tissues, and these are the causes which complicate it with idiopathic fever. When cold or heat operate alone, then we witness the most simple forms of venous congestion. (P. 220, and Section 12.)

The continued operation of cold ultimately occasions insensibility. This is perfectly compatible with the institution of inflammatory action, even after insensibility has taken place. (Pp. 28—35, 146—147, 185—187; and vol. I. pp. 157—160, 474—480, 568—572, 687—688.) The latter arises, mainly from the impression of cold upon the nervous system; torpor, from its operation directly, and through the medium of the nervous influence upon organic irritability. (Vol. I. pp. 474, 568.) This condition will account for the absence of pain in many inflammations and venous congestions. (P. 289.)

It is remarkably shown, in opposite aspects in respect to particular organs, in that highly congestive disease the cholera asphyxia. Sensibility is obtuse in the congested organs, where irritability is strongly pronounced. The converse of this exists in relation to the skin; "the most remarkable condition of which is the impaired state of its *irritability*, whilst its sensibility is not much diminished. Simple irritants, however active, do not produce their ordinary effect. Cantharides, ammonia, &c. are perfectly powerless, and the chemist is consulted for combinations that shall evolve more active principles. These, too, fail in their turn, and we resort to fire and boiling water as the last resource of art. But, the patient will not even always burn,—save in his sensibility, which pleads as piteously as in the fulness of health." (1) And so, Armstrong, in speaking of the congestive stages of typhus fever, observes,—

"On account of the general torpor, the secretions are diminished or suppressed; and, as justly remarked by Dr. R. Jackson, the *skin* is often in that peculiar state, that if blisters be applied, they either do not act at all, or so defectively, as to leave an appearance as if the part had been slightly seared by a hot iron." (2)

From what we have now seen of the effects of cold upon the human species, and its entire failure of similar results upon animals, *cæteris paribus*, as shown

(1) Author's Letters on the Cholera Asphyxia of New-York, of 1832, p. 105.
(2) On Typhus Fever, sec. 3, p. 50.

remarkably in the hybernating animals, and what is more remarkable, in the young of warm-blooded mammalia, we may offer it as another striking illustration of the truth of all that we have said of the fallacy of employing, without a strict reference to constitution, the results of experiments upon one genus to explain the influence of analogous causes upon another, or even different species of the same genus. It has been a fruitful source of error in physiology, and in its practical application to the normal and morbid states of the human race. (Pp. 18, 20, 21, 24—27, 37—41, 73; Vol. I. pp. 101, 108, 196, 509—512, 518, 565, 582—586, 613, 616, 619, 625, 650—651, 659, 696—698.) As a remarkable instance and as applicable to the subject of this appendix, we may advert to the experiment of Spallanzani, where he evacuated all the blood from the heart and large blood-vessels of a number of frogs and toads, and buried them in the snow, along with others which retained their circulation and vivacity. The whole became soon completely torpid, and "appeared as if frozen." In a few hours they were removed to a warm situation, where all of them began to leap and make their escape; the reanimation being apparently as perfect in those which had been deprived of blood as in those which had not. When exposed to greater degrees of cold, they perished in equal times. Now, in the application of this experiment, we shall see one of the sources of that false philosophy which we have endeavoured to exhibit under various aspects in our Essay on the Humoral Pathology, especially in our 8, 9, 10, 11, 12, 13, and 14 sections. The conclusion is drawn, that, "the failure of sense and motion does not arise from the refrigeration of the blood, for it cannot take place where there is none, nor from the *retarded* circulation of this fluid, but depends entirely on the solids." (1) Why, then, was not this reasoning extended as far as the poison of the viper, as stated in our first volume, page 509.

The writings of Hippocrates abound with the soundest philosophy as to the various influences of cold upon the human organism. See De Morb. Pop. l. 6, s. 3, v. 18, s. 8, v. 65—72. De Dieta, &c. l. 2, v. 29—83. De Humid. Usu. v. 17—32, v. 120—126. De Vet. Med. v. 255—278.

We shall have no place more appropriate for saying, that, since our article on Animal Heat was printed, we have received the January No. 1840, of the Medico-Chirurgical Review, containing an analysis of the Articles on "Hibernation," by Dr. Hall, and on "Animal Heat," by Dr. W. F. Edwards, embraced in the late "Parts" of "The Cyclopædia of Anatomy and Physiology." We see nothing, so far, to affect our views upon the question; but much to confirm them. They appear to be very able papers, and to contain many interesting facts. In relation to Brodie's and Chossat's experiments, (p. 23—29,) we have the following remarks from Dr. Edwards, after an approval of Sir B. Brodie's experiments.

"Dr. Chossat completed these researches upon the nervous system in its relations with the production of heat, by demonstrating in a series of experiments the following *very important fact*; viz:

"*That the depression of animal heat is constantly in relation with lesions of the nervous system, whether these lesions implicate the cerebro-spinal system, or the system of the great sympathetic.*" — *P.* 12.

(1) On the Natural History of Animals, vol. i. p. 91, *tr.*

APPENDIX IV. TO THE PHILOSOPHY OF VENOUS CONGESTION.

PATHOLOGY OF ERYSIPELAS.

(See pp. 279, 443, 461.)

We have not fully made up our mind as to the seat of that inflammatory action which constitutes erysipelas; and, therefore, shall only offer in this appendix a few of the prominent facts which appear to sustain the conclusion that the capillary veins are chiefly involved. This we do for the purpose, mainly, of rendering our Essay on Venous Congestion as complete as we may, and as it may derive some light from this investigation.

It is in erysipelas, as we have seen of venous congestions, (p. 221:)—

"A mystery," says Mr. Travers, "has hung over erysipelas to this day, which has rendered it a sort of enigma. Some have called erysipelas a salutary effort of the constitution to eliminate morbid matter, or depurate the blood of vitiated secretions, which it has absorbed, (Vol. I. p. 609;) others have regarded it as peculiar to broken down habits, and its fever depending upon a putrescent state of the fluids, (Ibid. pp. 419, 539;) others again have imagined that its seat explained its peculiarity, and have thought little of remote or constitutional influences, external or internal, and treated it accordingly on the same general principles as other local inflammations; and by one (1) who has seen much of it, it was gravely pronounced to be no inflammation at all, but an erythysm sui generis. If variety of doctrine were not sufficient evidence of its obscurity, I would simply appeal to the experience of the profession for the fact of its capricious, shifting, and *difficultly intelligible character, its danger, the vagueness of the opinion generally prevailing as to its cause, and the diversity of practice adopted for its cure.*" (1)

"The nosologists," says another, "scarcely know where to place it; the systematics cannot tell how to treat it." (2)

All this, in itself, is sufficient to show a well marked distinction between this affection and inflammations of all tissues except the venous; whilst we have the closest analogies between erysipelas and ordinary phlebitis, venous congestions, and congestive fevers, whether in relation to the remote causes, the symptoms, the coincidence of erysipelas with phlebitic and congestive diseases, the

(1) Further Inquiry concerning Constitutional Irritation, p. 137, 1835.
(2) Lon. Med. Chir. Rev. vol. xxi. p. 266.

humoral pathology, and the stimulant plan of treatment. If any thing more were necessary to satisfy us that pathologists have overlooked the probable source of all the obscurities attending erysipelas, and that it is to be found in the constitutional nature of some particular tissue which is invaded by this disease, it is the opinion of the shrewd observer whom we have just quoted, that we must look for a solution of the "enigma" to the nervous system. (P. 488, *Armstrong.*) What there may be in the phenomena of erysipelas which evinces any greater connection of that system with this disease than with any other inflammatory affection, we think our author would not undertake to say; whilst it is well known that erysipelas not only invades paralytic extremities, but may be actually produced by dividing the communicating nerves. (Pp. 28—36, 146, *note;* Vol. I. pp. 157—160, 474—480, 568—575.) We may also add, that a greater "mystery" has hung around the pathology of purpura hemorrhagica, scurvy, beriberi, and other kindred affections; nor has the treatment been less various, corresponding exactly with what different observers have imagined to be the essential nature of the disease. (Pp. 221, 243, 251, 261, 271, 293, *note;* Vol I. pp. 279, 420 *note*, 434, 438, 540, 544, 657, 663, 698, 716.) And, as a farther confirmation of the analogy between erysipelas and inflammatory affections of the veins, it may be said that morbid anatomists are never satisfied with the apparent causes of death. Thus Bricheteau says, of erysipelas,—

"We cannot attribute death to the action of a general inflammation on the nervous system. It probably results from an *excessive exhaustion of the principle of life*, which, having been at strife for eighteen days with pain, is extinguished by *a mechanism* that escapes us." (1) (Pp. 221, 329—331.)

To us it appears highly probable that this "mechanism" is the peculiar constitution of the venous system, not only that part which may form the immediate seat of the affection, but more so those venous congestions of great internal organs which are almost always antecedent to the cutaneous affection, when occurring spontaneously. We have already spoken sufficiently of the depressing nature of venous inflammation upon the powers of life. Such, then, is probably the "mechanism" in *physiological* science.

We would premise, that, if it should result that erysipelas is seated in the capillary veins, it must be constantly recollected, that the veins themselves are not the instruments of action; as indeed, they never are under any circumstances. This belongs to the capillary arteries, which are here, as in ordinary phlebitis, the agents of disease. There is no analogy between the functions of these two systems of capillary vessels.

Nor is it remarkable that the inflammation should be limited to the capillary veins, notwithstanding the disposition to diffuse inflammation in the venous tissue. We have seen, as in operations upon varix, how remarkably that principle is modified by the nature of the remote causes, and how, as in ordinary phlebitis, the inflammation breaks off abruptly at the entrance of a vein into a trunk, or how it fades away in remote parts, or, how it may affect the whole superficial system of veins, without extending to those of the internal organs. (Pp. 331—336, 479, &c.) If also, the capillary venous tissue be the seat of erysipelatous inflammation, it is another illustration of the foregoing principle, whilst

(1) Medical Clinic, p. 9. *Tr. Phila.* 1838.

its very limitation, with the attendant phenomena, is analogically a fact in favour of the supposed affection of the capillary veins.

We have said, however, that it is probable that, in venous congestion, the inflammatory action begins in the capillary series of veins. (P. 518.) But, its subsequent extension to the larger series is no ground of objection, (supposing our conjecture to be true,) to the imputed seat of erysipelas; since, in the former case, the veins appertain to the organic viscera, where there is a modification of the vital forces of all the tissues differing from that of the same forces which belong to parts that are invaded by erysipelas. And this, too, is shown by the exemption of the former parts from erysipelatous inflammation.

"The veins, the venous blood, the sub-cutaneous cellular tissue, and the skin," says M. Ribes, "being the seat of erysipelas, it is not surprising that the tumour should be so little elevated, that it should extend along in an unequal manner, and without being circumscribed; that the colour of the part should be a yellowish red, and a little livid." (1) His demonstration is conclusive, at least as it respects the cases which he investigated. He "found the small veins sensibly and principally affected; the arteries much less so. When the disease terminated in suppuration, which was rare, the walls of the veins were red, their internal tunic, throughout, increased in thickness, and they were filled with pus. When it ended in gangrene, the veins were black, easily torn, and contained *sanie*." (2)

Erysipelas is well known to give rise to the severest forms of phlebitis, and to varix; of which we have stated examples. This would imply a disposition of the inflammation, if it be in the venous tissue, to extend to the larger series of veins. It is also true that undoubted capillary phlebitis of the skin presents the aspect and phenomena of erysipelas. M. Ribes states that he has found in the peritonitis of lying-in women, which he regards as an erysipelatous inflammation, the veins filled with purulent matter. M. Chaussier has also seen the veins of the abdominal viscera, in numerous cases of this sort, filled with pus. And so of many subsequent observers. It is said in the Medico-Chirurgical Review, (3) that the writer had seen cutaneous erysipelas give rise to purulent deposits in the lungs. All this tends to illustrate the predominance of sympathy in the venous tissue, and the pathology of the venous congestions in true puerperal fever. (P. 457—471.)

It is the opinion of that true observer of nature, Mr. Travers, that "a large proportion of the puerperal cases are cases of erysipelas of the peritoneum." (4) And so Wilson, (5) The characteristic local results are analogous to those of erysipelas, and there is the same prostration of strength as in phlebitis. Indeed, the general phenomena are such as have led the best observers to confound this affection with the genuine puerperal fever, or with simple phlebitis, or even with typhus fever; whilst none are so liable to the severe grades of phlebitis as lying-in women.

Although Mr. Travers considers erysipelas a "nervous inflammation," he well remarks, "that the preternatural pulsation of the arterial branches, and

(1) Mém. de la Société Méd. d'Emulation. t. viii. p. 627, 1817. — We think Mr. Travers wrong in stating the colour to be "a bright red." — *Further Inquiry, &c. p.* 121.

(2) Ibid. p. 624; also Revue Méd. 1825, p. 13. (3) Vol. xxix. p. 271.

(4) Further Inquiry concerning Constitutional Irritation, p. 128; 1835.

(5) On the Blood, &c. p. 45.

the sensation of throbbing, felt often in the contiguity of phlegmon, are not observed in erysipelas. It is the *venous side* of the circulation that the *capillary system* inclines, when the balance which it holds between the two is disturbed." It is so evident to this observer, that erysipelas differs remarkably from all other forms of inflammation, that he has the following explanation:

"It is," he says, "an irritation due to a morbid condition previously existing, and in the absence of which it can no more appear than an effect can produce its cause." (1)

This may be very true; but should it appear that erysipelas is a venous inflammation, the supposed "morbid condition previously existing" will resolve itself into the *peculiar constitution of the venous tissue.*

Dr. Graves states, as the result of his observation, that, when erysipelas commences at any one point of the mesial line of the body, it is apt to spread in a symmetrical manner. "The inflammation commenced in a majority of cases about the nose, and then extended in a perfectly symmetrical manner over the forehead and down the neck," &c. (2) This phenomenon must have been observed by all practitioners of experience. It is not peculiar, however, to erysipelas. "The median line," says Bichat, "frequently separates the affections of the right side from those of the left, and the diaphragm seems often to be the boundary of many diseases." (3) The principle has a deep foundation, and we see it strikingly illustrated in the venous tissue itself, as we have pointed out in our 12th and 15th Sec. It is here, indeed, that it is most apparent; and, since it is well known that phlebitis is apt to terminate abruptly at particular points in the venous system, and since the foregoing characteristic is one of the most remarkable attendants of erysipelas, the indisposition of the inflammation to trespass upon the opposite side of the median line seems to favour strongly the doctrine of its venous nature.

And here, from the importance and ambiguity of the subject, we may say a few words more as to the failure of the very diffuse nature of venous inflammation to extend, in erysipelas, from the capillaries to the trunks of the veins. In the first place, the vital properties of the capillary veins exist in a different modification from those of their trunks; and again, they undergo in erysipelas a specific change which does not excite the corresponding sympathies in the larger series that are developed in other modes of capillary venous inflammation. This is known by the various phenomena, and sometimes by the known differences in the remote causes. It is also certain, that when those causes operate primarily upon either system, the inflammation may be most extended in that particular series. In traumatic phlebitis, &c., the inflammation may take an extensive range amongst the large vessels, whilst the capillary veins shall remain intact. It is the same principle operating now, and creating the distinction which we remark in erysipelas. At other times, the capillaries, being the primary seat of disease, as often in uterine phlebitis, and doubtless also in venous congestions, the inflammation is propagated to the venous trunks, but shall be limited by their terminations, &c. Here the vital properties of the capillary veins are doubtless altered in a different way from erysipelas, (p. 478; Vol. I. p. 610, *note*;) or, the predisposing causes may have acted upon both series, though most powerfully upon the capillary.

(1) Ut cit. pp. 132, 134. (2) In Lon. Med. Gazette, Jan. 14, 1837, p. 569.
(3) General Anatomy, &c. vol. i. p. 359.

"Osiander," says Breschet, "has related the case of an infant in whom the ligature upon the umbilical vein was followed by umbilical phlebitis, and an erysipelas that spread over the whole body." This case is preceded by another from Meckel, where the same cause produced extensive suppurative inflammation of the peritoneum. (1) This phenomenon occurred, also, in the other instance, and the whole taken together, with the other facts we shall have stated, appears to refer erysipelas to the capillary veins, and the occasional dependence of peritoneal inflammation upon this condition of disease. Sasse has made observations like the foregoing; and Duplay, in *L'Expérience*, 1838, relates six cases of peritonitis and *jaundice*, which were the consequences of umbilical phlebitis.

Dr. De Leon describes an epidemic erysipelas which prevailed at Kingston, Jamaica, in the spring of 1826. Many of its subjects were affected with cough, which the Doctor thinks was constituted by an erysipelatous inflammation of the pulmonary mucous tissue. "On looking into the throat, the appearances observed were singular and remarkable, so that it was impossible to refer them to any disease described by nosologists. A *dark-coloured, purplish* inflammation commenced from the curtain of the palate, spread over the uvula and tonsils; and the pharynx, as far down as could be seen, was often equally inflamed and purplish. Sometimes *several veins* were seen on the pharynx turgid with dark blood." In severe cases, there were ulcers upon the "puffy and purplish mucous membrane." Suppuration occurred upon the nates of one. In another, the entire abdomen was covered with a dark redness, which disappeared on pressure. This redness extended rapidly down the thighs, forming diffused tumours, "very painful, hard, massy, and dark red, terminating in suppuration." (2)

The remarkable fact is well known, that erysipelas often prevails in connection with endemic or epidemic puerperal fever; a disease, which we have endeavoured to show, is constituted by idiopathic fever, complicated more or less with venous congestions, or venous inflammations. The subjects of the erysipelatous affections are also the subjects of a much more formidable congestion of some of the great internal viscera, especially the liver; upon which, when not complicated with puerperal fever, we believe the phenomena, and the danger of the disease especially to depend. The erysipelatous affection appears to be only a consecutive result: and this is particularly shown where the cutaneous disease is developed by debauches in those who appear to be constitutionally liable to its attacks. And as here, so in the other cases, the most efficient mode of restoring the skin is to remove from the abdominal organs the primary cause. (Vol. I. p. 609—610.) Nay more, although traumatic erysipelas may doubtless be produced independently of any visceral disease, we believe it will be generally found, that when this affection is produced by the bite of leeches, there exists a state of venous congestion in some of the abdominal viscera. Yet, in the natural variety, leeching the inflamed part is the best local remedy, as general bloodletting is the best constitutional one.

The diversities which occur among the symptoms of erysipelas appear to depend essentially upon the nature of the remote causes, the extent and modification of internal congestions, and the general derangement of the organs to which the congested veins appertain.

(1) Sur l'Inflam. des Veines, in Journ. Comp. du Dic. Méd. t. iii. p. 318.
(2) New-York Med. and Phys. Journ. April, 1827.

APPENDIX V. TO THE PHILOSOPHY OF VENOUS CONGESTION.

PATHOLOGY OF TUBERCLE AND SCROFULA.

(Pp. 318, 323, 495, 525.)

"When consumption was regarded as a pure inflammatory complaint at its commencement, which it was when less perfectly understood than at present, physicians were in the habit of recommending and practising repeated small bleedings." — WEATHERHEAD, *on Diseases of the Lungs*, *p.* 43, 1837.

"Tubercle is always the result of inflammation." — GROSS; *Pathological Anatomy*, *Vol. I. pp.* 155, 178, 182. — 1839.

FEW subjects demand the attention of the physiologist more than the pathology of tubercle; and there is none upon which morbid anatomy has so fully proved, as it appears to us, its own incapacities, or inflicted greater evils upon the cause which it is its laudable object to promote. The importance of the inquiry is sufficiently evinced by the havoc of the disease, and by the contradictory practice of eminent physicians as determined by their theoretical views of its nature. "Tubercular disease is the cuase, in all probability, of nearly one third of the deaths that annually happen throughout the world." (1)

In all that we may say upon the present subject, we wish to be understood as referring entirely to the pathology and treatment of tubercle. We do not dwell upon the constitutional predisposition, or the method of protecting strumous constitutions against the invasion of the disease. That is wholly another subject.

We shall not inquire into the philosophy of Baron, (2) Dupuy, (3) Carmichæl, (4) Kuhn, (5) and of other philosophers, which supposes tubercle to possess an "independent vitality, that it is a parasitic animal, vesicular, hollow, or opaque." (Vol. I. p. 517, *note*,)

Nor can we agree in the philosophy of Dr. Carswell, and others, that tuberculous matter is sometimes the result of inflammation, but is again wholly independent of it. This matter possesses a specific character, and requires not only

(1) Gross' Pathological Anatomy, vol. i p. 150; 1839.
(2) Inquiry into Tuberculous Diseases, 1817. (3) Traité de l'Affection Tuberculeuse.
(4) On the Origin and Nature of Tuberculous and Cancerous Diseases, 1836.
(5) Bulletin des Sciences Méd. Aout, 1831; and Revue Méd. 1832.

a cause as uniform as that which produces "laudable pus," (p. 181—207,) but probably as much a specific action as the matter of smallpox, &c. The "*diathesis*" is not alone sufficient. Action must be present. The variety of phenomena will not affect the question. We think, also, that the proof is most various and conclusive against the doctrine that tubercle is an inorganizable substance, as maintained by Carswell, Alison, Sir J. Clark, Louis, Andral, and many other eminent philosophers.

"If," says Mr. Pool, "the accumulated researches of modern observers of morbid anatomy have succeeded *at all* in enlarging our views of disease in general, it must be conceded to them that they have established this fact, that the same lesion of structure may be characterized by the most different phenomena, according to the condition of the subject in which it occurs." (1)

This "concession" is mainly applicable to advanced conditions of disease when the phenomena are influenced by complex sympathies, or by lesions of structure. It shows, therefore, the necessity of referring to the early history of the symptoms. (See p. 235.) As little, also, are we satisfied with the statement, that, —

"Tuberculous matter exists in the circulation *as an extraneous body*, and, that, by a process which has many analogies, it is separated by the secreting vessels, and concretes into what we call tuberculous matter." (2) The doctrine was taught by Morton, (3) and De'Häen. (4)

This, as it appears to us, is a revival of one of the worst features of the humoral pathology, in a philosophical sense; being similar to that which supposes that pus, carcinomatous matter, &c. sometimes form in the torrent of the circulation, and are sometimes the product of vascular action, &c. (Pp. 188—191, 373.) It is a sufficient objection to the hypothesis to say, that it is defective in fundamental principles. Again, why is not this "extraneous" substance sometimes carried off by the great emunctories of the body, and why does its formation always depend on a modified action of particular tissues?

The microscope, too, has been greatly concerned about the non-inflammatory nature of tubercle, which imparts to the doctrine a still greater aspect of speculation. In the hands of M. M. Piorry, Andral, Donné, and some others, it has even advanced the humoral pathology of the disease; it being affirmed, that by the aid of this instrument, "grayish granulations, resembling tubercles, have been discovered in the buffy coat of the blood." (5) (Vol. I. pp. 56, 66, 626, 699, 708.) What, then, may not be suspected of the affirmation that tubercles have been detected in recently deposited lymph?

It is a substance *sui generis*, and requires a peculiar modification of the vital properties for its formation.

We shall continue this investigation, for special reasons, by affirming that it is the result of our researches, that a far greater success has attended the depletive than any other plan of treatment, in all the modifications which scrofula presents, at its early stages, and that, "we ought to be impressed with the belief that

(1) On Tubercular Phthisis, in Dublin Journ. of Med. and Chem. Sciences, vol. vi. p. 238.
(2) Weatherhead, on Diseases of the Lungs, p. 36. (3) Phthisiologia, &c. 1689.
(4) Rat. Medendi, t. i. p. 60. (5) Traité de Diagnostic et de Séméiologie, t. i.

prophylactic means are worth infinitely more than therapeutic;" (1) that, for the latter purpose, the best experience at all former ages is coincident with the latest, as expressed by the able observer whom we quoted last, in his important remarks on the subject of food. Diet, before any other means, is the most indispensable, whilst its neglect is precipitating millions into a premature grave. It is summarily and truly stated by our author, that "where a *well regulated* vegetable diet has been used, it has been with advantage;" allowing, however, the use of milk, which is non-stimulant, whilst other animal food, in a general sense, is prejudicial. There are some constitutions, however, that bear it advantageously. The vegetable diet must be "*well regulated.*" The stomach and intestines are apt to suffer in their powers and functions; and the latter organs, especially, to take on the most serious conditions of disease. The lungs may sometimes sustain temporary relief in consequence; but, they generally become more and more involved in disease in proportion to the frequency and duration of intestinal disturbances. For this reason, it is a matter of primary importance, to maintain the gastric functions unimpaired; and nothing is more essential to this object than food of easy digestion. Thence, also, one of the great advantages of open air; change of climate, and exercise. If diarrhœa supervene, all solid food, even bread and milk itself, should be immediately suspended. The patient should be restricted to rennet-whey, or arrow-root, and if this conversion be made early, the diarrhœa will generally cease spontaneously in less than twenty-four hours. Long, and critical inquiry, has satisfied us that the gastric derangements, the "colliquative diarrhœas," and intestinal ulcerations which supervene on phthisis, are to be ascribed mainly to mal-practice in relation to food, or to a stimulating plan of treatment. As the disease advances towards its more hopeless stages, the patient is apt to be abandoned to his own capricious desires for a gross diet, or for the luxuries of the culinary art; upon the ground either that his animal propensities should be allowed their full gratification for the little remnant of life, or that he is "a better judge of the adaptation of food than his medical adviser." An inexpressible amount of suffering is thus artificially produced, and life is more or less curtailed in most instances. Nor will we neglect saying upon the subject of medical delinquency as to diet, whether in this disease, or in most others, that the physician who is incapable of adapting the food to every condition of morbid action, and relies upon the experience of the patient, must be equally empyrical in all other details of practice. We are sensible that moral firmness is often at fault, and that public prejudice is allowed to embarrass this most important duty. But this is a selfish motive, and therefore more obnoxious to censure than the sin of ignorance. (2)

1) Boston Med. & Surg. Journ. Dec. 1839, pp. 298, 299.

(2) This principle is sometimes openly encouraged in high quarters. Thus Dr. Armstrong — "A placebo is necessary in the present state of *public knowledge*. If I give nothing but calomel and rheubarb at night and castor oil in the morning, and apply leeches, the patient, and *his friends* too, would lose all confidence in me. The best placebo is coloured water, or any thing simple." (*a*)

But, what would be their confidence should the imposture be detected? In the next paragraph, however, our author remarks that, "I know nothing in the world which requires a greater combination of intellectual and moral excellencies than to make a medical man." "He should be a man of the *strictest* integrity, and the most *upright conscientiousness*."

So far as the *patient* is immediately concerned, that is quite another question. Even Zimmermann

(*a*) Lectures on Acute and Chronic Diseases, vol. i. p. 415.

We have placed at the head of this appendix the doctrine which we shall continue to illustrate and defend: and, in the prosecution of which we shall confine ourselves principally to the consideration of tuberculous phthisis. Whatever may be true of the pathology of pulmonary tubercle, must apply in all other cases. (Pp. 10, 12, 67, 80, 119, 126, 128, 131, 141, 181—204; and Vol. I. p. 626—627, *note*.)

The conclusion that "tubercle is always the result of inflammation," is the suggestion of deep research on the part of one of the best and latest pathologists. This, also, is generally the doctrine of American physicians, especially throughout our great interior. Whether right or wrong, it has grown out of their adherence to the phenomena of nature. The contrary opinion, which now prevails extensively in Europe, is the offspring of morbid anatomy. The relative merits of the two systems of observation and induction, where one or the other is mainly pursued, may be well tested by the facts which are adduced in support of the respective doctrines. We have already stated, in behalf of the doctrine of inflammation, the salutary effects of a non-stimulating diet, and the pernicious tendencies of the opposite system. This is especially exemplified in the early stages of tuberculous phthisis, and we believe that we have fairly represented, upon that subject, the general conviction of the best observers who have made the vital signs of the disease paramount to the posthumous. We think, too, that we may safely say, that the earliest phenomena are those of inflammation. There is often expectoration of blood as an incipient symptom. (Sec. 16, and Vol. I. p. 371—384.) A sense of oppression at the chest, an increased secretion of bronchial mucus, pain in various parts of the chest, often increased, as well as the cough, by a deep inspiration, an excited state of the circulation, and more or less uniform heat of the skin, a dryness and burning of the palms of the hands, dryness of the feet, a *decided hardness* of the pulse, *buffing and cupping* of the blood, are amongst the earliest attendants of the disease. So universally, indeed, is this understood, that our brief recital of the symptoms may be considered supererogatory. The vital developments, which spring up in the progress of the complaint, carry all the weight which a constant succession of coincident results, from beginning to end, can accumulate upon any given question. These, to be sure, are more or less admitted, but cast aside as consecutive upon an inflammatory condition which is allowed to be sometimes established by the irritation of the tuberculous deposit, so only there be a due amount of red vessels connected with a degeneration of the pulmonary tissue. We shall therefore take the only ground which is left us, and the only one upon which it is of much importance to argue the pathology of the disease. The early stage of tubercle is the only period when art can often interpose successfully; though we see much in the foregoing admission to admonish us at all times to withhold those exciting causes, which may contribute along with the

went so far as to say that, he may very properly "be indulged with a few mercurial frictions merely to cure his imagination." (*a*)

Nothing is truer of the practitioner of medicine than the precept of the Imperial Stoic, — τον ως αληθως αρα μεγαλοψυχον, δει αγαθον ειναι — (*b*) their virtue is an essential element of greatness. This virtue is made to consist mainly in self-denial, which, in a strictly moral sense, is no where of such daily importance as in the chambers of the sick. This is the most difficult and painful of all his duties.

(*a*) On Experience in Physic, vol. ii. p. 349

(*b*) Meditations of Marcus Aurelius Antoninus.

irritation of the tuberculous deposit, to the production of that inflammation which is the essential cause of the fatality of the disease. Allowing to morbid anatomy all its claims, and taking its final admissions, and regarding the universal issue of the disease in destructive inflammation, nothing can be more obvious than the importance of avoiding stimulating food, and its worse associates, to keep down all predisposition to inflammatory action. We fully concur, however, in the combined treatment of mild tonics, and moderate depletion, when pulmonary consumption has reached its hectic stage, — according to the exigencies of the destructive inflammation. We go fully with the practical Rush upon this subject, and with him we also allow, at this period, a moderate proportion of animal food. But, we do it as without hope, except to prolong, a little, a painful existence. The period of hope has been lost in the irresistible march of disease, or has been suffered to pass under the delusion of a deceitful malady, or has been frittered away by a neglect of antiphlogistic means, or the catastrophe has been precipitated by the "incendiary treatment."

Doubtless the inquiry has already arisen, where are your anatomical facts? They are in reserve, for their appropriate place. We must first listen to the voice of nature, and we will then look at her when she can only tell the struggles through which she has passed.

We have done with our own experience, nor shall we greatly multiply that of others. We shall turn to the observer whom we last quoted, not only because he is always great, but because, on the subject of pulmonary consumption, his greatness is transcendent; but who, like Hunter and Bichat, "appears to be almost forgotten." (Vol. I. p. 713.)

"In relating," he says, "the facts that are contained in the inquiry, I wish I could have avoided reasoning upon them; especially, as I am confident of the certainty of the facts, and somewhat doubtful of the truth of my reasonings." (1)

This doubt, we believe, forever harrassed this faithful follower of nature; and, it is evident that it hung upon his universal doctrine of "debility." His first "proposition" is, that "pulmonary consumption is induced by predisposing debility," and this debility he supposed to be coextensive with "the whole system." He had even "a hereditary debility of the whole system" for "hereditary consumption;" and, in this respect he has not a few followers. In common with later writers, he has, also, "a debility of the blood-vessels" for "consumption," "a debility of the nerves" for "diseases of the stomach and bowels," and "a debility of the nervous and arterial systems" for "affections of the brain." But, he was aware of the difficulties of the hypothesis; and being in conflict with what experience taught him as to the method of cure, as well as his just estimate of the vital signs and the actual pathology, his testimony becomes the more valuable.

"Should it be asked," he says, "why does general debility terminate by a disease in the lungs and trachea, rather than in any other part of the body? I answer, that it *seems* to be a law of the system, that *general* debility should *always produce some local disease.*" (2)

But the fallacy of this hypothesis may be shown at once by those every-day

(1) Rush's Medical Inquiries and Observations, vol. i. p. 212.
(2) Ibid. vol. ii. pp. 62, 63, 77, 85.

cases where inflammations yield in the apparent ratio of the depressing means of treatment, and, as "debility" may become more and more strongly pronounced. It may be shown by those numerous instances where the system becomes excessively worn down by long confinement, by indigestion, &c., where no inflammations supervene, but where, according to the hypothesis, they should invade every tissue of the body. (Pp. 154, 261, Sec. 15, and *Bloodletting*, Sec. 11, 12, and 13.) We dwell, for a moment, upon this unintelligible subtlety in physiology, since, when we consider the havoc of phthisis, there are few diseases in which the doctrine is so likely to be pernicious. Rush felt the probability of its practical conflict with what he so well knew to be the real exigencies of the disease.

"It may seem strange," he says, "to recommend this *debilitating* remedy (bloodletting) in a disease *brought on by debility.*"

And the only explanation which he offers, is, to repeat the affirmation, state the causes, and to follow it up immediately by the contradictory fact that, "I have used this remedy with great success, in *every case* of consumption attended by a hard pulse," (1) &c.

The whole of Armstrong's theory of disease was, in like manner, vitiated with this chimera of debility, and led him to mechanical doctrines both as to the pathology, and the modus operandi of its remedies; whilst, like Hey and Denman, and Gordon, he persisted in stimulants where the former discovered their error and pronounced them to be demanded "in their greatest measure." (Pp. 231 — 243, 489, 520, and Vol. I. pp. 221, 256, 279 — 280, 312.)

But, Rush was eminently a man of facts, and it is truly wonderful to what an extent he disregarded his hypothesis of debility, both in his practice and in his specific principles. Debility, however, being his *sine qua non*, his authority as to the inflammatory nature of tuberculous phthisis, and the antiphlogistic treatment is, as we have said, the more entitled to weight, since it was the dictate of observation opposed by hypothesis.

We now proceed to a more methodical statement of the views of this remarkable man. He divided pulmonary consumption into three stages:

1st, The *inflammatory state;* 2d, the *hectic;* and 3d, the *typhus state.*"

"I yield," he says, "to the popular mode of expression, when I speak of a consumption being produced by tubercles. But, I maintain they are the *effects*," &c. "That the abscess, cough, tubercles, ulcers, and purulent or bloody discharges, which occur in pulmonary consumption, are the *effects*, and not the *causes* of the disease; and, that all attempts to cure it, by inquiring after tubercles and ulcers, or into the quality of the discharges from the lungs, are as fruitless as an attempt would be to discover the causes or cure of dropsies, by an examination of the qualities of collections of water, or to find out the causes and cure of fevers, by the quantity or quality of the discharges which take place in those diseases from the kidneys and skin. (Vol. I. p. 665.) It is to be lamented, that it is not in pulmonary consumptions only that the effects of a disease have been mistaken for its cause. Water in the brain, a membrane in the trachea, and a preternatural secretion of bile, have been accused of producing hydrocephalus internus, cynanche trachealis, and bilious fever, (p. 221;) whereas, we know they are the *effects* of those diseases only, in the successive order in

(1) Ibid. pp. 97, 64.

which each of them has been mentioned. *It is high time to harness the steeds which drag the car of medicine before, instead of behind it.* The earth, in our science, has stood still long enough. *Let us at last believe, it revolves round its sun.* I admit that the cough, tubercles, and ulcers, after they are formed, increase the danger of a consumption, by becoming new causes of stimulus to the system; but in this, they are upon a footing with the water, the membrane, and the bile that have been alluded to, which, though they constitute no part of the diseases that produce them, frequently induce symptoms, and a termination of them, wholly unconnected with the originial disease." (Vol. I. p. 342—361.) And, he argues farther, "that tubercles are the effects, and not the cause of pulmonary consumption is evident from similar tumours being suddenly formed on the intestines *by a dysentery*, and on the omentum by a yellow fever." (1) (P. 576.)

It appears, therefore, that Rush was not only tolerably well acquainted with morbid anatomy, but quite as well with the abuses to which it may be subjected as was Sprengell, who wrote about one hundred years before our author. (Vol. I. p. 457 — 458.) Now what has been the progress of medical philosophy even since Sprengell's day? Have the advances in mordid anatomy brought our "earth" to its right "revolution around the sun"? Has it speoially improved the pathology of tubercle since Rush poured the light of his philosophy upon it? But, let us hear our author as to his conclusions in regard to the nature of tuberculous deposit. He "maintains" that it is "secreted into the substance of the lungs, where it produces those tumours we call tubercles." Away, then, with the unfounded pretext that Rush was unacquainted with morbid anatomy. He understood it in all its aspects. Again,

"It is agreeable," says our man of facts, "to pursue the *analogy* of this disease with an acute inflammation of the lungs. They both make their first appearance in the *same seasons of the year*," &c. "In short, the pneumony and consumption are alike in so many particulars, that they appear to resemble shadows of the same substance. They differ only as the protracted shadow of the evening does from that of the noon-day sun." (2) And, our author justly adds, that "a difference in the *remote* causes does not invalidate the parallel." The two diseases are different species, or rather varieties, of a common genus. (Sec. 12 and 15.)

Our author divides the treatment into "PALLIATIVE and RADICAL," and all our experience is in strict conformity with his observation.

"1. OF THE FIRST OR INFLAMMATORY STATE. BLOODLETTING. I have used this remedy, with great success, in every case of consumption attended by a hard pulse, or a pulse rendered weaker by a laborious transmission of the blood through the lungs." (P. 248 — 251.) He states instances in which he bled eight times in two weeks, fifteen times in six weeks, and "with the happiest effects. Mr. Tracey was bled eighty-five times in six months by Dr. Shelton, in the inflammatory state of this disease, and he ascribed his recovery to this frequent use of the lancet. To these cases I might add many others of consumptive persons who have been perfectly cured by frequent, and of many others whose lives have been prolonged by occasional bleedings. But, I am sorry to add, that I could relate many more cases of consumptive patients, who have *died martyrs to their prejudices against the use of this invaluable remedy.*"

(1) Ibid pp. 74, 76, 82, 93, 1805. (2) Ibid. p. 94.

It is remarkable that Rush now combats the objection of "debility" that is raised against the use of the lancet; and he retorts with good effect that,

"The men, who urge this objection, do not hesitate to take from sixty to a hundred ounces of blood from a patient in a pnuemony, in the course of five or six days, without considering that the debility in the latter case, is such as to confine a patient to his bed; while, in the former case, the patient's strength is such as to enable him to walk about his house, and even to attend to his ordinary business." (Vol. I. pp. 274, 292 — 293, 318, 328.) "The relief obtained by bleeding is *so certain* in this state of consumption, that I often use it as a palliative remedy, where I do not expect it will perform a cure."

Our author quotes Cullen, and Hamilton of Lynn Regis, as concurring in his experience with bloodletting. He cites two instances, under his own observation, where the patients were radically cured by the discharge of a quart of blood from their lungs. (P. 558, and Vol. I. p. 275 — 277.) There are few practitioners who have not witnessed the same result from enormous expectorations of blood. And yet, "it is strange," says Armstrong, "how men of superior faculties to my own, go on to grey-headed old age without profiting at all by their experience." (1)

We had a patient, a young lady, who had been long troubled with indigestion, upon which had supervened an obstinate hepatic congestion. This was followed at intervals of about six months with distinctly marked idiopathic fever, (pp. 223, 236, 238,) frequently attended by moderate hæmoptysis. Copious and repeated bloodlettings were amongst the remedies by which we always succeeded in effecting relief, though never complete. In July, 1836, we were summoned to visit her in the country, and were told by her brother that she had been attacked with hæmoptysis, and had already lost several quarts of blood. Our immediate reply was, that it would either kill her or cure her, but most probably the latter. On our arrival, we found our friend Dr. Willet, who was also rusticating in the same place, had lent a helping hand to nature, and had abstracted from the arm two quarts more. From that time she advanced rapidly to a state of full convalescence, and has remained sound in health to the present April, 1840. The entire quantity of blood lost in one day was computed by Dr. Willet at four quarts. It may be also stated, in connection with what we have said in various places as to the doctrine of debility, that this lady was in a feeble state when the hemorrhage took place. (Vol. I. p. 292, 293.)

"From *many years' experience* of the efficacy of bleeding in this state of consumption, I feel myself *authorized to assert*, that where a *greater* proportion of persons die of consumption when it makes its *first* appearance in the lungs, with inflammatory diathesis, than die of *ordinary pneumonies*, (provided *exercise* be used afterwards,) it must, in *nine cases out of ten*, be ascribed to the ignorance, or the *erroneous theories* of physicians, or to the obstinacy or timidity of patients." Finally,

"In reviewing the prejudices against this excellent remedy in consumptions," continues Rush, "I have frequently wished to discover such a substitute for it as would with equal safety and certainty take down the morbid excitement, and action of the arterial system. At present we know of no such remedy; and,

(1) On Acute and Chronic Diseases, vol. ii. p. 101.

until it be discovered, *it becomes us to combat the prejudices against bleeding*; and to derive all the advantages from it which have been mentioned." (1)

Wardrop is an advocate for bloodletting in various scrofulous affections; but, with an enlightened prudence. And,

"When the lungs become the seat of tubercles, there is another source of relief derived from bloodletting, besides that of mitigating accompanying inflammation, and which ought to be kept in mind in the administration of this remedy." "By abstracting blood under such circumstances, (any increased determination of blood,) the congestion is relieved, which quells the increased frequency of the heart and embarrassment in respiration." (2) (Vol. I. p. 126—134, &c.)

We might go on with similar authorities, who derive their indications from enlightened experience and well grounded views in pathology; but this must devolve on such as may be disposed for a greater multiplication.

Having spoken of the prevailing doctrine of American physicians, that tuberculous phthisis is an inflammatory affection in all its stages, it becomes us to say, that the depletive system is less frequently carried out at the period when it can only be useful, than their views of its pathology would justify, or the symptoms and mortality of the disease most clearly demand. This is especially true of many physicians in our large cities; and we have reason to think that the prejudice against the antiphlogistic treatment has been more or less derived from abroad.

But, we must not yet abandon our modern Sydenham. "A second remedy for the inflammatory state of consumption," he says, "should be sought for in a MILK and VEGETABLE DIET,"— (these words being placed in capitals.) (3) Rice is put down as the best vegetable substance. "Vomits" are suggested where bleeding is opposed.

When the disease reaches the *Hectic* or *second* stage, "the treatment should be accommodated to the predominance of inflammatory or typhous symptoms; for the hectic state presents each of them alternately every week, and sometimes every day, to the hand or eye of the physician." "I am sorry, however, to add, that when bleeding has not been indicated, I have seldom been able to afford much relief by medicine in this state of consumption."

In the *third* or *Typhous states*, our author commends stimulants, and "a cordial stimulating diet," with which "milk" may be combined.

"A *dry situation, country air, and change of climate*, are alike proper in every state of the pulmonary consumption." What appears, as will be shown in the sequel, to be the result of late statistics, especially in warm climates, was well known to our close observer; viz.—

"It is remarkable that climates uniformly cold or warm, which seldom produce consumptions, are generally fatal to persons who visit them in that disease. Countries between the 30th or 40th degrees of latitude are most friendly to consumptive people." (4)

Our author then becomes zealous and full as to the "radical cure of consumption." "This consists of various kinds of exercise and labour in the open air, according to the absence of inflammatory symptoms, &c. This system

(1) Medical Inquiries and Observations. vol. ii. p. 97—104. (2) On Bloodletting, p. 56.
(3) The Italics are ours. See vol. i. p. 418. (4) Ibid. p. 107—115.

must be continued through life, or the disease will be likely to return. "Too much cannot be said, however, to enforce the importance of avoiding fatigue."(1)

We have pursued the foregoing system in all its details, throughout our professional life, and can truly say that every successive case has added confirmation to our views of the pathology, and of the no little control of art over pulmonary phthisis.

The incipient formation of pulmonary tubercle is supposed not to depend on inflammatory action, because it is rarely developed in acute or chronic pneumonia. (2) M. Louis sets aside the inflammatory origin of tubercle upon the ground, especially, that "the sex that seems most exposed to phthisis is least frequently attacked by pneumonia or bronchitis, and this, in the proportion of one to three." (3)

If our author mean by bronchitis, ordinary catarrh, or colds, his affirmation is at variance with universal experience.

Broussais, on the contrary, has found pulmonary tubercle so frequently attendant upon acute and chronic pneumonia, and pleurisy, that he considers those affections among the frequent causes of pulmonary tubercle. Sir J. Clark states, that "inflammation of the pulmonary tissue is considered a frequent, and, by some authors, the chief cause of consumption." That, "certain it is, no affection so commonly appears to precede consumption, as bronchial irritation." (4)

"If it be now considered who are the advocates, and who the opponents, of the inflammatory origin of tubercle, it will be found that the former are generally such as have formed their conclusions in the school of nature, and that the latter have derived them from doubtful appearances after death. The opinions of one are made up from a vast series of facts which appertain to the existing state of morbid action. Those of the other, after all the essential facts have disappeared, and the pathological cause has become extinct. And, if we cast our eyes around, and inquire who it is that so strenuously oppose the contra-stimulant plan in the early stages of pulmonary phthisis, we shall find them to be those who commend wine and bark in congestive fevers, in erysipelas, or in prostrating inflammations, or who rely upon tartarized antimony in those pneumonies which speedily end in pulmonary disorganization. We cannot, therefore, agree with Dr. Baron, that our practice "in tuberculous phthisis is founded upon a mere dictum." (5)

In respect to the objection that tubercle is not developed by pneumonia, allowing that it may be true in a greater proportion of the cases, it ceases to be tenable when it is considered that the cellular tissue is mostly the seat of the latter affection. Pulmonary tubercle is probably generated in the mucous and serous textures, (6) at least in a general sense. Andral alters his opinion as to the cellular tissue, (7) and subsequently remarks, "cette sécrétion peut égale-

(1) Ibid. p. 124—138.

(2) Laennec on Diseases of the Chest, p. 296. Louis on Phthisis, s. 467. Carswell's Illustrations of the Elementary Forms of Disease; art. Tubercle. Andral's Pathological Anatomy.

(3) Ut supra, s. 472. (4) On Pulmonary Consumption, pp. 183, 185, 187.

(5) Delineations of the Origin and Progress of various Changes of Structure, &c.

(6) See Carswell's Illustrations of the Elementary forms of Disease.

(7) Patholog. Anat. vol. i. p. 314.

ment avoir lieu dans plusieurs des tissus qui entrent dans la composition du poumon." (1) So, also, in his notes to Laennec's Treatise on Auscultation.

It does not exactly follow, however, that because pneumonia may frequently fail of developing scrofulous inflammation, that it will be equally true of catarrhal affections; and this will be the more apparent should it result that the tuberculous matter is most frequently generated by the pulmonary mucous tissue. It is also manifest that much may often depend upon the precise nature of the various predisposing causes, external and constitutional, and, therefore, upon the exact modification of the original inflammatory action. (Pp. 478—479, 502—506.) That variety which constitutes epidemic influenza is manifestly different from what arises from the special agency of temperature, and may result more frequently in phthisis than the latter. This we believe to be true; and it will account in part for the discrepancy of observers in respect to the connection of colds with pulmonary tubercle. Broussais remarks, that pneumonia and pleurisy constantly gave rise to pulmonary tubercle in the army. But, he had no "numerical tables;" and M. Louis, therefore, after saying that "pneumonia exerts no influence in the development of phthisis," assumes that the

"Proposition of M. Broussais is *wholly conjectural*." "To render this conclusion *rigorous*, tables of mortality were necessary for the purpose of ascertaining whether the lungs under these circumstances were more frequently tuberculated than in individuals of *the same age*, *dying in the civil hospitals*, *in a time of peace*." (2)

If no better objections than the foregoing can be brought against the observations of a most distinguished and profound philosopher, they must be classed in the category of those which have been hurled by our author at all ages and at all members of his profession.

But, that *catarrhal* affections are the most common causes of pulmonary tubercle is too obvious to American physicians to require any proof. On this continent the disease is greatly confined to the Atlantic coast, over the whole of which, from Maine to Georgia, whether in cities, villages, or a sparse population, it is well known to prevail. Our most popular retreats, Charleston in South Carolina, and Pensacola in Florida, are scarcely more exempt in inclement winters than the northern cities. And here, as everywhere else, pulmonary tubercle is mainly developed at those seasons when catarrhal affections are most rife; and should influenza sweep over our coast, we invariably look for a greater multiplication of the victims of phthisis.

On the other hand, the vast interior of this country is comparatively exempt from colds and influenza, and however other remote causes may contribute to the development of phthisis, it is a remarkable coincidence that an exemption from the latter appears to be greatly in the ratio of an exemption from colds. This is also true of the whole interior of Lower Canada, as we know from much personal observation. The population of that country are but little subject to catarrhs, and are remarkably exempt from pulmonary consumption. The fact should be also considered, that Lower Canada has been settled for centuries, whilst the whole interior of the United States is of recent date.

We believe the same to be generally true of Europe, although differently stated by Laennec, (3) Weatherhead, (4) and some others. We have no confi-

(1) Clinique Méd. t. iv, pp. 13, 25.
(2) On Phthisis, s. 467, 468.
(3) Diseases of the Chest, p. 300.
(4) Diseases of the Lungs, p. 41.

dence in the *general* inductions derived from the army statistics, for reasons which will be assigned. Beddoes says that in England, the disease is commonly owing to "*catching cold*;" that, "*colds* prove fatal to the young principally by giving rise to consumption;" that, "the *extreme frequency* of consumptions in Great Britain seems to have been *justly ascribed* to the variableness of the climate."(1) And, is it not upon this very principle that they who deny the connection of phthisis with colds advise a retreat of their patients to *dry*, *temperate*, and *equable* climates? Late investigations show that, in London, 236 deaths of every 1000 are from consumption, and about the same at Paris; whilst in Sweden and St. Petersburgh, where colds are comparatively rare, the ratio is only 63 to 1000. It appears to us that this view of the subject is generally confirmed by Sir J. Clark, and by Andral in his late edition of Laennec on the Chest. Such, too, was the prevailing conviction as to the connection of phthisis with colds, till the non-inflammatory origin of tubercle began to be anatomically denied.

Professor Alison enters into a very elaborate examination of facts, to show that tuberculous phthisis depends principally upon cold, producing repeated and neglected inflammations of the lungs. (2) And thus Morton, in his excellent treatise:

"A *large proportion* of consumptive patients are able to trace the first appearance of disease to some exposure to cold or wet, an incautious change of dress, &c., followed by a *cold*, which was *neglected until the symptoms became severe, complicated and unmanageable.* I think, with Broussais, that, if the catarrh appears to be accidental, the tubercles cannot be accused of having produced it. Those who deny the agency of bronchitis, appear to do so from *the fear of admitting* the *propagation of inflammatory action from one tissue to the other.*"(3) It is proper, however, to say that Dr. Morton supposes that the formation of tubercle may depend on different pathological conditions. We are utterly opposed to this construction for reasons which we have assigned in our second section on the Theories of Inflammation, and in many other places.

We might cite many other late authorities to the foregoing effect; but we shall only refer to an elaborate article in the British and Foreign Medical Review, (4) relating, in part, to the latest statistics of pulmonary consumption.

Various other coincidences betwixt the exciting causes of inflammation, the presence of that disease, and the formation of tubercle, might be stated. Thus, in dysentery, it is said by Cleghorn, that he "has seen in many cases scirrhous tubercles straitening the cavity of the colon in many places." (5)

Rush, also, "requests us to attend to the *suddenness* with which tubercles are formed in *dysentery.* They render it probable that they are formed in the *same sudden manner* in the *lungs*, and that they are the *consequence*, and not the *cause* of that disease in the lungs which ends in pulmonary consumption." (6) And, who will deny the accuracy of this philosophy?

Sir J. Clark, in his excellent work, remarks, that, "the mortality from consumption is greater in the West Indies than on any other station;" and he considers "its great prevalence there, one of the most remarkable results of

(1) On Consumption, &c. pp. 149, 150.
(2) Trans. Medico-Chir. Soc'y, Edin. 1824, p. 365.
(3) Illustrations of Pulmonary Consumption, p. 42.
(4) No. 11, 1838, p. 1.
(5) On the Epidemical Diseases of Minorca, c. v. p. 144.
(6) Ibid. *note.*

his researches." (1) And why so "remarkable"? For the reason that Sir James had truly imbibed the belief that consumption is a rare disease in those islands; and it is upon this well known ground that Europe and America have sent their phthisical subjects to those regions. Weatherhead, (2) Rush, (p. 616.) Costillat, Ferguson, Bancroft, and others, state the fact truly, that consumption is "comparatively a *rare* disease in tropical climates." The venerable Dr. Bancroft, long resident at Kingston, assured us that it could not be regarded as a disease of Jamaica; and we obtained the same information at other islands. Beddoes made particular inquiries into the fact. From some, he learnt that consumption occurred in the islands, "whereas, others, (and particularly persons not belonging to the profession,) assert that it is almost wholly unknown; in confirmation of which opinion medical writers may be quoted." He then addressed a letter to Edwards, the historian of the Antilles, for information upon the subject; to which Mr. E. replied, that "I understand that females of the West Indies are very liable to consumption on coming to England. *In their native country, consumption is almost unknown.*" (3)

The whole conclusion, as to the prevalence of consumption in the West Indies, has been derived from army statistics; made up, in the first place, according to admission, in a crude and ignorant manner, and, in the next place, of subjects who should never, as it appears to us, be taken as examples for any conclusions in physiology or pathology, unless to advance our knowledge of the remote causes of disease. (Vol. I. p. 302—303, 693, *note*, and on the typhoid fever of Paris.)

In Major Tulloch's late Statistical Report, we find it stated, that "catarrhal affections are not one half so common as in Britain," whilst "at least twice as many cases of consumption originate in Jamaica, as at home."

Now, however true this may be in relation to the army, it is wholly contradicted, as we have said, by all experience in respect to the inhabitants of that, and other West India islands. But, admitting its universal application, it in no respect shows that "catarrhal affections" are not the principal exciting cause of consumption in other climates. The conclusion is neither logical nor liberal. The fact would only show the existence of other causes, which has never been doubted. Of these, diseases of the digestive organs are among the most frequent; and such appears to have been the case with the army at Jamaica. Bilious fevers were excessively rife; and where convalescence occurred, it is a reasonable conclusion, that, in the materials of which the British army is composed, the fever had contributed either directly to the development of tuberculous inflammation, or laid the foundation of hepatic and gastric affections, which are well known to exert the same pulmonic influences. But, we do not speak without the best authority as to the miserable constitutions and habits of, at least, a large portion of the British army. We find it amply shown in British records; and, down to the last moment, we see that Sir A. Halliday, in his Review of Major Tulloch's Report, represents the army as addicted to intemperance, and, that one half of the sickness and mortality, on the West India station, arises from drunkenness and its kindred vices. Connected with these causes, are military hardships under a tropical sun, a bad diet, and impure air.

(1) On Pulmonary Consumption, &c. p. 156.
(2) Op cit. p. 41.
(3) Beddoes on Consumption, pp. 19, 20: 1799.

These Reports, therefore, are worthless as it respects the questions at issue, and especially in relation to the character of disease among the proper inhabitants of a country. This is even demonstrated by the Reports themselves, in the comparative exemption of the officers, (although high livers themselves,) from pulmonary phthisis; the proportion at Jamaica being only as 4 to 15. Of all the islands, too, the officers of the army at Jamaica are most addicted to habits that are likely to lay the foundation of consumption. It is evident, therefore, that it would be as reasonable to impute the vices and broken down, and scrofulous constitutions imported from Europe, to the natural tenants of the soil, as their diseases.

And what are the substantive means of information upon the question before us? Are the facts derived from medical sources that are proper to be admitted within the archives of science? Or, are not rather the best means of knowledge overlooked in the fascinating display of "numerical tables" and the "rigour of mathematics?" We are told by Sir A. Halliday, that such is the fugitive life of the army physicians in the colonies, that they only pass through them; and the sick, in consequence, are perpetually transferred from one inexperienced officer to another. (1) And, what can better prove the title of these Reports to the consideration we would provide them, than the fact of their being compiled by British Majors? (P. 207, and Vol. I. pp. 294—296, 302—309.)

But, it is a capital mistake, that colds are not common in the West Indies. So entirely the reverse of this is it, that it is truly stated, as the result of direct observation, that, at St. Croix, which is the place of refuge for American invalids, and one of the most equable climates, "their flannels should be worn next the skin; and the upper garments should afford a pretty good defence against sudden changes. Gentlemen of the island almost always wear cloth coats. Violent heats from exercise will *lead to colds*, if they are not followed by more serious consequences. They will be frequently admonished, as I often was, while walking at my ordinary rate, not to walk too fast." (2) There are no people more apprehensive of taking cold, or who more carefully avoid sitting in currents of air, than the West Indians. Not that they fear *consumption*, for there are other elements wanting; but, they constantly prove an *exciting* cause of *fever*. And, although these colds be otherwise unimportant, they are sufficient, especially in connection with the other causes we have stated, to develope tuberculous phthisis in the predisposed and broken down constitutions of the British army. And who, after knowing the delinquency of colonial physicians, and that the reports are compiled by Majors of the army, will yield his credulity so far as to suppose that the investigation of cases was carried back to the catarrhal affection, or even that catarrh forms a subject of any observation either by the physicians or the majors? It is notorious, in Europe and America, that it constantly happens when tubercle has actually supervened upon catarrh, that the attention of the patient is not attracted to his situation till his case becomes irremediable. And, we doubt not, it is especially from

(1) Moseley has a just rebuke upon the practice of physicians, "in travelling post through a country, in order to convert them to the support of some new hypothesis." — *On Tropical Diseases*, pp. 134, 136; 1804.

(2) Phila. Eclectic Journal of Medicine, vol. ii. 1838, p. 18.

this lack of observation of the early history of the complaint, that it is conscientiously supposed, by able men, that catarrh has no connection with consumption.

Nevertheless, there are physicians in the British army, who, like Robert Jackson, are among the great bulwarks of medicine; and it is from such observers that we obtain the following important record in Major Tulloch's Report, with which we are content to close this part of our investigation. Thus:—

"Not only is consumption productive of *great mortality* in this command, (the Windward and Leeward,) but, *inflammation of the lungs* and *chronic catarrh are nearly twice as prevalent, and twice as fatal as among troops in Britain.*"

There is, however, another aspect in which this subject should be regarded for a moment. Although consumption be rare among the natives of tropical climates, the West Indies are peculiarly unsuited to such as have become predisposed to the disease in foreign countries, and especially to those in whom the disease is already developed. (1) The reason is to be found, in part, in what we have already said as to the liability to catarrhal affections in these climates, but more so in the excessive heat. Europeans are aware of this, and have long since abandoned the islands as a place of refuge. But, Americans continue to be the victims of the old prejudice. Rush pointed out the error, as we have seen, (p. 616,) but in vain. Were the most salubrious mountains of Jamaica more accessible, (2) they would form the best asylum within the tropics; though Cuba is preferable to any of the West Indies. The best retreats are Madeira and the Cape of Good Hope.

Finally, passing to another climate, we learn that, at Copenhagen, "catarrh, manifesting itself by cough and coryza, is stated by Dr. Otto to lay the frequent foundation of phthisis, and other intractable diseases. (3)

Again, it is contended that there may be a want of the usual physical signs of inflammation. Thus, Dr. Hall: —

"Redness, tumour, enlarged capillaries, effusion of serum, or lymph, do not, any of them, uniformly attend the development of tubercle."

Admitting this to be true, we do not admit the assumption that these signs are indispensable to inflammation in all tissues and under its various modifications. It is a dictum laid down by the schools, as we have endeavoured to show. (P. 316 — 331.) It is the offspring of morbid anatomy, not of an observation of nature. Our author continues, —

"The very commonest results of inflammation, the effusions of serum, or lymph, do not take place at the moment the tubercle is deposited, or during the crude state of this morbid deposit."

(1) Prof. Alison's remark, that, "a great majority of the inhabitants of hot climates are unusually prone to scrofula, when they come to temperate climates," (*a*) must be received in a more limited sense. But, allowing its truth, since they generally settle in our sea board cities, it only shows the powerful agency of colds in the production of the disease. For, however they be liable to catarrhal affections in hot climates, they are always comparatively mild, and yield far more readily than in the temperate zones.

(2) Aristotle describes the whole interior of Jamaica, when he speaks of the verdant mountains, the flowing of rivers, the swelling tide of the ocean, the unfolding of flowers, and the ripeness of fruits.

(3) British & Foreign Med. Rev. Jan. 1840, p. 217. — Our reviewer says of Dr. Otto's paper, that, "here we have an essay exuberant with matter, all of which is admirably arranged, while each particular topic is treated with the degree of fulness and precision due to its relative importance." — *p.* 216.

(*a*) Trans. Medico-Chir. Socy. Edin. 1824, pp. 397, 399

Very true; and for the obvious reason that the tuberculous matter constitutes the morbid deposit, whilst the best pathologists are coming to the conclusion that it is composed, originally at least, of some modified state of coagulable lymph. The inflammation being either at the onset of a specific kind, (1) or soon influenced in that respect by the constitutional "diathesis" of the subject, we naturally look for a specific product; whilst this, by the way, obviates the necessity of any intervention from the humoural pathology. Upon the same principle, therefore, we might object to the dependence of an abnormal production of common lymph upon inflammation, because it is not accompanied by some of the other products. And so of pus, or any new formation that is the acknowledged result of that mode of action. The same remarks, too, are equally applicable in these instances to the frequent absence of vascularity, heat, swelling, or throbbing, &c. We should not, however, neglect stating the important fact that pulmonary tubercle is often preceded by effusions of blood, (sec. 16,) however this may be assumed to be otherwise. Here, Andral is with us (p. 558.)

In allowing, upon this subject, a proper weight to the opinion of our able author, it should be recollected that at other times, to carry out the hypothesis of irritation, reaction from loss of blood, &c. he refuses to rocogni se increased vascularity, effusions of lymph, of serum, &c. and all the characteristic vital signs of inflammation, although strongly pronounced, as significant of that disease; but, on the contrary, that he maintains they are then indicative of some mode of action directly *opposite* to inflammation. (Vol. I. pp. 229, 240 — 248, 253, 258 — 263, 267 — 269.)

Finally, it is the opinion of Dr. Hall, that, "tubercle is deposited from the influence of constitutional causes, and is neither the *effect* or *cause* of inflammation." And yet he says immediately, that "*softened* tubercle is a foreign body, and the exciting cause of inflammatory processes." (2)

What are the "constitutional causes?" Evidently nothing more than "diathesis," or a disposition to a disease which has no existence till excited by other causes. Otherwise, this "constitutional cause" should never cease its operation in depositing tuberculous matter. Most of the diseases to which we are liable are full of analogies which show the nature of the "constitutional cause," and what it is that gives rise to the "tuberculous deposit." And, if we now carry our views a little farther, and consider how constantly it happens that the "deposit" is preceded by colds, dysentery, hemorrhages, &c. and how often the progress of the deposit is arrested, and its absorption promoted in its early stages, by bloodletting, &c. we shall have no difficulty in seeing that the essential pathology of the disease is constituted by something more than the "constitutional cause."

No disease depends more proverbially upon peculiarities of constitution that modify the ordinary character of inflammation, whatever part it may affect, than the pathological cause of tubercle and its associate results. The whole subject, however, has been mystified by referring the formation of tubercle to a

(1) We employ this expression, as we have said, in Mr. Hunter's sense of the modified forms of inflammations.

(2) Lectures on the Theory and Practice of Physic, in London Lancet, 1837.

"tuberculous diathesis;" (1) and yet the very foundation of the principles for which we contend.

Dr. Davies asks, when objecting to the inflammatory nature of consumption, whether "*the products of ordinary inflammation are anything like tubercles?*" (2) Not exactly like, as we have just said. But, we find some analogies in the depositions of serum, lymph, pus, bone; medullary, sarcomatous, and melanotic matter, encysted, non-encysted, and infiltrated, — all of which take place in the lungs; the various products of cutaneous diseases, &c.; each being determined by peculiar modifications of inflammatory action. This is exactly the reason why tubercle differs from the products of "ordinary inflammation," and from all other specifically modified forms of that disease, which are the cause of all the differences in the various products.

To give this subject its utmost precision, and to render it luminously subservient to the humoral pathology, the old *experimentum crucis* by Moulin, of injecting quicksilver into the arteries and bronchia of animals has been repeated by Cruveilhier, (3) Kay, (4) Gaspard, (5) Lombard, (6) and other inquirers after truth. The animals were killed in a few days afterwards, when their lungs were found more or less filled with what were considered small tubercles, but the real nature of which had been explained by Moulin, in 1690. (See Vol. I. p. 517, *note.*) Nor does it appear to have been considered important in these experiments to inquire whether the animals enjoyed the tuberculous "diathesis." (7) But, supposing that the quicksilver established not only the "diathesis," but also the true tuberculous deposit, we should then get the best evidence of the direct dependence of the latter upon inflammation, since the pulmonary texture was extensively agglutinated by coagulable lymph. This will remove entirely the objection by Dr. Hall as just stated.

Whether from Moulin's experiment, whose accurarcy cannot be doubted, or by an a priori process, "it has been sometimes supposed," according to Beddoes, "that hæmoptysis and phthisis have been produced by the internal use of mercury; and this fluid metal has been imagined to increase the momentum of the blood so much as to break through the loose and tender vessels of the lungs." "This is a very clumsy account of the beginning of phthisis, and such gross mechanical ideas are doubtless inapplicable to the motions of the living system." (8) (Sec. 1, 2, and 3.) But if so, what shall we say of the injections? (P. 172, *note.*)

After what we have variously said as to the organization of all parts of the

(1) See Andral's Notes to Laennec Traité de l'Auscultation Médiate.

Although Dr. Williams advocates the general dependence of tubercle upon inflammation, and contends for "*established laws*," he sometimes compromises the matter; as, for instance, in this affair of "diathesis." He "thinks that the lungs may become infested with tuberculous matter, by secretion in tissue bearing no marks of other lesion, the tuberculous matter being here apparently deposited *through excess* of tubercular diathesis." (*a*)

(2) On Diseases of the Lungs and Heart, p. 287. (3) Nouvelle Biblioth. Medicale, Sept. 1826.

(4) See Alison's Paper on Tubercle, in Edin. Med. Chir. Trans. 1827, p. 302, &c. (5) Ut cit.

(6) Thèse sur le Tubercule, 1827. (7) In cases where there is no antecedent ground for suspecting the "diathesis," it is maintained by M. Andral, that "the diathesis is proved by the *occurrence* of the tubercles, *which could not happen without the diathesis.*" (*b*)

(8) On Pulmonary Consumption, &c. 143.

(*a*) Rational Exposition, &c. of Diseases of the Lungs and Pleura, p. 172. See, also, pp. 161, 51, *Philad.*

(*b*) Notes to Laennec's Trait. de l'Auscultation, &c. p. 221.

living body, and the necessity of organization in all new formations which carry on processes analogous to those of the natural tissues, (p. 354 — 361, &c.,) we shall not inquire very elaborately into the general doctrine of the unorganized nature of tubercle, which has been founded upon the failures of injections, and of that all-seeing instrument, the microscope. Analogy, the symptoms, the curative means, and the changes to which tubercle is liable, are rather more satisfactory than negative facts from sources like the foregoing. We have seen, that upon the same ground, the serous membranes, and other important parts, have been declared to be unorganized, and in no respect liable to disease. (Pp. 318, *note*, 577.) Have the nerves been injected? We have seen much of analogous speculations as to the formation of the secretions in our Essays on the Vital Powers, the Humoral Pathology, and especially in our second section on Inflammation.

It was the result of Andral's investigations, as it has been that of others who are supposed to belong to the non-inflammatory school, that,

"Tubercle must be considered as the result of a modification or perversion of *nutrition*, (p. 324, *note*,) which is often attended or preceded by *an active sanguineous congestion.* THIS is *all that we know for certain. Beyond this, everything is mere conjecture.*"

Divested of the phraseology peculiar to our author, (Sec. 15,) the foregoing statement avows that inflammation is the pathological cause of tubercle, beyond which "everything is mere conjecture." Again, our author says,

"In certain cases, and *these not the least numerous* either, the morbid alterations of the lungs were *evidently prior* to the formation of tubercles, and *contributed* to produce them." These "*prior* and not the least numerous alterations" are "*hyperæmia* of the air cells, and ultimate ramification of the bronchia, with or without *obliteration* of the cavities, and *considerable thickening* of their parietes." Also, "an effusion of blood into the tissue of the lung." (1) This opinion is variously illustrated in his Clinique Médicale; where this cautious observer, so scrupulous in his admissions as to all inflammations, distinctly avows his belief, predicated, too, of morbid anatomy, that the formation of tubercle depends upon that mode of action. Thus: "*Le travail pathologique qui précède la sécrétion tuberculeuse est une congestion sanguine active.*" (2) It cannot be allowed that contradictory suggestions, by the same writers, shall preponderate against their facts and experimental inductions. (3) (Vol. I. pp. 524, 525, 605, 611, 627—632.)

Cruveilhier, also, taking up the subject where it was left by Bayle, Louis, Laennec, Velpeau, Rostan, and others, concluded in opposition to them, that the formation of tubercle is more or less dependent on inflammation. (4) But, how far this may be connected with the quicksilver experiment, we are not distinctly told. Laennec, however, allows that *dissections* have disclosed the frequent coexistence of tubercles and acute or chronic inflammations.

The able Alison has pushed the inquiry, till it has again and again resulted in his conviction, that tubercle depends upon inflammation; and that it may be, at least, "safely stated, as a general principle, that the frequent excitation, and rapid acceleration of tubercles, by inflammation, is the great practical limit to

(1) Patholog. Anat. vol. i. p. 328; vol. ii. pp. 344, 345.
(2) Clinique Méd. t. iv. p. 25; also, 24, &c. 1834.
(3) See his Notes to Laennec's Trait. de l'Auscultation Médiate, 1838.
(4) Patholog. Anat. and Méd. Pratique.

the use of the tonic remedies and regimen in scrofulous habits." And, as to the proper remedies in what is called the "inflammatory stage," he advocates "rest, light diet, and even antiphlogistic remedies." (1)

The following reflection by the same writer is also worthy of all consideration by those who reject analogy, or who, departing from the plain rules of philosophy, would refer the changes which tubercle undergoes to a chemical or a mechanical process. (*Theories of Inflammation*, Sec. 2.) Thus, —

"The general and imperceptible transition of the matter constituting tubercles into that which is clearly the effect of inflammation is, of all the phenomena observable on dissection, that which appears to furnish the most direct evidence of the formation of tubercles by inflammatory action. I have no doubt of being able to form a series of preparations, in which a gradual transition shall be obvious, from the true hepatized condensation of the lung, through the intermediate changes of structure to which Laennec has given the name of gray hepatization, and of tubercular infiltration, to that kind of disorganization which consists for the most part of tubercles; — and I am much confirmed in my opinion as to the close analogy of these structures, by finding that the researches of Andral have led him to the belief, that the appearance to which Laennec gave the name of *Tubercules infiltrés*, is always to be regarded as the effect of chronic inflammation." And again, "it is the result of the observations of Andral, that the conditions which appear most requisite, in order that inflammation may generate tubercles, are the *long duration*, and *slight intensity* of that inflammation." (2)

Such, too, are the *facts* of M. Louis. Thus, for example, "peritonitis, when of a chronic character from its commencement among adults, is according to the facts which I have recorded, constantly tuberculous." (3) In all these cases, too, the same formations are said by our author to exist in the lungs. Here, then, morbid anatomy coincides with the vital signs, the results of treatment, &c. The inflammatory origin of tubercle in chronic peritonitis is obvious enough; but there may be less of the physical proof in the lungs than in the peritoneum. Reason, however, points to the foregoing coincidence as one of the evidences of a common pathological cause; nor do we doubt that physiologists will yet come to the conclusion that, if tuberculous deposit depend on inflammation in any one case, it does in all others. There is the same precision in respect to the mode of action as is necessary to the generation of common pus, and we may say, to the virus of smallpox. Any other philosophy appears to us subversive of fundamental principles. (P. 181—204.) We should say, however, that the testimony of Andral and Louis upon this, as on other subjects relating to the pathology of inflammatory results, cannot have much weight; since the former generally denies the existence of inflammation, as a disease, as will appear more fully in our next essay, whilst the latter reasons upon the results of inflammation like no other observer. (See *Remarks* upon his *Writings*.)

That distinguished observer, M. Lombard, is with us upon this question; considering tubercle the product either of active or chronic inflammation. (4)

Dr. Abercrombie thinks that we "have every reason to believe that tubercu-

(1) Trans. Med. Chir. Socy. Edin. 1824, pp. 404, 436; and 1827.
(2) Ibid. 1827, 286, 305. (3) On the Method of Observing, ch. 1, *tr.*
(4) Thèse sur les Tubercules, 1827.

lous disease of the mesenteric and lymphatic glands is strictly analogous in its nature to the tubercles which we find in the lungs." "In their first state of simple enlargement, these glands present, when cut into, a pale flesh colour, and an uniform soft fleshy texture. As the disease advances, the texture becomes firmer, and the colour rather paler." "There seems every reason to believe, that the *peculiar* disposition which constitutes it may take place from any tissue of the body." (1)

We may say, indeed, that almost every distinguished observer has allowed that the generation of tubercle depends *sometimes* upon inflammatory action, and that it is a *specific* substance. This, according to ordinary rules in philosophy, fixes its universal origin in an inflammatory condition of disease. (P. 181—204.)

It is allowed by distinguished pathologists, Andral,(2) Cruveilhier,(3) Gross,(4) Rush,(5) Gendrin,(6) and others, that tuberculous matter is at first fluid, and like lymph, it concretes afterwards. Andral says, "*les tubercules pulmonaires sont le produit d'une sécrétion morbide.*" Rochoux, and Laennec, consider them composed of granules, such as are seen in the buff of inflamed blood. Bricheteau calls the deposit "*une atome de liquide vicié.*" (7) It is stated by Carswell, that, "tuberculous matter is in general formed, ab origine, on the secreting surface of hollow organs, where it is seen as distinctly as if it had been thrown into them from a syringe." (8) According to Andral, recently secreted lymph is very apt to become the seat of tuberculous formations, sometimes within a fortnight after the access of the inflammation. Cruveilhier says, that the albuminous matter secreted in pneumonia and other inflammations often presents the appearance of tubercles. So, also, Addison. (9) It may be said, likewise, that Louis affirms, that "the fever, in the majority of phthisical cases, is dependent on tubercles in the lungs, and not on the consecutive morbid changes." (10)

Thus might we go on with authors who admit, and others who generally deny, the inflammatory origin of tubercle; and we should everywhere find their facts terminating in one conclusion. We should also obtain an undeviating evidence that tuberculous matter is not only secreted like coagulable lymph, and by an inflammatory action, but that it exists equally under circumstances the most favourable for organization. And yet, is organization denied by those who reject the vital signs, and rely upon injections, the microscope, and physical appearances. The suppuration of tubercle is accordingly supposed, as we have seen of common pus, (p. 181,) to depend upon physical agencies; that, it is owing either "to a spontaneous change taking place in the composition of the matter itself," or "to an imbibition of fluid matter, as serosity, pus, blood, &c. furnished by the surrounding tissues." (11) This, we believe to be a common doctrine; whilst the technical expressions of "softening" and "melting" are substituted for that of suppuration; lest, perhaps, the latter should imply organization, and the existence of inflammation as its cause. (P. 189.)

(1) Trans. Med. Chir. Soc'y. Edin. 1824, pp. 683, 686, 687.
(2) Clinique Médicale, t. iv. pp. 12, 24, 38.
(3) Méd. Pratique, &c.; and Patholog. Anat.
(4) Ut cit. (5) Ut cit. (6) Hist. Anatom. des Inflam. t. ii. p. 587.
(7) Gazette des Hopitaux, 1837. (8) Illustrations, &c. Art. Tubercle.
(9) Guy's Hospital Reports, No. 4. (10) On Phthisis, s. 395.
(11) Weatherhead, on Diseases of the Lungs, &c. p. 37.

And, in fulfilling our duty of demonstrating the operation of hypothesis upon the great laws in physiology and pathology, we must advert to the fact, that some distinguished observers who admit the dependence of pus upon inflammation, and that chemical tests identify with it the matter expectorated in consumption, maintain that the examination of this matter "is of no use whatever, in forming our diagnosis of phthisis; *for the disease is not the result of inflammation of the lungs.*" (1) And then, we have the declaration by one, that, "softening begins most frequently at the circumference of firm tuberculous matter;" (2) whilst others assert that, "the greater number of tubercles are perfectly soft within, whilst their external parts are in a state of considerable firmness." (3)

On the other hand, Prof. Gross hopes something from "chemistry in establishing the pathology of tubercle upon its inflammatory basis." In speaking of scirrhus, which he regards as inflammatory, he remarks that "chemistry must lend her aid and lay the results upon the altar of science." (P. 204.) But, *cui bono?* Our able professor shall say. The experiments of Hecht, he observes, "have fully established a close analogy between scirrhous and tubercular matter." Now, what say the chemists of the latter? Our author shall answer: "With these results (Hecht's) the examination of Thenard, previously made, *is strikingly at variance*;" whilst the latter chemist was as capable, at least, as the former. But this is not all. Our author very justly believes that "the chemical composition of tubercular matter varies, not only in the different stages of its existence, but also in different animals;" and he makes exactly the same affirmation of schirrous. (4) (P. 192—197; and Vol. I. p. 627—628.) Supposing, then, that chemistry is not perfectly imbecile when it approaches organic matter in its natural state, how is it to aid pathology in the midst of such various and incessant mutations? (Pp. 114—121; and vol. I. 529, 639—640.)

If, however, chemistry must be brought to the illustration of this subject, (p. 192,) let us hear Mr. Bird, who has done so much for the dignity of chemical science in its relation to organic matter. At the meeting of the British Association of 1837, (the authority being rather "old,") Mr. B. stated that he found the crude tubercle similar in its composition to albuminous or fibrinous substances; whilst, in its softened state it was chemically identical with pus. He subscribes, however, to the doctrine of "softening." Dr. Williams concurred in this view; having taught that "softening" was a modification of suppuration. But, since he had also taught that tubercle is wholly unorganized, where is the analogy in the processes upon which the different modifications are supposed to depend? (P. 188—192.) The argument of Dr. Williams, as stated in another place, (p. 323,) is worthy of all praise.

But again, how will the physical philosophers explain the formation of purulent globules? This can only be done by referring them, as in the case of all other globules, to the organic actions, or by another hypothesis in collision with the laws of life. Or, if "softened tubercle" be merely a physical dissolution of dead, inorganic matter, how happens it that the product is not more diversified? Why always so much like pus? Is this the exactness of chemical or other physical agents, where the elements are numerous? (Pp. 198—200;

(1) Davies, on Diseases of the Lungs, &c. p. 277.
(2) Carswell's Illustrations in Morbid Anatomy, Art. Tubercle.
(3) Davies, on Diseases of the Lungs and Heart, p. 257.
(4) Ut cit. vol. i. pp. 155, 158, 178, 182.

and Vol. I. 57, 66, 581—604.) Of the same character, also, is the philosophy which imputes the disappearance of the bronchial ramifications in tuberculous cavities, either to a physical degeneration, or to a transformation into the tuberculous matter itself. M. Louis remarks that,

"It might be thought that this destruction arises from the transformation of the air tubes into the gray or tubercular matter; but this supposition appears to us *very doubtful, from the fact* that we have never *seen* this tranformation. It is, *then*, highly probable that this destruction takes place by absorption." (1)

But, suppose it were to be imagined that such a phenomon had been "seen;" shall we give up to this species of evidence the fundamental principles of life?

Again, we find tubercles converted into carbonate of lime; and here, as in the case of phlebolites, the advantage of analogy is overlooked. "Sebastian thinks that the tubercle may be directly changed into a chalky matter." (2) And so do Stokes, (3) and others. But whence comes the chalk? Or, is Alber's explanation a more probable one, that "the tubercle is first absorbed, and the chalky matter afterwards deposited?" Certainly, then, by an interstitial deposition, and, therefore, interstitial absorption; all of which requires organization and vital actions as much as the conversion of cartilage into bone, or the concentric layers of the oyster shell, or of phlebolites. But we have already gone over the philosophy of this subject. (P. 353—361.) It is a part of the same chain of evidences, as that the "gray semi-transparent granulations are transformed into tubercles," and which, according to Laennec, and Louis, "form the first stage of tubercle, through which the latter must pass before assuming those characters which are peculiar to them." (4) And, it is not a little remarkable, that M. Louis succeeded in injecting the "grey, semi-transparent" substances, or the tuberculous matter at its *incipient* stage. (5) Nor should the analogy, as to secretion, as well as the importance of the direct fact, be neglected, that *blood* has been often found in tuberculous cavities, "both in a fluid and coagulated state;" as is, also, "a cheesy matter," and "*calculous* concretions." True, M. Andral asks "are these concretions formed *there*, or are they *detached* from the surrounding parts of the pulmonary tissue?" (6)

Finally, how shall we explain, upon any physical principles, the interstitial growth of tubercle as allowed by Laennec and others? (Vol. I. p. 594—596.)

But, if we must come to the positive demonstration that tubercle is organized, before its diversified vital phenomena can be allowed as a proof of the fact; or its inflammatory origin, which is more variously denoted, can be admitted, we would again refer to the statement by M. Louis, that he has injected tubercles through "the pulmonary artery," in what he regards as their forming stage. The like has been done by Dr. M'Cartney; (7) And more recently an "experimentum crucis," the *argumentum ad sensus*, has been exhibited by nature herself. Dr. Kingston relates seven cases, "in which *great numbers* of pulmonary tubercles of the ordinary kind were distinctly seen to be traversed by red vessels;" and

(1) On Phthisis, p. 35, s. 35.
(2) British and Foreign Med. Rev. No. 7, p. 216.
(3) On Diseases of the Chest.
(4) Louis, on Phthisis, pp. 5, 6.
(5) Ibid. p. 331, s. 380.
(6) Andral's Patholog. Anat. vol. ii. p. 340; and Clinique Méd. t. iv.
(7) Carmichæl's Essay, ut cit. p. 52.

Andral allows that Kuhn has used the microscope to the same effect.(1) In Kingston's cases,

"In many instances they might be traced from the centre to the circumference of tubercles, *whence* they were sometimes *seen* to extend into the *adjacent pulmonary parenchyma*, or to communicate with the vessels in the neighbourhood." (2)

"The results of Kingston's investigations," says the Lon. Medical Gazette, "throw *light* upon several practical questions; in particular they confirm the opinion of those who, while they allow the necessity of a predisposition, think that a lingering inflammation, or long continued irritation of the air cells, and minute bronchi, excite the disease in many, who, *though the diathesis existed, would otherwise have escaped.*" (3)

Still, all that we have yet stated appears not to be enough. As to Dr. Kingston, an able writer observes, that, "so great appears to be the facility with which Dr. Kingston can detect these red lines in tubercles, that one can hardly help feeling surprise, after reading his paper, that their vascularity should be a subject of dispute at the present day. And yet it is true that not only we ourselves, but the most eminent and experienced pathologists of past and present times, have failed in observing what seems to have been so readily discovered by Dr. Kingston. Is there not some fallacy in this?" (4) (P. 577.) But, have not the statements of Mr. Gulliver in relation to the existence of pus in the blood been received as a valuable accession to the humoral pathology? (Vol. I. pp. 388, 686, *note.*)

We know not that we can advance the knowledge of this subject by any farther testimony, though we have other and very conclusive facts lying around us. We cannot, however, leave it without quoting the observation of Professor Gross, that, —

"I have examined not less than *six specimens of organized tubercles*, one occurring in the kidney, two in the spleen, one in the peritoneum, and two in the lungs. They were of the *miliary* kind, and *numerous vessels, loaded with fluid blood*, could be *seen* shooting into them in *every possible direction*, many of them penetrating a considerable distance into their substance." (5)

Still, we grant that there may be "some fallacy in this;" though Professor Gross took the precaution to have *witnesses* present at his dissections. Should Gross', or Louis's testimony prevail, we shall then have detected nature beginning the work of organization almost at the moment the tuberculous matter is deposited; and what is not less important, in various parts of the body.

If it be objected to the inflammatory origin of tubercle, that it is found in the new-born infant, it is impossible to deny its origin in inflammation; since infants have been born with pneumonia and bronchitis, and only a low degree of this action is necessary to various formations, and especially, as we have seen, to the tuberculous in adults. (P. 319.) Tubercle, however, in new-born infants, is exceedingly rare; so rare, indeed, that Denis maintains that such as have reported its existence have been mistaken. In the multitude of his dissections he has never seen them until the age of five or six months. Such,

(1) Notes to Laennec, Trait. de l'Auscult. Médiate.
(2) Lon. Medico-Chir. Trans. vol. xx. p. 310, 1837.
(3) Dec. 10, 1836.
(4) British and Foreign Med. Rev. July, 1837, p. 152.
(5) Patholog. Anat. vol. i. p. 164.

also, was the experience of Velpeau, and Breschet; the latter of whom was surgeon to *Enfans Trouvés*. We have also the testimony of Magendie, that he always discovered marks of inflammation attending the tubercles of infants, and this even in their earliest stage. The *quicksilver* experiment should not be forgotten. (P. 624, and 172, *note*.)

We have thus protracted this inquiry, from the magnitude of its importance, and from the fact that speculative views as to the pathology of tubercle have, as in typhus, and other congestive fevers, (Vol. I. p. 279,) swayed the practical habits of many, more than an observation of nature and the results of treatment. But, should it be even ultimately conceded that the disease is inflammatory, it can scarcely be expected that the antiphlogistic method will not be strenuously opposed by that powerful class who stimultate in congestive fevers, erysipelas, &c., or who rely upon tartarized antimony in pneumonia, and admonish us to be sparing of the precious fluid and "support the strength." (Vol. I. pp. 279—309.)

We shall now examine one of the strong cases of the anatomical school, for the purpose of fairly laying before the reader the method of reasoning which is brought against the conclusions which we have drawn from the foregoing facts. We take the principal example of the most acknowledged authority upon the non-inflammatory nature of tuberculous phthisis. Our author in referring to a former case, remarks that, —

"These morbid productions," (tubercles and tuberculous matter) "were certainly not the result of bronchial inflammation. To support the contrary idea, we must prove that bronchitis of *twenty-four hours' duration* could have produced tuberculous deposit, &c. But, *we respect the reader too much to suppose him supporting such an opinion*, or other equally improbable suppositions, *and shall consider the fact of phthisis being developed independently of* ALL *inflammation, as satisfactorily as possible demonstrated in the instance before us.*" (1)

The personal appeal to the "reader" led us to turn back 90 pages where the case is related from which the foregoing important inductions are so summarily drawn; especially, too, as our author is *professedly* adverse to generalizations which are not founded upon a great number of "rigorous facts." We confess that we were not a little surprised to find that in this *test* case, the patient

"At the commencement was attacked by rigors with shiverings, followed by heat and perspiration. The rigors were repeatedly renewed; the heat increased, and the thirst became urgent. Complete loss of appetite and frequent constipation. Lastly, on the *tenth day* from the commencement, there were slight cough and expectoration." April 30th, (11th day) "respiration 44, cough frequent; sputa, semi opaque, rather greenish; pains in the middle of the sternum and under the left clavicle; pulse, 103; heat intense; night sweats; tongue red on edges; mouth dry; thirst urgent. May 1st. — Increased uneasiness; pain in right side of the chest; expectoration copious. A *poultice* to the painful part. May 2d. No remarkable change. From this period to the 19th of May, the day of her death, the progress of her disease was rapid and regular. The respiration, thoracic, frequent, 49 to 60 in a minute. Cough very violent, especially during the night. Expectoration more or less copious; heat, much elevated and dry; constant night sweats until the 12th;" and so on. (2)

(1) Louis, on Phthisis, p. 442, s. 474.

(2) Ibid. p. 353 — 354, Obs. 33.

Now, what becomes of the assertion, that, in the foregoing case, "we must prove that BRONCHITIS of twenty-four hours' duration could have produced tuberculous deposit," &c.; since it is admitted that fever "began at the commencement," and ten days before the patient entered the hospital; whilst the dissection was not made till "the 35th day of her illness, and 25th from the *commencement of the cough*"?

Let us hear our author on another occasion. "The *softening* of the mucous membrane was, in a great number of instances, an evident result of inflammation, which only lasted a few days previous to the death of the patient." (1) And what will our author say as to the structural lesions of the glands of Peyer, and the degeneration of the intestinal tube in the Asiatic cholera, which take place in less than 24 hours? Aye, too, and of the glands of Peyer in "the typhoid fever;" which he affirms to have been affected in their organization from its very invasion in all the cases? What shall we not say of our author's induction, when it is as well known to our author as to any other, that no very violent degree of inflammation in the mucous tissue of the trachea may produce an effusion of lymph, or albuminous matter, that shall have become concreted into a thick tube in less than 12 hours from the invasion of disease? And may we not ask, whether the judgment of our author upon his "reader's" understanding has not had some influence in obtaining a submission to his generalizations of a solitary fact? (See *Essay* on the *Writings*, &c.) But the foregoing is not all, since our author affirms in another place, that, "*Tuberculous matter is capable of very rapid development*, as we shall hereafter prove," &c. (2) (Pp. 310, 320, and Vol. I. p. 480.)

But, let us look at the morbid anatomy in the foregoing non-inflammatory *test* case. The state of the pulmonary mucous membrane is accidentally omitted; but "the right lung was *universally adherent*;" and here was the *principal seat* of the tubercles. "It was *transformed*, at its base, into a mass of tuberculous matter of a *pale rose tint*." Here, "it was *perforated* by a kind of anfractuous canal," and "its tissue was slightly *engorged*."

Our author, in his comments, says, "the *violence of the symptoms* is as remarkable as the rapid progress of the affection;" but, in opposition, also, to the foregoing lesions, our author observes, that, "notwithstanding the *extreme rapidity* of the *tuberculous* development, there were *scarcely any traces* of inflammation in the surrounding tissues."!!

Let it also be remembered, that the foregoing case appears to be regarded by our author, as supplying the strongest facts by which he pushes the conclusion, that,—

"Out of 123 cases, six, or one twentieth of the whole were direct illustrations of the production of phthisis, *independently of all inflammatory action*, either in the substance of the lung, or in the bronchial mucous membrane;" and "that we think we have proved, that in one twelfth part of our cases, pulmonary tubercles were developed *independently of all inflammation, either of the substance of the lung*, pleuræ or bronchia." (3)

Some of the special *anatomical* objections of our author to inflammation as a necessary cause of ulceration, and other common results of that mode of action, will be stated hereafter; which the reader should not fail to consider in connec-

(1) Ibid. s. 135. (2) Ibid. s. 151. (3) Ibid. s. 475, 477.

tion with the present subject; as well as to remark how our author, when employed in subverting any great fundamental law, draws his conclusion even from a "solitary" and accidental lesion of structure.

And now for the treatment of the foregoing "rapid" and "*violent*" case; which should be carried to our account between the Hippocratic and Anatomical schools.

The patient was an inmate of La Charité 21 days. "April 30th. Twelve leeches to the labia; acidulated infusion of violets for drink; solution of gum syrup; two emollient enemata." "May 1st. A poultice to the painful part," (the chest.) "May 2d. Blister to the chest." "The blister ceased to suppurate some days before death. She continued the same drinks; emollient enemata were frequently ordered; and during the last week, some warm baths."

Of the 50 cases of phthisis that are given in detail, the subjects of Obs. 6, 9, 10, were each bled to the extent of ℥ viii.; of Obs. 36 and 38, each ℥ x,; of Obs. 44, ℥.v; of 15, 42, each 3 times; and in the 1st Obs. marked 10, v. s. was employed 7 times. The bloodletting was generally resorted to for the purpose of arresting hæmoptysis. (1)

Perhaps a fair average example of the treatment may be found in a severe case, Obs. 3. It consisted of "sweetened infusion of violets; gum potion; flaxseed tea enemata; emollient fomentations upon hypogastrium; a half-julep; yolk of egg mixed with water." "In addition to this prescription, an infusion of the *triticum repens* with the syrup of the *five roots*, and aromatic fumigations *under the bed clothes* with juniper berries, were had recourse to. For the diarrhœa, the white decoction of *quince syrup*, some *diascordium* and a grain of opium, with a narcotic enema afterwards, were prescribed." This patient was in the hospital 28 days.

The reader will be compensated for his trouble, if he will examine the coincidence in the foregoing case with the treatment of Madame de Stael, by a number of the most distinguished of the French medical philosophers, as reported by M. Portal, and which, should we have room, we may present in our Essay on the Hippocratic and Anatomical Schools. But, we are at present most interested, in this particular, as we shall be in another place, in laying before the reader the ground of M. Louis' conclusion that "medicine is now in its infancy."

Finally, may we not, by the showing of the anatomical school, safely contend that tubercle is of inflammatory origin; and shall we not unite with others in endeavouring to expose the fallacy of that stimulant, or that negative treatment, of the early stages of phthisis, which has been proclaimed from the dead-house?

Having just met with the following experiment which is remarkable for the *a priori* conclusions upon which it is founded, if not objectionable in other respects, we shall give it a place.

"*On the Prevention of Tubercles.* — Two years ago M. Coster placed a number of dogs, rabbits, &c. in the circumstances most favourable to the develop-

(1) See Ibid. s. 488.

Chomel was the principal; but our author was clearly the acting physician. Indeed, his particularity everywhere evinces this, and much of the reputation of his work rests upon this supposition.

ment of the *scrofulous diathesis*, (p. 624; and Vol. I. p. 517, *note* 4.) Thus, many of the unfortunate animals were shut up in dungeons, without light, incapable of moving, and exposed to a moist cold by means of wet sponges which were hung up in the cages. Some of the animals placed in these conditions, were fed on their ordinary diet; others were fed with *ferruginous* bread, containing $\frac{1}{2}$ oz. of carbonate of iron to the pound. All the former became ill, the greater part tuberculous, but *not one* of those fed on the bread containing iron presented a trace of tubercles." (1)

How far this feeding of dogs with bread and iron comports with Magendie's experiments of feeding them with bread alone, and how far the inductions may be carried up to man, we have already endeavoured to show; especially in our first volume, pages 696 — 698, and in our Essay on Animal Heat.

(1) Boston Medical and Surgical Journal, May 6th, 1840, p. 211; from *Bul. de l'Acad.*

APPENDIX VI. TO THE PHILOSOPHY OF VENOUS CONGESTION.

(See p. 376.)

MELANOSIS, ANIMAL PIGMENTS, AND THE PHILOSOPHY OF ADVENTITIOUS GROWTHS.

The object of this appendix is to consider another of those instances of false inductions which mar the philosophy of life, whenever it is subjected to the principles which obtain in physical sciences. We offer it, therefore, as a subject possessing extensive connections, and involving in its train the whole philosophy of the vital powers. It is only in this sense that we feel any interest in the discussion.

"There is," says Dr. Hodgkin, "no organ so liable to be affected with melanosis as the eye; and I cannot help strongly suspecting, that it is the *natural and healthy production of black pigment* performed by the choroid coat of this organ, which is the *chief cause of this predisposition.*" (1)

Now, we do not believe that our excellent author would have reached this conclusion, had he "employed the term vitality, for want of a better," in the acceptation which he derides. (Vol. I. p. 48; and Vol. II. p. 124—128.) One of our principal objections applies to the fact, that the black pigment is "*a natural and healthy production.*" We do not comprehend how "a natural and healthy production" should constitute "a chief cause of predisposition" to malignant disease; nor do we see that the pigment because it is *black* should be in favour of our author's opinion, since melanosis is *black* in every other part of the body.

The natural secretions are constantly liable to alterations and redundancies; but, this arises from an antecedent disease of the solids, to which the former in no way contributes, nor do they form any part of the disease, or the predisposition to it; no, not even in their "*natural and healthy*" state. We know, however, that this doctrine is held, and we cannot expose its merits better, than by a reply which was given to a celebrated professor in this city, who was employed in propagating the humoral pathology, and as we had it from the student himself. The professor, at a catechetical review of his preceding lecture, reproved the student for saying that he did not believe that the water in dropsy, consti-

(1) Lectures on the Morbid Anatomy of the Serous and Mucous Membranes, vol. i. p. 262, London, 1836.

tuted the disease. "Well, sir," replied the latter, "then you must admit that when you have evacuated the fluid, you have transferred the disease to the pot." (Pp. 221, 613.) So, in the same way, if we remove a melanotic formation, we shall easily satisfy ourselves, that neither "a pigment" has contributed to the "predisposition," nor that the disease is constituted by any of its physical results, as soon as we see the same consequences returning. It is admitted, indeed, by Dr. Hodgkin, that the "production of the black pigment is the least formidable part of the complaint;" and he speaks of a "constitutional taint," in a general sense, (1) which, if allowed, would quite preclude the necessity of the supposition, that "the natural and healthy black pigment of the choroid coat is *the chief cause of the predisposition*" in "melanosis of the eye."

Whether there be a morbid production of the natural pigment, in melanosis of the eye, or whether the pigment be something wholly different as in other parts, is more than we can say; though we are strongly inclined to lean to the side of those strong and numerous analogies which are supplied by new formations in disease.

We shall take one example more of the nature of the foregoing, from the author to whom we have appropriated this appendix, since he can so well afford to spare something to criticism for the benefit of the cause we advocate. Our able author, in an elaborate analysis of the nature of the colour of tissues in health and disease, reasonably supposes that the colouring matter is derived from the blood; though we think it not so clear that the "pigments are deduced by an easy transition from the colouring matter of the blood." We object, also, to the nature of the proof, and to his demonstration, not only as irrelevant, but as confounding the powers and actions of living matter, and their results, with those of dead matter. The "illustration" consists in "the familiar instance of a bruise."

"Within a very limited space," he says, "and in a very short time, we see a variety of colours produced; whence we may infer both the near relation of the colouring principles, and the simplicity of the apparatus essential to their production." (2)

Here, we maintain, is a radical defect in physiological principles. The colouring matter of our tissues, &c., is a secretion, whilst the mechanical effusion of blood is not. The former is the product of a purely vital process, and may require a most elaborate apparatus. Extravasated blood, therefore, can prove nothing whatever in relation to the vital effect. And again, the changes of colour in extravasated blood do not depend upon a vital process, excepting as organized matter may contribute something as it carries off the different parts of the coagulated mass. But here it is obvious that there is not the slightest affinity with the vital secretion. In the former case, too, the changes of colour are only taking place in a mass of dead matter, whilst in the latter, the colouring matter is a new formation out of what is recognised as living blood; always uniform, or nearly so, in each tissue, or in diseases where morbid action is the same, and not varying in tint like extravasated blood. Its diversities, too, in different parts, connected with their individual uniformity, and the differences in the organization and vital properties of the secreting tissues, and the precise variations of colour in disease, connected also with all that is known of vital

(1) Ibid. p. 338.

(2) Ibid. p. 298; also, 307, 309, 318.

products, show that the substance is essentially different from any component part of the blood. (Vol. I. p. 56 — 57, &c.) If we look at the liver, and the colour of the bile, we shall find that the supposed "simplicity of the apparatus" cannot be sustained. That the colour depends, in this instance, upon the complexity of the organ, is shown by the variations which the colour undergoes in common with the other products, whenever the functions of the organ are disturbed. Though this do not relate to the colour of the tissues, it is an analogical fact, and proves that the colour of secreted products depends upon the same laws and actions that generate other products. Indeed, our author embraces milk, &c. in his general plan. The blood of some individuals has undergone a temporary change to white; but does this make any difference in the colour of their tissues? Why are some negroes born white, and why does the skin of the black man sometimes become white? Why does the hair become gray, and other tissues change their colour according to manifest variations in their vital properties? Why did the skin of the female, to whom our author alludes, become black in this reprieved criminal, in consequence of "the shock she had received" in being condemned to the guillotine? "The deposit of pigment was in that part of the tissue in which its presence is natural in the dark races of mankind." (1) What has the colouring matter of the blood to do with these phenomena; or, are they analogous to the changes of colour which extravasated blood undergoes? or, why have not some animals green, or blue in their tissues, according with the foregoing changes?

From these considerations we might pass to the diversified colours of the hair of man and animals, the feathers of birds, &c., always the same in each species under ordinary circumstances, but liable to great variations from unusual influences, whether external or internal; whilst these varieties may be perpetuated. We might dwell, with still greater effect upon the varied hues of the white blooded animals; and could possibly derive some help from the leaves, the flowers, &c., of the vegetable kingdom. "The transition from the colouring matter of the blood" would not be so "easy" in these cases.

Our author states that, "the pigment on which both the colour and the name of melanosis depend, is, with apparent accuracy, considered as the same with that which is common and natural to various parts, both in man and animals." "I suggested that this black matter depended on a highly venous state of the blood, induced by prolonged stay in the veins." (2)

But, does this correspond with our author's previous statement, that "it is the *natural and healthy* production of black pigment *performed* by the choroid coat of the eye, which is the chief cause of its predisposition to melanosis;" and where, too, the disease is of more frequent occurrence than in any other part? Is it not manifest that our author's philosophy turns mainly about the *colour*, — pigment or blood being equally the cause, so only both are *black*. If natural pigment or blood, why are they associated in such a peculiar manner with a specific condition of disease? But, our author is dealing with a series of analogies; and allowing him, therefore, the last imputed cause of the colour of melanosis, will it equally follow that the colour of the African, or that of any tissue which has naturally a black pigment, is owing to "a highly venous state of the blood, induced by a prolonged stay in the veins"? We think, on the contrary,

(1) Ibid. p. 306. (2) Ibid. p. 333.

that this is one of the numerous exemplifications of the truth, that no philosophy can stand which is not based upon the appropriate laws of nature.

Leaving out of the question, however, the "black pigment of the choroid coat as the predisposing cause of melanosis of the eye," it is due to our author to consider his specific proof of the other cause, viz. the identity of the colouring part of melanosis and venous blood. He gives us the authority of the chemists, who appear to have decided upon it with their usual precision and harmony, in matters of this nature. The reader must judge how far it sustains our author's conclusions. Thus: —

"The black matter of melanosis has been repeatedly subjected to chemical analysis. Socquet states, that the colouring matter is insoluble in alcohol; and that it consists of carbon dissolved in some animal fluid, from which it cannot be separated by any known chemical process. Hurtrel d'Arboval gives as its constituents, phosphate of lime, a portion of iron, and a black matter insoluble in alcohol. Lassaigne, from the analysis of a specimen of melanosis taken from a horse, gives: 1. A coloured fibrous matter. 2. A blackish colouring matter, soluble in dilute sulphuric acid, and also in a solution of sub-carbonate of soda, to which it gives a tinge of red. 3. A small quantity of albumen. 4. Muriate and sub-carbonate of soda, phosphate of lime, and oxide of iron. According to Barruel and Breschet, *it bears a great resemblance to the black pigment of the eye*, as analyzed by Berzelius." (1)

Now to the main point of proof. Vauquelin, Berzelius, and Brande, found the colouring matter of blood *insoluble* in all the *acids* which they employed, *sulphuric* being one; whilst *alcohol* converts it into an adipocerous matter. Le Canu, however, says it is *insoluble* by *alcohol*, and that *sulphuric*, nitric, and muriatic acids entirely *decompose* it. (2) It is *not dissolved* by the alkaline carbonates. This, therefore, would establish, by our author's own showing, an absolute distinction betwixt the colouring matter of blood and that of melanosis. Barruel's and Breschet's analysis of the black pigment of the eye is equally adverse to our author's construction.

Our author frequently adverts to the colour of tissues which is constituted by the blood itself. But we have no such reference in our remarks. We confine ourselves to the pigments, natural and morbid, as having no analogy with the colours that are constituted by the blood. The pigments are new formations, and can derive no light, as to their nature, from the colour of the blood.

Before parting with our able author, we will call his attention once more to the evils which arise from "employing the term vitality without any connection with the mystification which sometimes attends its use." (3) Our author, in entering upon the philosophy of the "origin of adventitious growths," remarks, that "the following, from the effects which are within our observation, appears to be the kind of operation which takes place." Our author allows, what is plain enough, its hypothetical nature. Nevertheless, our author thinks that the hypothesis "seems to possess *the conditions required*," and "will serve to render *more intelligible* the views which I have detailed;" whilst, it will be seen, that he comes at last to the conclusion that the "principle" involved in the explanation "is nearly or quite the same" as that which is concerned in "adventitious growths."

(1) Ibid. p. 337. (2) Leçons Chimiques sur le Sang. Hum. p. 30, 1837. (3) Ibid. p. 272.

Preliminary to the exposition, he controverts the opinion that they "originate in extravasated blood." "The correctness of this I am disposed to dispute." Our author next derives some "hints from the granulations of a common ulcer." But these are only hints, and we are indebted for the true philosophy to "an accidental circumstance which arrested my (our author's) attention, and appeared to throw some light on productions presenting the compound adventitious serous cysts."

From what we have seen of our author's views of vitality, the reader will have concluded that, in conformity with the usage of his school, our author lays his philosophy in another department of nature. Accordingly we read, that, —

"It happened that a fluid, perfectly transparent, and having a small quantity of mucus diffused through it, dropped into some spirit, of sufficient strength to precipitate or coagulate the mucus, and prevent its immediate admixture or diffusion through the spirit. As the drop descended towards the bottom of the vessel, its parts tended to spread and separate from each other; and, as the mucus became opaque in its coagulation, the falling drop was distinctly visible in a BBANCHING FORM, BEARING THE CLOSEST RESEMBLANCE TO THE RAMIFYING BRANCHES OF PEDUNCULATED CYSTS so frequently found in the cysts constituting ovarian dropsy. The resemblance was too striking to be overlooked, and the RATIONALE of the figure of the falling drop was sufficiently obvious. Each of the several portions into which the drop was divided, after it entered the spirits, met of course with some resistance as it descended through the spirit. Its slight degree of coagulation would favour this effect, and would produce the rounded dilated extremities of the branch, by which, in consequence of the same coagulability, it continued connected with the other portions into which the original drop was divided." "We may see the appearance which I have just described *on our plates at table;* having added a little CREAM TO FRUIT-TART, it makes its way through the narrow straits which may allow it to pass between two pieces of fruit, *into the transparent juice below.*

"Analogy drawn from the diffusion of mucus in spirit."

"In these two rough and accidental experiments, although the form produced is *very striking*, it is extremely evanescent, and is *scarcely a moment in existence; but if* the exterior of the fluid, which assumes the branching form, became a continuous coherent *coagulum, as in the case of effused plastic lymph*, a permanent *bunch of cysts would have been formed.* Let me not be misunderstood, as wishing it to be inferred, from what I have just said, that the bunches of cysts, entering into the composition of adventitious structures of animals, are thus instantaneously formed by the coagulation of the exterior of a quantity of coagulable lymph *suddenly oozing through a small orifice.* The *process, though very similar*, is doubtless gradual; THE PRINCIPLE I BELIEVE TO BE NEARLY OR QUITE THE SAME." (1) (Pp. 10, 168, *note; Billing;* and 117, *note* 4.)

We shall only say in addition to our prefatory remarks, that the foregoing experiments have less relation to "adventitious growths," which depend upon the vital processes of organized tissues, than they have to the ebbing and flowing of the tide, or to the annual revolution of the earth; to which, indeed, the experiments are analogous in "principle." (See p. 325, *note.*)

What relation has alcohol, or fruit-tart, to organized tissues and their ac-

(1) Ibid. pp. 358, 359, 360. — Our Italics, &c. see p. 145, *note* 3.

tions; or does the dead, dissolved mucus, or cream, &c. derive from the tart, or alcohol, anything like that condition which is bestowed upon secreted matter in its conversion into organized "adventitious growths"?

We have intimated that the popular problem respecting the origin of the colour of the black man cannot he explained. (P. 128.) It has been considered of easy solution by referring the colour to a *lusus naturæ*, which has been perpetuated. But, this hypothesis requires an improbability more difficult to admit, than the subject which it is designed to expound is otherwise paradoxical. It supposes that a male and female were not only the subjects of such *lusus* at the same period of time, or became changed in colour from some other extraordinary cause, but that the coincidence must have probably happened in the same country, and that they paired in consequence of these coincidences. An individual could have never propagated the peculiarity; for his or her descendents being mulattoes, the partial colour, at most, could only have been perpetuated; whilst even this is highly improbable, since the tendency inclines, amongst mulattoes who intermarry, to the complexion of the original white man. The same objection is applicable to the hypothesis which supposes Cain, or Ham, to have been the progenitor.

COMPARATIVE MERITS OF THE HIPPOCRATIC AND ANATOMICAL SCHOOLS.

"The most important duty of the physician in the treatment of disease, inculcated by Hippocrates, and observed by his followers of the present day, is to watch the operations of this nature, (φυσις, or the Hunterian principle of vitality,) *with the view of promoting those actions which appear to be salutary, and of checking, or suppressing those which appear to be hurtful.* According to this doctrine, the chief study of a modical man should be semiology, &c. The tendency of such precepts is to induce *great caution* in the treatment of diseases. *Much is left to the superintending influence of Nature;* and violent and very energetic remedies are not *generally* resorted to, *unless the symptoms denote a strongly hypersthenic excitement.* The 'médecine expectante' is characteristic of the Hippocratists, and is antipodal to the 'médecine héroique ou abattante' of the anatomical physicians. These latter are styled the Whigs of medicine; and well do the Hippocratists deserve to be called the good old Tories. Their inviolable attachment to antiquity, their love for whatever is the established order of things, their dread of innovations, their opposition to all change, and even to many improvements in medical science, such as the use of auscultation, their preference of mild to violent remedies, and their implicit credence in the salutary and self-correcting operations of the system itself, well entitle them to this appellation. It is worthy of notice that most of the medical Tories in France, and, we might add in England, are at the same time political Tories."

"M. Andral is acknowledged by the profession to be the first pathologist in the world, and M. M. Chomel and Louis have already earned a reputation only second to his." "Instead of *localizing* the primary seat of fevers, &c. in the brain, or in the stomach and *intestines*, &c. they have wisely taught us that these great organs are, or at least may be, involved in the morbid action simultaneously," &c. "*And they have laid a foundation for that rational Humorism which has sprung up of late years.*" (1) — Medico-Chirurgical Review, *July*, 1836, *pp.* 226, 227.

"Pathological Anatomy is incontestably the surest guide of the physician, either in the recognition of maladies, or in the cure of those which are susceptible of cure."— Laennec.

"Anatomy is the strongest support of Pathology."— Louis *on Phthisis, Preface, p. lxx.*

"The favourite maxim of the Pathologico-Anatomists is that saying of Bichat, '*what is observation if we are ignorant of the seat of disease?*' This is the sentiment of an anatomist, but not of a physician."— Stokes' *Theory and Practice of Medicine, p.* 11, 1838.

"We must infer that it is in this last lesion, (the *glands of Peyer,*) *and not in any other*, that we must look for the *cause* of the *delirium*, and more especially of the *somnolency*" in typhoid fever. "All the facts seem to prove that the delirium of the typhoid affection cannot be explained by any appreciable alteration of the brain."

"What importance is it, however, with respect to *effects*, for us to know *what is the* Cause of trouble in our functions, when this trouble is of a *serious* nature?" (2) — Louis, *on Typhoid Fever, Vol. II. pp.* 145, 132, 142, 144; or the Anatomical School.

(1) Here, as elsewhere, the *italics*, &c. are generally ours.

(2) Qu' *importe* d 'ailleurs, relativement aux effets, la cause du trouble de nos fonctions, quand ce trouble est considerable?"— *Recherches, &c. de Fievre Typhoide, &c. t. 2, p.* 171.

"I shall employ the term *hyperæmia* to designate the increased quantity of blood, *without any reference to the* CAUSE *which produces the accumulation.*"—ANDRAL. *Pathological Anatomy, Vol. I. p.* 10.

"It is not to be imagined that he should know the remedies of diseases, who does not know their *original* CAUSES, though some think it *unimportant* what is the *cause.* — CELSUS. *L.* 1, *Præf. pp.* 7, 17.

"He who proceeds upon a *rational* method, though things do not turn out as they should, must not immediately run from one medicine to another, so long as that CAUSE remains which was there at first." — HIPPOCRATES. *Aph. s.* 2, *a.* 52.

"One hundred typhoid patients were treated by M. Louis, simply with emollient ptisans, Seltzer water, and linseed enemata, — a mode of treatment scarcely differing from pure expectancy." "His cases were of the severest kind, accompanied with intestinal hemorrhage to a considerable extent, the formation of large eschars, &c."—BRITISH AND FOREIGN MEDICAL REVIEW, *Jan.* 1840, *p.* 218.— (*See our Vol. I. p.* 305.)

"The only satisfactory proof of inflammation having existed in the internal structures of the body is afforded by the detection of those changes, such as thickening, and unnatural adhesions and secretions, which we know to be the invariable (?) results or products of a continuance of this morbid affection."—CURLING *on Tetanus, p.* 33.

"In this country, (Great Britain,) few would be disposed to admit that inflammation had existed, unless some of its known *products* were brought forward as proofs."— BRITISH AND FOREIGN MEDICAL REVIEW, *July*, 1837, *p.* 138.

"It was a false taste in the opinion of Lord Shaftsbury, which throws itself upon that which immediately strikes the senses, rather than on that which might be likely to interest the understanding after a proper examination."— ZIMMERMANN'S *Experience in Physic.* (*See our Preface.*)

"Nothing is more simple minded, (*stultius*) than to suppose that a man has been in his life time as he is found when he is dying, or already dead."— CELSUS, *L.* 1, *Præf.*

"We shall never discover the sources of disease by pottering in the dead-house."— SIR C. BELL. *Institutes of Surgery*, 183.

"Nunquam aliud Natura, aliud Sapientia dicit."— JUVENAL.

"Res corporeæ admirabili quadam, eaque æterna, et constanti regula gubernantur."— BAGLIVI. *Prax. Med. c.* 1.

Hippocrates, (non Asclepiades) "officium esse medici docet, ut tuto, ut celeriter, ut jucunde curet." — CELSUS. *L.* 3, *s.* 4.

"Ad extremos morbos exacte extremæ curationes optimæ sunt."— HIP. *Aph. s.* 1, *a.* 6.

"In acutis morbis venam secabis, et si vehemens appareat morbus, qui ipsum habeant, *robur ipsis* affuerit."— IBID. *in Morb. Acut.*

"Quæ prodeunt, non multitudine æstismare oportet, sed quamdiu prodeant qualia oportet. Et ubi opus est, usque ad animi deliquium ducere oportet, et hoc facere, si sufficiat æger."—IBID. *Aph. s.* 1, *a.* 23.

"Qui in tranquilitate navem gubernantes delinquunt manifesti non fiunt; at si ventus magnus, ac tempestas ipsos deprehenderit, tunc conspicui fiunt palam omnibus hominibus, quod ignorantia, et erroris sui culpa navem perdiderunt. Sic sane etiam mali et plurim medici, quum curant homines nihil grave patientes, in quos siquis etiam maxime delinquat, nihil grave faciat. Sunt autem multi tales morbi, et multo sæpius quam graves hominibus accidunt. In talibus sane delinquentes, non fiunt manifesti idiotis homnibus. Ubi vero obtigerit magnus et rtis et periculosus morbus, tunc ipsorum delicta et ars omnibus in conspicuo sunt."— IBID. *De Vet. Med. v.* 134—142.

"A curante nullum malum accedere oportet, sufficiunt enim quæ ab ipsis morbis adsunt."— IBID *de Affectionibus, s.* 1, *v.* 125.

"Ubi cognoveris transmutare oportet, habitaque consideratione naturæ hominis, et ætatis, et specici, horæque, anni, et *modi* ipsius morbi, curationem facere, aliquando detrahendo, aliquando appo-

nendo, et quemadmodum jamdudum a me dictum est, et singulas ætates, et horas, et species, ac morbos respiciendo, tum in exhibendis pharmacis, tum in constituenda diæta."— IBID. *de Nat. Hominis; ver.* 185. (See Vol. I. p. 307.)

"At vero curationem optime fecerit, ubi prænoverit futuras affectiones."— IBID. *Prænot. v.* 6.

"Quisquis enim Medicinæ scientiam sibi vere comparare volet, eum his ducibus voti sui compotem fieri oportet, Natura, Doctrina, Loco studiis apto, Institutione a puero, Industria, et Tempore. Primum quidem igitur omnium natura opus est. Natura enim repugnante, irrita omnia fiunt. Si vero natura ad optima viam demonstret, artis Doctrina facile contingit." "Natura enim nostra velut ager est, dogmata præceptorum velut semina sunt." "Timiditas equidem impotentiam, audacia vero ignorantiam artis significat. Duo enim sunt, Scientia, et Opinio, quarum altera quidem scire facit, altera vero ignorare."— IBID. *Lex. Hip. v.* 10—31.

"Mihi quidem videtur principium corporis nullum esse, sed omnia similiter principium, et omnia finis."

"Si vero ad aliam aliquam partem incubuerit, illi ipsi morbum facit. Ita etiam reliquæ partes altera alteri morbum inducit. Et optimum fuerit sic curare ægrotos per has, quæ morbos faciunt."— IBID. *de Locis in Hom. v.* 1, 17.

"Altera enim pars ad alteram delegat in corpore."— IBID. *de Morb. Mul. l.* 1, *n.* 87, 45, 62, 92, &c. (See Vol. I. p. 655.)

"Quare artis potentiam magis ex merito admiremur, si quem ex clandestino morbo ægrum restituat, quam si impossibilibus, et desperatis opem ferre moliatur."— IBID. *de Arte, v.* 204.

"Proinde, justissime in multis terræ locis divinos honores etiam ipse consequutus est, et iisdem donariis cum Hercule, et Æsculapio ab Atheniensibus dignus factus est. Hic est sanitatis pater, hic servator, hic dolorum curator. *In summa, hic* DIVINÆ SCIENTIÆ PRINCEPS EST."— *Epis.* ARTAXERX.

Η πατρις φιλτατη βροτοις. — EURIPIDES.

"Be not surprised that we should have recommended so large a majority of foreign writers, nor accuse us of a want of attachment to our country. Such a charge we would resist with indignation. Though we be able to perform little or nothing, in respect to *patriotic feeling* we will allow ourselves second to none."— HODGKIN. *Lectures on the Serous and Mucous Membranes, p.* 22.

We shall scarcely be suspected in anything which we are about to say, of entertaining an unreserved opposition to morbid anatomy. We have already given evidence of the contrary in our obligations to this auxiliary to the science of disease. It is only what have appeared to us its misapplications, that will become the subject of our remarks. We intend only to carry out the opinion expressed in the following quotation from the distinguished Journal, whose contrast between the Hippocratists and Morbid-Anatomists stands at the head of this essay :

"While we make these concessions," says the Journal, "in favour of our own times, we must not shut our eyes against those fallacies and errors, which have been engendered of late years by the exclusive and extravagant attention to the morbid anatomy of diseases, as if the great object of medical science was only to expound the discoveries of the dissecting-room !" (1)

We have arranged many brief extracts in the form of mottos to this essay, that we might group together in a narrow space the leading tenets of the two great schools of medicine. Other ex-

(1) Medico-Chirurg. Rev. Oct. 1837, p. 453.

positions will occur as we advance in our inquiry; and we shall continue to derive them from such as preside at the portals of the temple. This will necessarily lead us to refer more to individual philosophers than we could desire. In doing a proper justice to the medical doctrines and habits of the age, it has been necessary to allow the several chiefs to speak for themselves; being in no respect disposed to suppress the weight of opinion against us. It has been our desire to follow the path of philosophy, as trodden by others; and we have endeavoured to keep this principle in view whenever we have attempted an interpretation of disease, or of physiological processes, which may have escaped the observation of others. Original views may be perfectly compatible with the most complete system of philosophy.

We have been liberal, here and elsewhere, in our quotations from Hippocrates, as we believe them to concern fundamental principles, which will endure till the order of nature is changed. We acknowledge him as our guide in the pursuit of truth, and we do but extend his views in the practical application of great elements. That he has come short of the entire philosophy of diseases, is certainly true; and it is only astonishing that he succeeded so well in drawing the great outlines. It should be also considered that the art of printing was then unknown, and that it is the province of extraordinary genius to unfold the laws of nature, to group her phenomena into general truths, and to leave to mankind at large their illustration by endless facts, and to weigh specifically the adaptation of principles to individual subjects. (Vol. I. p. 323.)

True, it is laid to the charge of Hippocrates that he sometimes confounded the nerves with the tendons, that he supposed the "popular phrase of the black bile passing into the blood to be *consonant with the science*," (Vol. I. p. 605,) and that he was even guilty of the absurdity of imagining that the brain discharged its "pituita" through the nostrils. But, these are amongst the rare blemishes upon his system of medical philosophy; and we think that we discover an offset in the existing chemical doctrines of life, in the mechanics of inflammation, fever, and venous congestions, in the spontaneous generation of animals out of inorganic matter, in the abyss of the humoral pathology. But, that this remarkable man had a general knowledge of healthy and morbid anatomy is evident on almost every page of his writings. That he has not narrated the lesions of structure was probably, in part, for reasons which we have assigned both here and else-

where; and the same reasons apply to those details of healthy anatomy which he has neglected, but without a knowledge of many of which he could have never attained that philosophy in relation to disease and its remedies which must remain forever the great basis of medical science.

We would also premise, that, as we shall go over much of the ground which appertains to our present subject, when we come to speak of the writings of the distinguished Louis, we shall reserve for that occasion many remarks which would otherwise have place in this essay. We have also at former times, especially in our Theories of Inflammation, and 7th Section of Venous Congestion, anticipated much of our present subject; though we must be indulged in occasional recapitulations.

The whole object of medical science is its practical application to the welfare of man; and this principle must be constantly considered in all our investigations. It must form the great test of the advantages of investigating disease according to the phenomena of the disease itself, or according to those lesions of structure and other results, which may be found after the disease has terminated; or, in other words, according to the Hippocratic or the Anatomical methods of observing Nature. We have already given in our Essay on Bloodletting many specific examples, as well as a general outline, of the practical habits of the two great schools; and in our *Appendix on Tubercle,* especially near its close, we opened a wide door where their relative merits may be broadly observed. And, coming to the writings of M. Louis, we shall make it a particular object to inquire how far both himself and Chomel are entitled to the rank which is awarded to them by the able writer who stands at the head of this essay, as we have already endeavoured to show with what consideration "M. Andral is allowed by the profession to be the first pathologist in the world."

We may here correct a misapprehension on the part of the writer to whom we have just referred, and which is equally made by the disciples of Andral and Louis. He ranks these philosophers and Chomel at the head of the *Eclectics*, whilst he places Broussais and Bouillaud in the front rank of the *Anatomicals.* Never was greater error. Whatever may be the dogmas of the two last, and we allow them to be great, they have but little connection with the school of morbid anatomy in its latest sense. They form an isolated sect, which had before occasionally sprung

up, but never made any great advances till the appearance of Broussais; and he is now fast in the wane. There was, however, great philosophy, though nothing new, in his doctrines which concerned inflammation, *per se*. It was the philosophy of Hunter. But he was excessively narrow in his limitations of disease, and profoundly dark as to idiopathic fever; quite as much so as Marcus, Ploquet, Mills, Pew, or Clutterbuck, each of whom located that disease in the brain. They are all of one school, and attained their doctrines not through the lesions of structure, but by blending much speculation with the phenomena of disease.

The greatest mistake, however, which has been made by our humorous analyst, is that of confounding Andral, Louis, and Chomel with the *Eclectics*, and even exalting them to the head of the sect. This, however, is a prevailing impression, and has contributed to the ascendancy of the school in which they lead. But we shall have abundantly shown, that they are the dead-anatomists of the day; and that, however they may pass in review the phenomena of disease, all their conclusions are founded upon *lesions of structure;* scarcely paying any regard to those morbid products which do not concern the structural lesions; at one time yielding a little to the signs of vascularity, and, at another, imputing the same lesion of organization to an "exactly opposite condition of disease," because the signs of vascularity may not have existed, or had disappeared before or after the extinction of life. These are they, indeed, (especially Louis,) who do not allow, or but very doubtingly, any consideration to morbid action; but place the essence of disease in absolute lesions of organization. Nay more, these are they, as we have variously proved by facts which admit of no contradiction, and by their own abundant showing, who have converted the temple of nature into a school of speculative philosophy; who have given an importance and dignity to those leeches upon society who profess to do all by cleansing the blood of its vitiated humours, and of whom it must be said, in solemn regard for the highest interests of man, that the whole of that philosophy, and its practical illustrations, appear to us but little else than a "meditation upon death." This is no rash, or hasty conclusion; nothing meant without a full sense of our responsibilities; nothing said or implied which has not its reasons fully and fairly set before the reader in their appropriate place in our several commentaries.

True, it may have been a fundamental and disinterested object to advance the interests and the dignity of science, and to improve the healing art; but, motives are without merit when they lead but to error. Nor is all the toil which may be stimulated by false philosophy, of any other consideration than that its evils may be in the ratio of its measure, and that it should form the ground of lamentation that it had not been applied to those luminous results, which, in the hands of others, will be gleaned from the accumulated and "miscellaneous assortment." All human inquiries, however wide from the truth, terminate in certain advantages. None have been more zealous or more indefatigable than the alchymists, and none approximate so nearly the modern school of morbid anatomists. The speculations and crudities of the former resulted in facts of no inconsiderable moment to chemistry and medicine, whilst those very facts increased the delusion and became the ground-work of farther hypotheses.

In relation to the therapeutical habits of this school, it is remarkable that our analyst, after satirizing the imputed imbecilities of the "Hippocratists," (though in conflict with his statement of their principles,) should have afforded us an exemplification in himself of the general tendency of his own school to pursue the plan of "*pure expectancy;*" as we have seen affirmed of M. Louis, in relation to the worst forms of "typhoid fever," upon the personal observation of an able reviewer who is gathered amongst our introductory quotations. For thus our commentator: —

"To such an extravagant extent has the depletory practice been carried, that an express 'formula' for venesection has of late years been laid down for the treatment of some diseases."

This objection is specially directed against Bouillaud and Broussais; and how far they were "extravagant," and our analyst in favour of "expectancy," we may learn from the fact, as stated by Bouillaud, that

"The medium quantity of blood which we draw, in individuals of *good frame* of body with *intense* articular rheumatism, is from four to five pounds, being the same as in pneumonia of medium extent and intensity;" "and in rheumatism without fever a single bleeding often suffices." (1) (Vol. I. p. 326.) As to Broussais, he did not often go beyond leeching the epigastric region.

We shall have little to say in this place as to the generalizations of M. M. Andral and Louis. They are abundantly examined in former essays, and in the article which we have appro-

(1) On Acute Articular Rheumatism, p. 58.

priated to the latter author. To them is especially due the credit of laying the foundation of many of the most fatal doctrines which have supplanted the philosophy that had been established upon the Hippocratic method of observing nature. No sooner had the former exploded inflammation as a disease not warranted by *cadaverous* lesions, or had spread before the world his incongruous medley of humoralism, (Vol. I. p. 626 — 632,) than it became a signal for strenuous cöoperation in more enlightened countries. Louis followed with a heavy artillery, and took possession of the field. So completely, indeed, is the work accomplished, that we now listen to annunciations like the following, as to repetitions of long acknowledged truths. Thus:—

"The blood," says Magendie, "traverses with ease the infinitely more minute tubes that abound in our tissues. There must be some particular conditions to facilitate its passage. What proves their existence is, that if certain alterations are effected in the composition of the blood, it stops, undergoes morbid changes, becomes extravasated and decomposed, and produces the various disorders which pathologists have vainly attempted to explain by the words *inflammation* and *irritation*. What sense, in truth, is there in applying the word *inflammation* to our organs? *Do our tissues actually* take fire? I confess I know of no such phenomena." (1) What! not of "human combustion?" (See Vol. I. pp. 397 — 398, 576 — 518.)

Then speedily followed in enlightened England the astonishing coincidence of a renunciation of the Hunterian doctrines of life, of inflammation, &c., as had happened to the great expositions of Bichat in France, and a full espousal of the humoral pathology. (See Essay on *Inflammation*.) This is the more remarkable, since a just devotion to national greatness and independence had hitherto given its best dignity to our mother country, and interposed a shield against those corruptions in philosophy which enabled her long line of sages to exalt her to the proudest eminence. At the present, however, we confine ourselves to errors in medicine, nor need we here recapitulate what is more than amply shown in other places. But, having just adverted specifically to inflammation, which is the greatest outlet of human life, we may say that there are not wanting many who, like Mr. Searle, pronounce both that and fever "a bugbear," and who equally condemn the "antiphlogistic treatment." (2) Nor had we fully realized the extent of the anatomical doctrines, till we met with the affirmation from that undoubted source, the British and Foreign Medical Review, and which we have pla-

(1) Lon. Med. Chir. Rev. Jan. 1839, p. 208. (2) London Lancet, 1836.

ced as one of the introductory extracts to this Essay. (Sec. p. 319—330.) And, as to the practical results, let us hear a writer who, like a thousand others, adorns the pages of the foregoing Review: —

"I have often thought," says Dr. Lewins, "that our most eminent pathologists sometimes appear much more desirous of having an opportunity of dissecting the bodies of the dead, than anxious to make any practical application of their post-mortem examinations for the benefit of the living." (1)

Armstrong had already avowed this opinion:

"I trust the facts of the French, but I receive their conclusions with great caution. They have just burst from the old system of pathology, and seem to me to draw conclusions too general from a few facts."

"They seem as if their only object was to find out the disease without any reference to its relief or cure." (2)

"———— Non est sapientis medici
Canere carmina ferrum poscenti malo." (3)

Tertullian reproached Herophilus "with having hated man that he might learn to know him by dissecting living criminals."

And who can doubt that Mr. Travers was right, and conscientious, when he said, —

"I am desirous to guard myself from the imputation of being in the slightest degree tainted by the heresy of the French school, that there is no such reality as inflammation; that 'it is an old-fashioned coin, of which the impression is effaced, and that it ought now to be withdrawn from circulation.' (4) A better or more convincing proof that the doctrines of Hunter are not understood, for if understood they must have been appreciated, could not be given, even in a school which has only yesterday begun to practise the mode of healing wounds by adhesive inflammation." "If the study of diseased actions had gone hand in hand with that of diseased structures, and the wards had contributed their fair proportion with the museum, it is impossible that such a doctrine could ever have been advanced. Out of the debris of the dead subject, however accurately inspected, examined, and arranged, to attempt a solution of the great problem of living actions, and to build upon such a foundation an edifice of pathology of self-support, is as injurious a fallacy, and scarcely less arrogant or absurd than that of the Cartesian philosophers, who undertook, out of the depths of their anatomical sagacity — to make a man." "The effect of morbid anatomy holding the first, and almost the only place in the mind of the medical inquirer, is to substitute effect for cause, the laws of physics for the laws of life,

(1) Ibid. Oct. 1837, p. 365.

(2) On Acute and Chronic Diseases, vol. i. p. 100; vol. ii. p. 270.

(3) Sophocles, Ajax, v. 582.

(4) Here our author refers to the treatise of M. Andral on Pathological Anatomy. —Mr. Hodgkin, in quoting the same declaration from M. Andral, stamps the new coin with the inscription that "Prof. Andral is one of the most accomplished pathologists of the day." (a)

(a) Lectures on the Morb. Anat. of the serous and mucous Membranes, p. 19.

to confound the cause of death with the cause of disease, and in short to obscure by attempts at simplification." (1) (See Celsus, *motto.*)

Or, take the words of Chomel, so little regarded in his *Leçons sur la Fièvre Typhoide :* —

"L'essence des maladies est distincte de la lésion organique qu'on reconnait à l'examen du cadavre." (2)

"It cannot be too often repeated," says a distinguished writer, "that physical signs only reveal mechanical conditions, which may proceed from the most different causes ; and that the latter are to be determined by a process of reasoning on their connection and succession, on their relation to time, and their accordation with *symptoms.* It is in this that the medical mind is seen. Without this power, I have no hesitation in saying, that it would be safer to wholly neglect the physical signs, and *to trust in practice to symptoms alone.*" (3)

And, as to the substantial advantages of morbid anatomy where it has been pushed to its utmost limit, we have the following view of the subject from another competent source, thinking and speaking in the midst of prejudice, but looking sternly at the facts. It is a commentary upon one of the best specimens of French literature of recent date, whilst it appears to have a more general reference : —

"Upon the whole," says the reviewer, "after all we have constantly heard of the skill of the French Physicians in diagnosis, we must say that we have risen rather disappointed from the perusal of this first part of M. Piorry's work." (4)

Nevertheless, the important guides to pathology are sometimes so overlooked by eminent and discriminating observers, that morbid anatomy might seem to be unusually recommended by this delinquency, were it not the fault of the observer rather than of nature. We are told, for instance, that, —

"Dr. Stokes thinks that there is no absolute line of distinction between bronchitis and pneumonia, and that it would hardly be a misnomer to call pneumonia a bronchitis of the terminal tubes. We may express a doubt whether such an approximation of two diseases that are in most instances distinct in *character*, and in relation to *remedies* as well as its *seat*, be expedient or philosophical." "It should never be forgotten, too, that the *function* of the parts affected in bronchitis is quite distinct from that of those affected in pneumonia." (5)

It was cases of this nature, and more especially what we have seen of morbid anatomy, the doctrine of Brown, the humoral pathology, the physical theories of digestion, of animal heat, and of other secretions, which prompted the remark, that,

(1) Travers' Farther Inquiry into Constitutional Irritation, p. 211 — 212. 1835.
(2) Elémens de Patholog. Gén. p. 522.
(3) On the Diagnosis and Treatment of Diseases of the Chest, *Pref.* p. ix. 1835.
(4) British and Foreign Med. Rev. vol. viii. p. 313. (5) Ibid.

"Simple views of either health or disease, however ingenious, can seldom be just. They have their origin in the spirit of system, not in the careful study and faithful enumeration of the complicated circumstances which concur in the production of all vital phenomena." (1)

And this leads us to say that M. Ribes, holding the same enlightened views, and conscious of the ultimate prevalence of truth, predicts that —

"The time will come when physicians will feel that they have been justly censured (qui ils étaient mépris) for having thought that pathology consisted exclusively in the study of organic degradations, and that they merit the same reproaches which they have bestowed upon others for confiding in the symptoms alone." (2)

We have said, and shall have abundantly shown, in different essays, that the only elements of disease that can be of much importance to mankind, and which form the great basis of medical science, have been neglected by the anatomical school. True, its chiefs have been goaded to an ostensible recognition of symptoms as of some value in ascertaining pathological conditions; and we grant that dissections are often encumbered with unimportant minutiæ. All their conclusions, however, as to the pathology, the radical seat of disease, &c., we repeat it, have been founded upon the degenerations of structure. All the "Remarks," &c., have, in a general sense, a direct reference to those lesions, and little or none to the vital signs; whilst it is everywhere obvious that the treatment has not its proper relation to the vital manifestations. We shall have much in the way of exemplification of what we are now saying, in our next essay; and, as a fair example of the anatomical principles of diagnosis, we will now quote, by way of variety, and for a specific object, from D'Espine. In a case of acute phthisis, the subject (Polian) died after a course of treatment by ptisans, &c. Such, however, was the violence of pulmonary inflammation a short time before death, that her mouth became dry, the tongue dark, and there were fits of delirium. The vital signs were necessarily the guide for the time being. But, with what discrimination? They were supposed to indicate a transmutation of the disease into typhoid fever. The glands of Peyer, however, being ultimately examined, and found in a healthy state, it was decided by D'Espine that there had been no typhoid fever, but that it was a case of "genuine acute phthisis." (3)

(1) Thomson's Lectures on Inflammation, p. 174.
(2) Anat. Pathologique, &c. Pref. p. vii.
(3) Archiv. Gén. 1837.

"The art of discerning the transitions of disease from sthenic to asthenic, and of adapting new kinds of treatment to these changes, belongs exclusively to the skilful practical physician. No study of morbid anatomy can teach it." (1)

This, in the sense in which we have regarded it, (Sec. 15,) is one of the most difficult problems in the healing art; yet it is readily solved by the man of enlightened observation. How emphatically, then, does it "belong to the skilful practical physician" to ascertain the pathology of all the more obvious conditions of disease, and how much more easily may he dispense with morbid anatomy in those less intricate cases where the same principles equally apply? Are not all the gradations of treatment, and, therefore, the whole treatment of diseases, ascertained by the same data as those which enable us "to adapt new kinds of treatment to the sthenic and asthenic forms"? Take any case in the wide range of diseases, and, ere its termination it may present many new problems for the pathologist. It may have lost its original character, or its variations may consist of such modifications of a common pathological cause, that the conduct of the cure shall require alternations of opposite remedies. Every pathological change is attained with accuracy through the direct phenomena, and is a far more difficult effort than the recognition of the primary condition. Morbid anatomy contributes nothing through all the intermediate changes; and what, therefore, is its positive benefit in any given case of disease at its invasion, or throughout its progress? The whole matter is settled before morbid anatomy can contribute its light; and nature would have been faithless to herself had she left her dependence upon art to her own ruins. The mechanical philosopher who is incapable of appreciating the actions of life, and the phenomena of disease, may require this sort of aid; but the very admission implies that he can never attain the art of divination through the vital signs; and sure we are that his anatomical guide can be only appropriated in the way of swelling the record of his own ignorance.

Morbid anatomy relates especially to the high advances of disease, and

"The fullest collection of dissections would be of very inferior use, speculative or practical, unless we should be enabled, by comparing the symptoms with the appearances brought to light, to judge of the *moment* when great local changes are taking place, and of the necessity of a *treatment adapted to this contingency.*" (2) (P. 609.)

(1) British and Foreign Med. Rev. vol. iv. p. 307.

(2) Beddoes, on Fever and Inflammation, p. 104.

True, the vital signs may be wanting, where great disorganizations exist; but, what is the advantage of the discovery after death? Or, where some obscure symptoms were found to depend on a destructive process in some part which yielded none of its usual signs, or there may be an indolent morbid growth in the abdomen, who but a morbid anatomist would treat a subsequent case with the same symptoms upon the ground of such anomalous examples? Who, but the same school, would treat as a case of degeneration of the glands of Peyer a palpaple instance of pulmonary phthisis, when this affection might be suffered to put on some of the symptoms where the glands of Peyer might have been found affected? And this leads us to inquire, whether morbid anatomy be not perfectly powerless as to all those affections which we cursorily noticed at pages 329 — 330, and in other places? Whether it be of any value, if it identify those "typhoid symptoms" which supervene on the fatal stage of almost every fever, with a lesion of some particular, perhaps unimportant part, as the pathological cause, until the mistake may be corrected by the cadaverous results; or, if we must hold inflammation as of doubtful character, till "some of its known products can be brought forward as proofs"?

"Our knowledge of the nature of functional diseases of the brain is not more certain when we find them accompanied by organic changes, than when no morbid appearances can be detected." (1) — So, also, Georget, (2) Ribes, (3) &c.

And is not all this equally true, in a general sense, of the diseases of every other organ?

"Morbid anatomy has thrown no light," it is said, "upon the proximate cause of hooping-cough." (4) But, allowing that it has; it has done no more than to coincide with the best inductions from the vital signs, and to approve the treatment which those signs, enlightened experience, analogies, and general principles founded on an observation of nature, have established as the best. But, say the enlightened observers whom we last quoted,

"In point of fact, there is no part of the body which has not been found diseased in hooping-cough, *and assigned as its seat by those who argue post hoc ergo propter hoc.* (See Vol. I. p. 457, *Sprengell.*) As an example, we may mention that Girtanner lays some stress upon his having found the parts of generation in a morbid condition upon these occasions."

(1) Brit. & Foreign Med. Rev. vol. iv. p. 428.
(2) De la Physiologie du Syst. Nerveux, t. ii. p. 196.
(3) Anat. Patholog. p. 34.
(4) Evanson & Maunsell on the Diseases of Children, p. 210.

That this has been an affair of perpetual occurrence, in relation to most diseases, since morbid anatomy gave the first blow to the doctrines of Hunter and Bichat, we presume no one will deny. May we not, therefore, be well content with

"Knowing that the symptoms show the bronchial mucous membrane to be in a state of irritation, if not inflammation, in the first stage of hooping-cough; and that in the second, they assume a distinctly spasmodic character;" (1) and form our conclusions in respect to any complications from analogous signs, and the effects of treatment?

Has morbid anatomy done anything more for the essential pathology of smallpox, measles, and all the specific diseases; and did not Sydenham, with no better acquaintance with morbid anatomy than possessed by Hippocrates, attain a better knowledge of their pathology, and devise a better mode of treatment for those, and for all inflammatory affections, than has prevailed under the auspices of the anatomical school. (P. 325, *note*.) But we must write as well *by the book*, as from observation.

"With respect to the causes and intimate nature of measles, scarlet fever, and smallpox," says a distinguished observer, "we are in a state of total ignorance." In either affection, as in idiopathic fever, "death may occur at an early period; and then we may fail to discover any morbid characters apparently capable of causing dissolution;" and yet, "their treatment for the most part is fully understood." (2)

And yet we are no more ignorant "of the intimate nature" of the foregoing affections, than we are of fever, or of the essence of inflammation. This essence consists in the alterations of the vital properties, and constitutes the true pathology of disease. It is utterly beyond the grasp of morbid anatomy; but sufficiently illustrated, for all useful purposes, by the vital phenomena.

What has morbid anatomy done towards indicating those modified states of inflammation and venous congestion which are cured by the Peruvian bark? Here the generic character of either affection, and their remarkable modifications, are as readily known, and the remedies as easily adapted, as if morbid anatomy had been long at rest. Or, every other deviation from the standard of common inflammation; as gout, rheumatism, syphilis, scrofula, &c.

Dr. Stokes, after stating that the "numerous, complex, and interesting class of diseases, — the neuroses, — present no trace of structural alteration appreciable *by the senses*," laments "the startling fact, which must be a source of gloomy reflection to

(1) Ibid. (2) Plumbe, on Diseases of the Skin, pp. 215, 224, & Preface, p. ix.

the pathologist." (1) We cannot but think, however, that nature, in avoiding disorganization, has taken the kinder and wiser course in teaching him the pathology of these "numerous and complex" diseases by their "violent symptoms" alone. For our part, we sympathize with the morbid anatomist, who, like Tantalus, may forever pursue his toil without reward; hopeless, and never ending, as the task of Sisyphus, or of the daughters of Danaus.

What has morbid anatomy done towards the pathology of tubercle, unless it be to discard those vital phenomena, and those results of treatment, which so readily teach us its true nature?

Do not those morbid anatomists who fancy tubercle to be only "the result of a peculiar diathesis;" (p. 623,) and its suppuration only a mechanical "softening, or melting," (p. 627—628,) overlook the vital phenomena, and condemn the treatment by which the observers of nature are endeavouring to remove this opprobrium medicinæ? What, again, has morbid anatomy done for inflammation, but to establish conclusions in direct opposition to all the vital indications? What has it done for idiopathic fever, but like inflammation to reject it from the catalogue of diseases, — expunging the constitutional origin not only of fever, but many of its sequelæ, and assigning these ulterior results as the source of the great primary changes? And yet, as much appears to be known of the pathology of fevers, and their appropriate treatment, as of inflammation itself. Amongst those who allow of the latter disease, the speculations are quite as discrepant as in respect to the former; and for this, morbid anatomy is almost wholly responsible. We reach the nature of each through the vital phenomena, their remote causes, and the effects of treatment; and we are no more embarrassed in conducting the cure of fever than inflammations. A common road is travelled in attaining the whole, and morbid anatomy only comes in, in an almost solitary disease, to show us by its physical results that nature is easily interpreted.

Has not morbid anatomy, as taught by its most acknowledged expounders, in ascribing the essence of disease to lesions of organization, (2) precluded all regard for those pathological condi-

(1) Theory and Practice of Medicine, p. 126.

(2) This is an old doctrine of the early anatomists, and went down before the humoral pathology. Casmir Medicus became one of its combatants. "It would be in vain to say," says Medicus, "that changes of structure may be too minute to be

tions upon which the lesions of structure truly depend, and about which the art of medicine is mainly interested? And how untrue to itself, after assuming the "known products of inflammation" to be the great test of its existence, to affirm that in different cases, where all the vital phenomena had been exactly the same, the most common products may have depended upon exactly "opposite conditions of disease;" or that the presence or absence of redness in ulcerated parts respectively denote "opposite" modes of action, and call for opposite systems of treatment. (Vol. I. p. 239 — 270, and next Essay.) And what are the special benefits which accrue from this mode of interrogating nature? Shall we realize the criterion between these "opposite" pathological conditions at an advantageous moment for their philosophical treatment; or shall we by these doctrines promote a luminous ratiocination, or that steadiness of purpose which was inculcated by Hippocrates in his comprehensive admonition, that, "we must not run from one medicine to another, so long as that cause remains which was there at first"?

Or, if morbid anatomy be at other times consistent, where is its regard for the modifying influences of remote causes, which may demand great versatility of treatment, but where, the cadaverous results being the same, morbid anatomy would establish an identity of treatment?

Has morbid anatomy, in an *original* sense, ever given us a solitary clue to the pathology of disease? Do we not at last revert to vital indications for the knowledge we obtain upon this subject? And, following up the various and most essential conditions of disease of which we have spoken as being, by admission, utterly beyond the reach of the scalpel, let us ask, what are the alterations of structure arising from functional derangement, compared with the sympathetic results where morbid anatomy professes nothing, but which, in a general sense, more than the local signs, conduct us to the true pathology of disease? Here, too, it is, on account of the variety of signs, that our embarrassment is often the least. We analyze the group of symptoms, and, by the aid of experience and principles, we go to the work of cure without a doubt or hesitation. The anatomist will tell us no; that we must look to the alterations of structure for

detected. I have excellent eyes, and am too well acquainted with the state of the brain not to be able to perceive alterations on looking after them carefully. But this man had to my astonishment a well preserved brain."

the commotions which are agitating distant parts; that the vast conflict in typhus is but an emanation from the glands of Peyer, (with a variety of parallel examples,)—disregarding those great constitutional causes of which this minor evil has been but a common result; or, rather, neglecting its immediate dependence upon a most important inflammation of the intestinal mucous membrane.

However morbid anatomy, when well directed, may have had its share in promoting the doctrines of the vitalists, we question its assumed importance, and shall have variously offered our proof that all that is known of true pathology, and of fundamental principles in medicine, has been mainly deduced from the living signs of nature. Morbid anatomy could have had no existence, except as a matter of curiosity, had it not been for an antecedent knowledge of the vital properties and actions, and of their alterations in disease, as inferred from their various phenomena. (Vol. I. p. 42—44.) Nothing could have been deduced from morbid products, or from lesions of structure; since in our ignorance of the vital forces and actions, or of their morbid conditions, the physical products might have been as well ascribed to one cause as another. Of this, indeed, we have abundant evidence in the variety of physical causes to which the results of living actions are now ascribed by those who deny the existence of the vital powers. The whole theory of vitalism, in a general sense, was established without the aid of those final results in which the forces of life more or less disappear. But, the forces and actions of life, and their alterations in disease, being known, morbid anatomy contributed to illustrate their effects, and to confirm the science of life. It even went far in exploding the humoral pathology; of which, however, it is now assumed, in connection with chemistry, to have become the foundation. But, do the anatomists or chemists tell us *how* they obtain their conclusions? Was not humoralism at its acme, when less was known even of the natural organization of the body, and chemistry had not advanced as far as alchymy?

The legitimate object of morbid anatomy is to expound the sensible changes which may take place in the instruments of morbid action, the secreted products, and the lesions of structure which may supervene upon disease. These it associates with what had been determined by the vital signs as to the essential physiological changes; and, when doubtful cases may arise, from

the absence of vital signs, should the physical results occur that have been found to have been the sequelæ of certain pathological states, it is then that morbid anatomy reflects its posthumous light with various degrees of importance. In relation to specific cases it is of little or no practical moment; for where the same obscurity occurs, there must be the same deference to the cadaverous results. We know, as in tubercle, the glands of Peyer, &c. that sweeping generalizations have been made, and put forth as the proud trophies of morbid anatomy; but we deny their accuracy and practical advantages.(1) It sometimes happens, however, where some great general principle is concerned, as whether venous congestions depend upon mere physical causes, or upon vital changes, that morbid anatomy may not only preserve the integrity of physiological science, but stay the havoc of disease. Nevertheless, we have endeavoured to show that even here its auxiliary aid should not have been wanted.

Morbid anatomy, in its latest acceptation, points only to the condition of an isolated part. It is exclusive and selfish, though bountiful in liberal professions. It annihilates, at its pleasure, the two genera of diseases which make up the great amount of human maladies, without supplying a substitute upon which we may rest the vital phenomena, or guide the practical hand. Or, if it yield something to the actual signs of disease, it makes that disease to consist in a lesion of organization, and locates all constitutional affections in the part which may happen to suffer most, or most frequently, in its structure. Or, turning to a more enlightened country, we are told that it is nearly the universal consent of the profession that the vital signs of inflammation must submit, unless "some of the known products of the disease

(1) We have been told of "the great value of the generalizations which morbid anatomy has enabled us to make" as to the dependence of sanguineous apoplexy upon organic disease of the heart, or arteries; (Vol. I. p. 379—381,) that it has taught us that chronic peritonitis supposes the presence of tubercles in the peritoneum and lungs; that tubercles in any organ, after the age of 15 years, imply their coexistence in a more advanced stage in the lungs, &c. These and a few other very analogous "generalizations" we admit to have been the fruit of morbid anatomy. But are they of any practical advantage? Have they any foundation in truth? None whatever. They are the "generalizations" almost of an individual,—contradicted by all other experience. And here we are reminded that whilst M. Rochoux was generalizing that the "foyer apoplectique" is always accompanied with ramollissement of the surrounding cerebral substance, M. Bouillaud told him that he had never seen an example of genuine ramollissement of any part of the brain associated with cerebral hemorrhage. (*a*)

(*a*) Lon. Med. Chir. Rev. No. 67, 1837, p. 226.

can be brought forward as proofs" of its existence. But, of what are these physical results predicated but of certain vital signs? They stand as the ultimate indices that a certain mode of action had existed, of which the vital signs had been the attendants, and which had formed the sole ground of that pathological induction, which, after a series of observations, the physical products illustrate, and are merely taken as an indication that those signs, the basis of the pathology, had been present. Or, if absent, or neglected, the products being known to be a general result where those signs had existed, they show us, too late it is true, that an important disease had been either concealed or overlooked. Farther than this, in a specific sense, morbid anatomy contributes no light. But, is it permitted to yield even this light? Is it practically consistent with its own doctrines? How far it is so, we have briefly stated.' (Pp. 316—330, 632.) The action of which we have spoken as being denoted by the vital signs, or by other indications which attend its progress, produces a variety of physical results; and, it is also admitted that it must continue an indefinite time before those results can take place. Can it, then, be said that these signs shall not stand for inflammation till the physical effects have taken place? What would be the value of pathological indications antecedently to death, if this philosophy obtain? It would necessarily end all rational attempts at pathological inductions during the treatment of disease; since, if the true import of the vital signs must depend upon the ultimate contingency supposed, and as the "known products" are often wanting where the phenomena of disease had been exactly the same as in other cases where the "products" have occurred, it is obvious that no just conclusions can be formed, either as to the nature of the action, or the mode of treatment, till the patient is dead. This, it will be allowed is repugnant to reason, from which it will follow that the premises are wrong, and that true pathology reposes upon the vital emanations of disease. And, if it be considered for a moment, that the most important pathological condition of inflammation is more or less subdued when its physical results take place, the relative value of the signs which exist during life, and such as are presented after death, will be fully appreciated.

In a general sense, therefore, morbid anatomy comes in at the closing scene to demonstrate the nature of the physical changes, or to illustrate the extent of our cöoperation with nature, or to

admonish us of an error, or of the imbecilities of art. So far, its lessons, if properly applied, may be of the highest advantage.

It is the devoted pursuit of morbid anatomy which has become the great cause of the confusion which we have endeavoured to present fairly, however strongly, and which must be allowed to be the principal source of the multitudinous theories and hypotheses that have lately encumbered our science, notwithstanding its professions of "rigorous facts," and its denunciation of principles. It is this which is diverting medical physiologists from the true sources from which the doctrines of vitality have sprung, and chaining them to the "triumphal car" of chemistry and mechanics.

Whenever morbid anatomy is told to give way, it seems like again speaking light into existence. Take, for example, the great class of cutaneous diseases. That excellent philosopher, Plumbe, maintains that,—

"The etiology of diseases of the skin" may serve as "a basis for their classification," and "hopes to prove that the constitutional influences over another and more serious class of diseases, will be a far better guide in their treatment, than any arrangement dependent on the form the cutaneous eruption may assume." (1)

Who is there that has bewildered his eyes and his mind with the anatomical details of patches and scabs, and a lumbering nomenclature of regular phases, that does not feel an electric encouragement at the foregoing suggestion? How vast a proportion of cutaneous affections depend upon derangements of the digestive organs, and other constitutional disturbances, of which morbid anatomy can afford no account; and how much better have they been treated by those who have made their sensible appearances of secondary consideration. They are scarcely, indeed, of any other moment, than as serving as an imperfect basis for systematic arrangement, and directing the inquiries of the physician into the nature and extent of those constitutional derangements upon which they are apt to depend, and at which he arrives through the remote causes and the vital phenomena alone.

It is, then, upon the vital signs of disease, its remote causes, and the effects of remedies, that we are to depend in reaching all practical knowledge of any individual case. And here we may say, that we include among the vital signs all that relates to the

(1) On Diseases of the Skin, pp. 39, 43.

evidences of disease before its fatal termination; by which we embrace every possible means of ascertaining its results. All our reasoning, and all our remedies, are directly concerned about this knowledge, with which morbid anatomy can have no connection in any given case, till those physical products or those lesions of structure have supervened which more or less paralyze the hand of art, and which, when once seen, or once stated in connection with the modified phenomena, have received all the contribution of which morbid anatomy is capable, and are then included among the vital signs, as much as the coating of the tongue. Causes are only known by their effects, and the most immediate are the most simple and replete with information; and, to the vital phenomena, in a general sense, we must constantly bend our inquiries to obtain any light upon organic lesions. It is obvious, therefore, that "anatomical medicine" is more a verbal than a real distinction, and that morbid anatomy has been cultivated without a proper reference to philosophical principles. Since, however, the physical products of disease are the result of the same forces and actions upon the which the vital phenomena depend, they form an ultimate and subordinate source of information; and, concurring with the primary causes of disease in ultimately modifying the phenomena, it is important to know how far they may contribute their agency in this respect. But, so far as the general pathology of disease is concerned, it is absurd to carry morbid anatomy beyond a certain limit. As soon as the pathological influences of physical products are settled, necroscopic inquiries may safely cease, and the whole may be left to established inductions, and the phenomena of individual cases. In a more restricted sense, morbid anatomy may, in the foregoing manner, reflect some useful light upon the modifying influences of climate in respect to ulterior results, and upon the limited operation of some remote causes which affect specifically particular parts. But, it can never alter the general principles which it may have assisted in establishing; and this assures us that the time will come when the various errors it has set up in opposition to the laws and phenomena of life will be swept away, and the philosophy of Hippocrates, of Hunter, of Bichat, will again prevail in its purity.

When we have once ascertained the nature of common inflammation in one part, principles are established which are applicable to this disease in all other parts, and at all times and un-

der all circumstances. The varieties must be ascertained by interrogating the particular phenomena, and appurtenances of life, in each individual case, and our practice adapted accordingly. The great principles will, of course, be always under the modifying influence of the phenomena from which they have been deduced. When once ascertained, morbid anatomy can no longer assist us even in our interrogation of those phenomena which may depend in part upon physical products.

"Though all hypotheses," says Sydenham, "founded in philosophical reasoning are quite useless, since no man is possessed of intuitive knowledge, so as to be able to lay down such principles as he may immediately build upon, yet when they result from facts, and those observations only which practical and natural phenomena afford, they will remain *fixed and unshaken.*" (1) (P. 299.)

From what we have now said of fundamental principles, it is obvious that before morbid anatomy could foist itself as a competitor with nature, it was necessary to reject the inductions of Hippocratic philosophy, and to proclaim an adherence to "facts" as the only legitimate object of pursuit in medical science; whilst, under this disguise, it might encumber the science with endless hypotheses, and spurious generalizations. It is also evident, from the extent of those innovations upon fundamental laws and principles, that the man who never dissects may be a far better physician than he who has passed his life with scalpel in hand; but who has supplied the former with a useful ground for induction under the auspices of his habitual observation of Nature.

We shall here revert to one of our introductory quotations from Dr. Stokes, in relation to Bichat. (P. 641.) He did not advance the doctrine as there implied. It was intended wholly in the light of the soundest pathology, as evinced by all his writings. Next to Hunter, the preservation and improvement of Hippocratic medicine will have been most indebted to the French philosopher. Is not the great bulk of Dr. Stokes' work devoted to a luminous inquiry into the localities of disease; and is not the whole force of his genius directed, at last, to an excellent analysis of the distributions of morbid action when he surveys the wide range of idiopathic fever? Like our enlightened author, Bichat followed nature alone; looking upon morbid anatomy as the mere wreck of nature. Of this we have shown ample proof in

(1) Sydenham, on the Dropsy, s. 25.

the brief extracts with which we have enriched our work. There is one sentiment alone, at page 106 of our first volume, which shows that Bichat was not an anatomist in the recent acceptation.

It is not, then, the great architect of the anatomico-pathological school, of which we are a humble advocate, who laid the foundation wrong; but the glare of his light was too much for his countrymen, who, as Armstrong expresses it, "have just burst from the old system of pathology," and who, as avowed by Travers, are unacquainted with the achievements of Hunter. Have the writings of Morgagni, of Haller, &c., been standard works in France? What shall be said of the late success of Mesmer? The transition was too vast and abrupt for France, as she was in Bichat's day, and even for a greater part of Continental Europe; whilst England, from character and habit, has looked on more unmoved by what had been either long before derived from Italy and the north, or was already a work of her own creation. We speak not, however, of the excesses of the dead-house; but of the physical results of disease in their proper subordination to a study of the vital signs, and of those great laws and actions upon which they depend.

There are two sorts, therefore, of anatomico-pathological inquirers: one of whom seeks for "the seat" and the nature of disease through an enlightened knowledge of physiology, and an observation of morbid symptoms; associating the physical results as the offspring of physiological changes: the other looks for "the seat" and the nature of disease through those results alone. This we shall make apparent in our next essay.

Let us now regard, for a moment, the advantages of morbid anatomy in a practical sense; which is the great test of its subservience of true philosophy.

This question must depend upon a broad observation of facts; and we shall first institute our parallel between the existing cultivators of medicine. We are therefore constrained, in placing Europe on one side, to bring to the other the only remaining nation where science has held its equal march; but where, in a general sense, morbid anatomy has not increased in consideration during its late unrestrained career in Europe. With few exceptions, its pursuit has been carried on as in the days of Rush, Wistar, the elder Warren, &c. We have gone on observing na-

ture after our old fashion, have drawn our indications from her living phenomena, and have seen no reason to alter the great fundamental principle, that she is always consistent; and though ruling by many, yet by simple laws which terminate in specific results.

Nevertheless, in justice to ourselves, and the question at issue, it should be stated that American physicians have been long in the habit of making examinations of the dead in those cases especially where the vital signs may have been of doubtful import, or where they had been rendered complex by the physical products of disease. But, we do not think that our pathological views, or our practice, have been much improved by these observations. It is true, however, we have had before us the best authors who have studied nature, and who have made pathology the basis of practice. The writings of Sydenham, Senac, Hippocrates, Celsus, Robert Jackson, Hillary, Hunter, Bichat, Bancroft, Cleghorn, Fordyce, Beddoes, Rush, and a few others of the same class, have been our lights in philosophical and practical inquiries. With these opportunities, we have always known the various results to which inflammation, the great patron of morbid anatomy, is liable; and we have generally been able to recognise its physical products so as to meet the contingency before the death of our patients. Were facts accumulated beyond their present measure, they would not add one mark to our conviction, that it is the constant tendency of inflammation to result in effusions, or to disorganize the body; but we have never allowed that morbid anatomy can infallibly designate the primary, or essential seat of disease, or interpret the cause of death.

With these habits and means of information, we are now prepared to say, that it is the result of no little investigation that American physicians greatly surpass all other nations not only in the decision, but in the success of their practice. They have followed the Hippocratic rule, "that severe diseases require extreme and exquisite remedies," and so in proportion through all their gradations; nor do they substitute the *expectant* plan, till there is a manifest disposition in nature to take upon herself the completion of the cure. And here we may say, her suggestions are equally regarded. We have already justified these conclusions as they respect many of our most prominent writers. (Pp. 278, 292, 485, 487; and Vol. I. pp. 188, 190, 203, 206, 230, 276,

278, 279, 281, 300, 318, 327—328.) Other, and ample records exist to sustain the statement which we make; and placed in a right position, by a charge of imbecility from the anatomists, to illustrate the doctrines and practice of Hippocratists, it has been necessary, in this restricted sense, to contrast their results with those of the school who derive their indications of cure from the debris of the body. And, that we are right as to the neglect of all efficient remedies by the school of morbid anatomy, in a general sense, we appeal to what we have already variously stated, (p. 631—633, 642; and Vol. I. pp. 278, 281, 283, 287, 296, &c.,) to what will appear in our next essay, and, may we not add, to what is proverbially known of the present "expectant" treatment in France, and of the "stimulant" and expectant, in Great Britain.

In America, practice is not only "heroic," but remarkably coincident in relation to the same diseases; which farther shows that our indications are taken from the proper source. Nevertheless, there is a limited class to which we have already adverted, (p. 121,) who are not sufficiently watchful of nature, and, therefore, have no proper, definite principles, and aim at no specific object. They prescribe for symptoms, without duly regarding the nature of the altered powers and actions upon which they depend. Each most prominent symptom is taken in its turn; and, as the pulse rises or falls, or the strength fluctuates, so do bleeding or stimulants have their respective chances. This is the class, also, who cultivate the humoral pathology; and hence a morbid state of the secretions, as we have seen, (pp. 221, 635,) is apt to be considered the disease itself. In all inflammatory affections and in fevers of much excitement, the old books, such as Sydenham, Rush, &c., are taken as the guide: so that, their practice in such cases is scarcely inferior, at the onset, to that of the Hippocratists. But, what they so well accomplish by the lancet, &c., at the invasion of those diseases, they are apt to neutralize soon by an opposite class of remedies. They have little conception of the distinction between that prostration of strength which may instantly follow the invasion of fever, and that real debility which grows out of prolonged disease. Stimulants, therefore, are sure to obtain when the weight of disease encumbers the strength, but where depletion is most required. The illusory signs of returning vigour are an ordinary consequence; but they are the signs of disease modified for the worse, and bring in their train those

developments which nerve the practitioner to the use of the lancet. It is therefore not unusual with this school to carry on, in regular alternation, the stimulant plan during the day, and the antiphlogistic during the night. If patients recover under this ordeal, it is often but in that partial manner which we have described in our first volume, page 288—289, and are liable, sooner or later, to relapses. "*It is of great importance*," *says Celsus*, "*whether one be rightly cured at first, or otherwise.*"(1)

The most sad results of the foregoing practice are witnessed in our congestive diseases. Here it is, that stimulants rouse the circulation for a moment, the sunken features become expanded, and the patient tosses as if with returning vigour. But, the apparent triumph is only momentary; for the sun, which sheds its setting beams upon this antipathological effort, rises upon disappointed hopes and a family of mourners.

This is the only, and a limited school, which approximates the Brunonian in America; and we have adverted to its tenets not only upon principle, but to sustain the comparison we have drawn between practical medicine in Europe and in these United States. *We know of no instance, nor have we ever heard of one in this country, where a practitioner has been addicted to the "expectant" plan of treatment.*

Nor do we think that our treatment of disease has been much improved within the last fifty years, amongst the educated part of the profession. We gather this conclusion from our medical literature, which shows that our predecessors were close observers of nature, and founded their principles and practice upon her phenomena. Those principles, and that practice, have been remarkably without change. The pioneers of the new world, even her clergymen,

"Studied the ancient medical authors, Hippocrates, Galen, Aretæus, Celsus, &c., as among the accomplishments of a finished scholar. New-England, therefore, at its earliest settlement, was provided with some able and well educated physicians; and though not favoured with the great facilities of the present day, our fathers were not less learned in the science of their time, than we in ours." (2)

Let us hear Hosack as to our practical habits before morbid anatomy became the special basis of pathology.

(1) L. 3. c. 2, p. 114.

(2) Thatcher's History of Medicine in America, vol. i. p. 17.

The very first physician, (Samuel Fuller,) of whom we have any account among the American Colonists, writes in a letter to Gov. Bradford, 1630, "I have been to Matapan and let *some twenty* of those people blood." — *Ibid.*

"In the 188th No. of the London Medical and Physical Journal, this practice, (bleeding, cathartics, &c. in fevers,) is recommended as a new practice! and announced as a great discovery; and lately the same practice of bloodletting has been recommended in the treatment of typhus by Walsh of Edinburgh, and by Dr. Armstrong in his late work on the same subject, as a novel treatment. Venesection in intermittents, and in typhus, is a doctrine which has been well understood and practised in this country for many years, and taught in the University of Pennsylvania by Rush, and in this College, (New-York.) I also taught it in the very first lessons I ever delivered on the treatment of fever. All light does not proceed from the East. I had almost said, that Dr. Rush alone has done more towards introducing an efficient practice in the treatment of diseases, than all his cotemporaries in Europe or any other parts of the world collectively." (Vol. I. p. 278.)

This is true of Rush as it respects America; but, if the reader will turn to our first volume, pp. 225, 277, 280, 285, 288, 310—342, he will find that the "heroic" practice, even in typhoid fevers, dates farther back.

"Venesection," says Hosack, "in the cold stage of an intermittent, has been lately recommended in the Edinburgh Journal, as if a new practice. It has been long since well understood in the United States." (1)

Whilst Rush, and Miller, and many others, even Hosack, have been denounced abroad for their bold achievements with the lancet, it is said by foreigners, that,

"In the United States, the practitioners have even surpassed our brethren in India in the use of calomel. They have given one, two, or three drachms for bilious fever, three or four times daily, for some successive days." (2)

Call you this "expectant practice"? It is the practice of hundreds in our wilderness, who have made no other anatomical examinations than of an incidental nature, and yet are familiar with the common physical results of disease. But, they do more, and more wisely. They abjure bark and wine, and carry on a vigorous treatment of inflammations, and of their terribly prostrating fevers, by the lancet. And may we not also inquire how all this compares with the dominant treatment of the same diseases by "gum potions," "a decoction of triticum," or of the "five roots," "bullock's gall," "millipides," "magnetised steel," "a pinch of chevril," "aromatic fumigations under the bed-clothes," (3) &c. &c. (P. 633.) Or is it a less efficient system than the treatment of "the worst forms of fever by table salt"? (Vol. I. p. 397.)

(1) Lectures on the Theory and Practice of Medicine, pp. 246, 247.

(2) Sigmond's Lectures on the Materia Medica.

(3) This is the treatment which was adopted by several of the most eminent anatomists in the complex case of Madame de Stael, and is well known to prevail at the Metropolis of France.

"If such patients," says a true luminary of science, "had a Rush by the side of their bed, they would not be transferred from it to the grave." (1)

True, it has been sometimes urged that American diseases require more active treatment than the European. But, we have shown the error of this conclusion in our essay on Bloodletting, and in other places, by comparing the results of different systems of practice in diseases of the same character and severity, as they have occurred in European countries, and in having always found that success has only attended the practice which prevails in America. We shall add here one instance more to sustain our conclusions, though it might have been well left to the fundamental laws of human organization:

"The pleurisy," says Beddoes, "which, I am informed, was so destructive among our soldiers and sailors till the *lancet* was used with a boldness almost *totally abandoned in this country*, proves that it is not only *American diseases and American constitutions* that require such treatment." (2) (P. 532; and Vol. I. pp. 289, 300, 332, 335.)

What Americans have received from the devotees of morbid anatomy, or from such as would make chemistry the basis of organic science, has only tended to show them more distinctly, that the phenomena of life, in their various relations, are the true foundation of principles in medicine. On the other hand, they are often receiving, from Great Britain especially, medical works whose sound philosophy equally confirms their position. These are periodically, and extensively republished at Philadelphia, (our great emporium of medical literature,) — forming an important era in the medical, if not the general literature, of America. The selections illustrate not only the individual bearing of our subject, but the conviction of the eminent American editors, that they will best serve themselves, as well as science, by giving us the soundest philosophy of the old world; and, whilst we feel our knowledge improved by the luminous views which are thus obtained, they have no tendency to divert us from our steady purpose of taking nature for our guide. It is the ruling genius of this land, to consult the understanding first, and the imagination and senses next; and whatever we may obtain from abroad, that concerns the interest of medicine, is subjected to rigorous analysis, and to the test of reason and individual experience. It is, doubtless, owing to this, that all the efforts of Hosack, backed by the press of Europe, have failed of any

(1) Beddoes, On Fever and Inflammation, p. 84.
(2) On Fever and Inflammation, p. 97.

general restoration of the humoral pathology in America; whilst, as ever, her physicians adhere to the Hunterian doctrines of inflammation, and the philosophy which concerns the products of that disease. Hence, also, it is that France finds little or no quarter except in her unadulterated facts; whilst England is, as ever, our favourite for the purposes of sound information. Still, we are not unfrequently reminded by England of the by-gone days, when one of her best countrymen exclaimed,

"But our own! The first existing! — Well, then, our own, some of our own, scandalized at the disgrace of the common art, did begin to give lessons amain *across the Atlantic.* The lessons, indeed, were received as the irrelevant effusions of men who set themselves *to harangue against facts.* And American pride was destined ere long to have a fearful revenge. Gibraltar, Gibraltar, is pitted against Philadelphia." (1)

We need not tell you what fearful odds drew from a Briton the honest declaration, that,

"After all British reproaches against the American government and American physicians, for *permitting* the yellow fever to commit such ravages, this yellow fever made incomparably greater proportional ravages in a British garrison, where the authority of medical police was unlimited." (2)

And yet again: — "In the West Indies, on the continent of Europe, at home, wherever the British army was posted during the last war, its history of health has offered not only pretexts to a challenged rival to scorn the arrogated superiority of the British modern medical practice, but also reasons for an impartial man to pause upon the claim." (3) (Vol. I. pp. 286, 302 — 303, 693, *note.*)

If we now advert to the history of morbid anatomy in Great Britain at the foregoing period, we shall find that it was cultivated with zeal, especially in the army; and it would seem that practical medicine had been progressively falling off in the ratio of its departure from the true mode of observing nature; whilst we have shown in our essay on Bloodletting, that the latest British writers, who have advocated the Gallic system of Morbid Anatomy, have not only abandoned the vital doctrines of life and disease, but have inculcated the practical imbecilities of the whole system.

We could have desired a less selfish theme, but we had no alternative; and, since our subject is replete with instruction, we shall still pursue it.

It cannot be denied that the faculty of America, in a general sense, have neglected the literature of the profession, and have been even hurried by the necessities of life into its most respons-

(1) Beddoes on Fever and Inflammation, p. 235.

(2) Referring to the New-York Medical Repository vol. viii. p. 449.

(3) Beddoes, *ut supra.*

ible occupation without those preliminary qualifications which accomplish the minds of Europeans. This may seem to conflict with what we have said in another place of the advantages of learning. (See *Appendix* on *Learning*.) But, it is in no respect an argument against us. Much is owing to the national character of Americans, which is early formed in the most active scenes of life, and renders each in his place a subject of irresistible impulses in all practical pursuits. The influence of such habits upon the physician we have already shown, (Vol. I. p. 327.) American physicians, also, are generally conversant with the writings of those illustrious observers whom we have mentioned as their chief counsellors in the interpretation of nature, and it is only within a brief period that their attention has been at all directed to those physical theories of life which have so extensively supplanted the Hippocratic and Hunterian philosophy in Europe. No obstacle, therefore, having been interposed to divert him from his habits of observing nature, and having no leisure for ambitious views, the American physician has gone on with his former conceptions of the vital nature of disease, and of the operation of remedial agents, and would as soon wait for the plough to move itself in the furrow, as to adopt the "expectant treatment." We agree, therefore, that a more extended and varied knowledge of true science would have exalted him even higher in the scale of philosophy, and the practical pursuit of his calling.

In the mean time, let us go on as we have been going,—"gathering, like the bee, from abroad, but digesting that which is gathered by his own virtues;" still leaving the numerical system, and all others like it, "to spin out, like the spider, all its own bowels," and thanking "the empyrical philosophers, who, like pismires only lay up and use their own store," for any proportion of their harvest they may be willing to spare,—being ever willing to receive from the anatomical school their continued castigations for our undaunted energy in the treatment of disease, or their maledictions for waiting upon nature when art has won its triumph. "*Esto perpetua!*"

We will not carry out our conclusions beyond the limit of practical habits and results which we had proposed as a test of medical philosophy. It is certainly humiliating that we should be so far compelled to depart from any becoming modesty; but, truth must not be suppressed where the most vital interests of the healing art may demand its promulgation. It is right that we

should thus inquire into the practical habits of an almost entire profession devoted to the Hippocratic mode of observing nature, and contrast them with the fruits of that philosophy which is founded on the ruins of nature, or seeks an interpretation of her vital phenomena in the crucible of the chemist, or through the glass of the optician. It may be, however, that example, in so agreeable a matter, may have had its influence, since we are told on high authority, that,—

"The inhabitants of European countries, for the Orientals are more modest, easily persuade themselves that their own physicians are the best existing." *We agree also with our patriotic author, that,* "one may assert that no country, that alone excepted which produced the incomparable Hippocrates, can boast a name in medicine equal to Harvey, Sydenham, Hunter, and Jenner; though Bichat seems to have been prevented only by the shortness of his life from attaining equal prëeminence." (1)

We might, indeed, have contented ourselves with tracing the progress of medicine, in its practical sense, from the restoration of letters to the time where morbid anatomy was left by Bichat. What a field of contemplation is here opened to the practical man. We have already explored it, especially in our 13th section on bloodletting, as far as may be necessary to our present purposes. We shall, therefore, only add the remarkable fact that nearly all the distinguished medical authors embraced within that period, carried out in its largest sense the "heroic treatment" of disease. It is stamped upon almost every page of their writings, and has even brought down upon them the censure of the anatomicals. Does the present age afford more than one parallel in Europe of that energetic practice which distinguished most of the luminaries of the 17th and 18th centuries? And what a contrast betwixt France as it then was and now is! Has not the distinguished author of the "Influence of Tropical Climates on European Constitutions" been almost alone conspicuous in Europe, for the last quarter of a century, in his defence of that vigorous practice which marked the eras we have just designated? And do we not at this moment hear a voice from the farther Indies proclaiming the fruits of his individual efforts? (2)

Finally, we have intended nothing offensively in the foregoing remarks; but have spoken according to our means of information, and from a strong conviction of justice and duty. That national feeling may have been interested, we will not deny; but,

(1) Beddoes, Op cit. p. 163.
(2) See India Quarterly Journal of Medical Science, April, 1837, p. 147.

that we are just, we do believe will be sooner or later conceded. And, it is with the same sentiments we must add, that dangers are to be apprehended from the influences of novelty, as well in America as in France; and, we have feared, that one generation, at least, might pay a sad tribute before experience would correct its vices and extinguish its charms. Already our young men, with the most laudable ambition, are crowding the schools of the French Metropolis in pursuit of a more thorough knowledge of morbid anatomy; stopping only in Britain to obtain the acquaintance and civilities of her eminent men, and scarcely glancing at her truly rational institutions, they immure themselves within the walls of Parisian hospitals, to contemplate the worst ravages of disease upon subjects of broken down constitutions, and who have passed the ordeal of French hospital practice. (Vol. I. pp. 302 — 303, 305.) They return home with Gallic pathology, and the *results* of Gallic therapeutics which they could not realize in their own country, and will never witness again but by carrying out the principles which have supplied them with their means of information. Disciplined, however, in their elementary schools, to sound practical habits, thus far they have appropriated their knowledge with a better reference than their teachers to experimental results in the great field of nature. Indeed, they have already given such ample evidence of this power of discrimination, that we entertain the full conviction that they are to be among the first to restore the proper connections between morbid anatomy and pathology. We confess that we were not prepared for this demonstration of independence, and this attestation of our national characteristic. Gross has taken a decisive step; and, what is more auspicious, he is greeted by his fellow labourers.

Having spoken of the general coincidence of efficient practice amongst all the followers of Hippocrates until a recent era, it may appear that we have suppressed some of the facts unless we advert farther, in this place, to a temporary departure from Hippocratic habits which became quite general in Great Britain. But, if we examine the records of this period, as we have already done as it respects the army, we shall find that Hippocratic philosophy had given way to speculation. We have, indeed, also fully shown this on the authority of British writers. (Vol. I. pp. 279 — 280, 286, &c.) And thus Beddoes: —

"Our standard writers held out the terrors of putridity." But this was an

ephemeral doctrine; for, "putting *debility* in place of putrescence, they rendered it almost the universal watchword of medicine, *and annihilated, for a time, a great part of the benefits of experience.*"

This, however, was almost peculiar to the country of Sydenham, and,

"How wonderfully did we pride ourselves in its progress! How did *these discoveries* soothe our national jealousy with the idea of superiority over France in the widest department, — in the whole, of medical practice." And yet, "if we can rely upon what *large and miserable experience* seems to have evinced, we might venture to conjecture, that a person ill of fever was fortunate, or otherwise, according to the variety and stage of his complaint, as he fell into French or British hands." (1)

In those days, the French strove, to the last, with the lancet, or its associate means, in the treatment of those affections to which Beddoes refers; whilst "bark and wine" desolated Great Britain. The latter practice never found its way to the Continent, (except with the passive followers of Brown,) where every medical author of consideration, till the advent of morbid anatomy, engrafted upon his writings whatever is most valuable in the works of Hippocrates.

The brief era of solidism soon followed, and the reign of *putridity*, and *debility*, which even the ancient humoralists had never imagined, was of short duration. But, with the aid of retorts, destructive distillation, and other manipulations of the laboratory, and, withal, in the debris of the body, the modern anatomicals have arrived, by this new route, at the discovery of what had prevailed for many centuries as a mere matter of hypothesis. Being thus the result of "rigorous experiment," the discovery has superseded in its practical effect those phenomena of nature which guided our humoral ancestors in their treatment of disease. Nevertheless, there is a large class who are not yet prepared for the entire metamorphosis; but, mixing together the rival doctrines, equally compromise the bugbear putridity and the nature of the remedies. This is the class who cling to *debility* with the tenacity of phthisis. They look upon soda and salt with philosophical toleration, good, at least, to save from putridity; but, most of all, do they like brandy and bitters.

Finally, take any of the great fathers of Medicine, how little, comparatively, of their time did they give to morbid anatomy. Hippocrates, though very unjustly, is now said to have been even ignorant of the great outlines of the human frame; Sydenham

(1) Beddoes, Op cit. pp. 165, 166.

certainly taught that we should never be able to study disease in the dead body; and Haller, with all his immense experience, discouraged any elaborate inquiry into the physical results of disease. Coming to Hunter, we find him analyzing the principle of life, and expounding the whole philosophy of inflammation, with scarce a reference to a post-mortem examination. All the distinguished ancients held to the idiopathic and constitutional nature of fever, distinguished it from local affections, and were perfectly aware that it often gave origin to those inflammations which morbid anatomy would have us believe are always primary, and the cause of fever.

Returning to the practical results of morbid anatomy as cultivated since the exit of Bichat, do we not find throughout Europe a prevailing disposition to temporize in the treatment of disease? Is not bloodletting held to be baneful in typhus, puerperal, and other congestive fevers, nay, in the gravest inflammations? Is not the strongest language of caution in the treatment of acute pneumonia heard, ex cathedra, not only from the metropolis of France, but of England and Ireland? Are we not told by the highest authority, that,—

"A practitioner ought to think twice of the probable and possible effects in every case of disease or injury, before he determines upon, and proceeds to open a vein for the purpose of, draining off the vital fluid?" (1)

Is there not more than one "Star in the East, who boasts of never wetting a lancet, or using any depletion whatever, in fever or any other disease?" (2)

Have not eminent men, renowned in letters, resorted to mechanical inventions as substitutes for bloodletting? (Vol. I. pp. 215, 290, and *notes*.) Is it not true even of the school of Broussais, that "it thinks there is nothing required but cold water and gum," in chronic gastritis, (3) (p. 647;) and is it not "cold water and gum" throughout all diseases with the anatomical physicians of France? Is not Zeronis' "*expectant* medicine," in acute rheumatism, as contrasted with "the *heroic* practice of Bouillaud," considered, on high authority, "worthy of attentive consideration?" (4) Nevertheless, be it remembered, that Bouillaud's unfortunate expression of "coup-sur-coup" is the most sanguinary part of his practice. Galen, (5) Ægineta, (6) and others of

(1) Liston's Practical Surgery, p. 156.
(2) Med. Chir. Rev. Lon. vol. xxxii. p. 575.
(3) Stokes' Theory and Practice of Medicine, p. 53.
(4) British and Foreign Med. Rev. vol. v. p. 233.
(5) De Curand. Rat. per Venæsec. c. 28. (6) De Re Med. l. 2.

remote eras, and in later times Jackson, (1) Pringle, (2) and especially the French of the 18th century, depleted far more extensively in rheumatic inflammation. (Vol. I. p. 326.) We will not speak of those stupendous remedies, natural magnetism, and artificial magnetism, so recently applauded by the French sçavans, but we will ask, for the proof that there exists in Europe one open advocate, with the exception we have mentioned, (p. 671,) of that "heroic" practice which distinguished the most illustrious of the Hippocratic school? We mean in a broad and general sense.

Therefore do we agree with Dr. Latham, when commending morbid anatomy, that the "face of practical medicine has been completely changed, even in our day." (Vol. I. p. 397.) And yet in the next following lecture to a class of medical students, it is said by our author, who directs the treatment of "500 hospital patients," that,—

"Many a clever man practises physic with tolerable success, who has never troubled his head about morbid processes, and who has not the remotest notion how those things come to pass which he has been witnessing, in their effects or their symptoms, all the days of his life." (3)

Is this, then, to be the abortion of anatomical investigations? Need we be surprised, therefore, that they who had made pathology their study through the phenomena of vital actions, and who, as allowed by that learned physiologist, Stromeyer, had made "the greatest physiological discoveries," (4) need we be surprised, that they had attained, also, the highest advances in practical medicine?

In a final retrospect of our subject, we think it must be conceded that the whole foundation of philosophical medicine was broadly laid by him whose cognomen designates the most illustrious schools that had flourished till the recent origin of the anatomical. We shall not here survey the grandeur of that philosophy. The record is widely extant, and we have often had occasion to gather gems from its exhaustless mine. We are now, too, concerned about the practical uses of human inquiries; and these we have variously set forth as not only the manifest object of the Hippocratic philosophy, but as eminently characteristic of its author. True, Hippocrates had the honesty to relate the histories of the worst, and often unsuccessful cases. Shall we reproach

(1) Ut cit. *passim.* (2) Obs. on Diseases of the Army, part 3, c. 2.
(3) Lectures on Clinical Medicine, pp. 46, 59,
(4) On the Production of Sensations and Motions; *tr.*

him for this; or rather emulate his example? It should be considered, too, that in his Epidemics, he aimed at *delineating* disease, and less at defining the method of cure. The latter he had amply done in other appropriate places; and whilst he commends efficient bloodletting,(1) cathartics, &c., in all inflammations, we hear nothing of a stimulating treatment in fevers or other affections which prostrate the powers of life at their invasion, unless as a temporary expedient to enable nature to sustain those specific agents which can alone establish *a change* in the forces of life, when so greatly altered by morbific causes. That Hippocrates was eminently successful in practice is amply attested by the unexampled renown which he enjoyed during life, by the divinity with which he was invested. That his principles were founded in nature is immutably shown by that universal renown, and by the applause of all posterity;(2)—save, indeed, those philosophers who seek in physics the mysteries of life, and who therefore adjudge with Euripides, as in another instance, that, "whoever shall be found crowning this dead man shall suffer death for his pains;" or, who do not, as Xenophon has it, "allege against him that which is commonly charged on philosophers by the multitude."

We shall not farther speak of the luminous cultivation of the Hippocratic philosophy by Aretæus, Celsus, and other eminent ancients, nor repeat the proof of its consistent results, and of its practical influences in medicine; nor will we dwell upon its decline as the source of that ignorance and superstition which covered the earth for so many ages. Deride the past as we may, vaunt of the genius and discoveries of the present age as we will, (we speak only of medical science,) believe as we may that, "medicine is now in its infancy," we cannot resist the overwhelming reaction that is about to come. Those men, once so illustrious, but whose opinions and habits we are now affecting to despise, are about to step forth upon the theatre of life with a more mighty tread than ever. They are coming forth to decide the

(1) He often carried bloodletting *ad deliquium;* and even in the quincy, where we are now often told that bloodletting must be avoided, he bled simultaneously in both arms.

(2) It has been considered a defect in the Hippocratists, that they do not use the stethoscope. We allow the great advantages of this method of investigation; but, it is probable that Hippocrates paid a more general attention to the state of the respiration than any modern stethoscopist. It is a theme of constant remark in his writings, and appears to have engaged his attention in all cases of disease.

great contest which is now going on betwixt their cöoperators in observing and interpreting nature, and those who are employed in seeking for nature in the midst of her ruins. At the head of this mighty band you will see the Great Father of the healing art. On all sides you shall meet the stirring spirits of Aretæus, Ætius, Celsus, Galen, Aurelian, Trallian, Ægineta, marshalling the great multitude that worshipped in nature's temple, and who, with them, are rushing forward to the rescue. "Ἐκτοῤα ὧας ἑπεται." So vigorous the onset, you may almost hear the clashing of the gigantic minds.

"————————'Tis the Past
Contending with the Present; and in turn
Each has the mastery.————————" (1)

Is it not a glorious spectacle? Will you not fall down with us and do it homage? The trial must be decided by that great test of principles in medicine, their practical application to the uses of man. You see them all executing with decision what the living voice of nature had prompted. They were unacquainted with her mysteries when they were only uttered in her expiring groans. On the other side, you hear of nothing but nature in her last agonies, and therefore of nothing but despair and the imbecilities of art.

"Let the happy combination of genius be honoured with admiration, the tribute of spirit be paid to spirit, the highest acknowledgment to the highest desert."

"Had we to fit up a hall for practical lectures in medicine, opposite the chair we would place a group of the faithful observers of our nature in conflict with sickness, fresh in their evergreen honours; and Sydenham in the midst. We would also introduce a group of those medical pedagogues, the most vainglorious of mortals, who have abused their professional authority to dazzle the boys around them, and whose vanity has stimulated them to exertions of a bastard ingenuity, till their faculties have become too weak to burst through the hypothetical cobwebs which themselves had spun."

"Meanwhile, it will be some satisfaction to have given a plan for stifling, in their birth, some of those epidemic errors, which issue from the schools and become more destructive, more inveterate, and extensive in their range than any epidemic diseases, and the chance of being considered in other countries and other ages. Whenever it is executed, it will furnish a work for the whole earth, which we mortals inhabit; and he who shall execute it well, would entitle himself to a civic crown from the growth of all its zones. (2)

(1) Rogers' Florence.
(2) Beddoes on Fever and Inflammation, pp. 171, 237, 241.

ON THE PRINCIPAL WRITINGS OF P. Ch. A. LOUIS, M. D. &c.

"Fiat Justitia, ruat cœlum."

Πειφμαι τον Θεον η υμας, χαι εωσπερ αν εμπνεω χαι οιος τε ω, ὃ μη παυσομαι φιλοσοφειν.—PLATO.

"———————————— 'Tis a base
Abandonment of reason to resign
Our right of thought,—our last and only place
Of refuge; this, at least, shall still he mine;
Though from our birth the faculty divine
Is cramped and tortured,—cahin'd, crihh'd, confined,
And bred in darkness, lest the truth should shine
Too hrightly on the unprepared mind,
The heam pours in, for time and skill will coucb the blind."—BYRON.

"The school of Louis, to which we owe many excellent monographs on individual diseases, urgently impressing, as it does upon the tyro, the necessity of the most careful observation of the phenomena presented by disease, is apt to leave the impression that this is all the practitioner needs, and to convey the too exclusive idea, that self-observation is alone necessary to make the accomplished pathologist and physician; an ideal rock on which the profession has struck for ages, and which has greatly retarded the onward course of medical science."—PROF. DUNGLISON'S *Introductory Lecture*, 1840.

"These volumes, (Louis, on 'Typhoid Fever,' 'Phtbisis,' and 'Bloodletting,') contain the results of the observations of one whose clinical practice has had more influence, perhaps, upon the therapeutics of the present day, than that of any other individual. They must be perused hy the student free from all hias; and careful observation must prove to him the sources of fallacy, if sucb there be, in the views of this distinguished author; due attention heing paid to the circumstance, that the observations of M. Louis were made in another hemisphere, and therefore require careful repetition in this."—IBID. *Medical Student, &c.*

"The results attained by tbe foregoing inquiry will, no doubt, surprise many, if not most, medical men. They certainly do not accord with my own previous impressions in various respects."—JACKSON'S *Appendix to Louis*, "*on Bloodletting*," *p.* 99. 1836.

"If anything may be regarded as settled in the treatment of diseases, it is that hloodletting is useful in the class of diseases called inflammatory; and especially in inflammations of the thoracic viscera."—JACKSON, *ut supra, Preface, p.* 1.

"It would seem, howcver, that he, (M. Louis,) bas not ahandoned the practice, (hloodletting,) although we were assured that this conclusion was drawn from most accurate arithmetical and statistical data! How are we to place any confidence in the practical directions of such a writer, however high his attainments as a morhid anatomist may he? And yet some of our most popular authors of the present day attempt to laud him as another SYDENHAM, or MORGAGNI."—MEDICO-CHIRURGICAL REVIEW, *Lon. Oct.* 1839, *p.* 543.

"The empyrics of the present day, whetbcr within or without the pale of the profession, by no means take the ground occupied hy those of the ancient schools. Instead of ahiding within tho narrow limits of EXPERIENCE, THEY ERECT THE MOST HASTY GENERALIZATIONS."

"The band of ohservers which M. Louis is training up may contribute much to the advancement

of science; but, we doubt much whether they will do more, on their present plan, than render the art more empyrical.—British and Foreign Medical Review, *July*, 1838, *pp.* 111, 112.

> "Who is there now, sayde the porter,
> That maketh all thys knocking?"—*Adam Bell, Clym of the Clough, &c.*
> "Downright impossibilities they seek;
> What man can be immortal in a week?"—Young's *Epistles.*

"I hope I shall not be chargeable with the dangerous fault of seeking and hankering after novelties."—Reid. *Experiments on the eighth pair of nerves.* 1837.

"That the numerical method should have opponents is natural, and was easily foreseen." "Fortunately for the progress of science, the numerical method is considered by the most judicious and experienced men, as a necessary INSTRUMENT for establishing general principles in medicine; and attacks on it will be futile." "Therapeutics cannot advance without it."—Louis *on Bloodletting.*

"Nothing is MORE FATIGUING than the method we have described; for, what numerous tables, what separate analyses does it not require?"—Louis *on Phthisis, Preface, p. lxviii.*

"Happily for the future well-being of medical science, the numerical method is considered as the SOLE means of arriving, in medicine, at rigorous and accurate results."—Louis, *on the Proper Method of examining a Patient. Tr.*

"We are constantly told of the experience of ages in medicine; but, how can this experience ever be embodied, if those who write, instead of saying I have seen so many and so many times, merely say I have often seen, or seldom seen?" "Suppose thousands of authors to have proceeded in this manner, it is as if there had been but one; and in many respects, as if there had been NONE AT ALL. If, then, there is any means of embodying the experience of ages, it is the numerical method. This proposition will undoubtedly soon be the current opinion; and then we shall hear no more of medical tact, of a kind of divining power of physicians. No treatise whatever will continue to be the sole development of an idea, or a romance."—"Behold, then, TACT, INSPIRATION, CHANCE, TRANSFORMED INTO GENIUS! For what is inspiration or tact, if it is not chance?"

"I will add, that the bare idea of proving, in pathology and therapeutics, according to the practice of THE PRESENT day, by selected observations, even when sufficiently detailed; that this bare idea shows that the science of medicine is not considered by physicians, as they say it is, to depend entirely on observation."

"The reader will be astonished, undoubtedly, that in the nineteenth century, authority could have been invoked in a science of observation; without remarking that that, which we call experience, is nothing but authority. In fact, to what authorities do those, MOST CELEBRATED for the wisdom of their precepts refer, unless it be to the practice of their predecessors."

"THE PRETENDED EXPERIENCE OF AUTHORS IS WORTH NOTHING, and after all their assertions and denials, we are no farther advanced than before."

"They," physicians, "scarcely agree EXCEPT on points which are admitted WITHOUT ANY EXAMINATION, or as established by long usage, which has nothing to recommend it but time."

"Here, again, the author, (Poliniére,) appeals to experience in support of his assertion. But the experience, to which he refers, is evidently tradition, custom, common belief; it is that almost WORTHLESS THING, which Quesnay denounced so vehemently; a compound of VAGUE RECOLLECTIONS."

"He cites authorities; and among others, that of Pinel. He might as well have confined himself to a SIMPLE DENIAL."

"The author, (Poliniére,) makes the following remarks: 'I could easily have adduced a host of facts, in support of my propositions, on the subject of bloodletting. In a vast hospital like that of Lyons, there is no want of facts; but such a mass would have encumbered my book, without adding to its usefulness; being well persuaded, that extraordinary and rare cases ought not to occupy a prominent place in a work on practical medicine; that those of daily occurrence should be the subject of study';" &c. "Without doubt, a few examples are sufficient to exhibit the practice of M. Poliniére in similar cases; but they are not enough to prove that it is good," &c.

"His precepts are founded on some facts FAVOURABLE to his views; bad logic for one who can thus PROVE ANY THING, or upon the experience of the ancients; and we have seen before what is the character of that experience, founded as it is ALMOST ALWAYS UPON TRADITION without proof."

"As the word experience, ill defined, has been an unanswerable argument for many physicians; it has been the same with the word success. Thus, in speaking of the period at which we must bleed, Fréteau cries, 'Baillou, Riverius, SYDENHAM, &c., have imitated the example of HIPPOCRATES, and obtained success'! But how has this success been demonstrated?"

Fauchier proceeds in the whole course of his work, A PRIORI, as has been done even TO THIS DAY BY THE MOST ABLE MEN, who have considered therapeutics as a simple corollary of pathology."

"Fauchier, thinking that experience sufficient, contents himself with being its ECHO"; "for his

work is in truth nothing else. Yet this work, and let us not forget it, for it shows the SPIRIT OF THE TIME, was crowned by a medical society." "He denies the doctrines of derivation and revulsion, because he believes them not to be in accordance with the laws of the circulation. That is to say, he follows the course of Quesnay," &c. "We feel besides, that a man, who places so much confidence in A PRIORI considerations, WILL NOT BE VERY EXACT IN RESPECT TO PARTICULAR FACTS."

"There is not a SHADOW of direct demonstration, (in Quesnay's philosophy and indications of bloodletting;) we might suppose that he would have thought himself DISGRACED by attempting it." "Unfortunately Quesnay HAS NO OTHER METHOD, and certainly the incomplete experience of practitioners cannot take rank BELOW HIS ASSERTIONS."

"Fréteau proceeds NO OTHERWISE THAN HIS PREDECESSORS; his point of departure is the SAME. Like them, he makes the indications of bleeding, in each disease, to result from its general effects. An excellent method, if essay-making were the object, but which brings us to the infancy of therapeutics; since it can only conduct us to probabilities, and not to certain results." "In HIS estimation, presumption, probability, indication, and demonstration, are SYNONYMOUS terms."

"The course of Vieusseux IS LIKE THAT OF THE AUTHORS WE HAVE CRITICISED;" "repeating what others have said, as if unimportant customs, and not a science, were the object of HIS STUDIES." "Our author, as may be easily conceived, has not been VERY DIFFICULT as to particular examples." "He shows but little accuracy in appreciating facts."

"The authors of prize essays, and ALL who have been engaged on the subject of bloodletting, have failed to settle definitively any one precept."

"In therapeutics, nothing is effected, unless it is demonstrated," "and the MOST ABLE physicians have, it must be confessed, FORGOTTEN LITTLE ELSE than this very demonstration."

"Let us not forget it then in future: if the experience so justly scorned by Quesnay is an uncertain guide in practice, it is that it possesses NOTHING of true experience BUT THE NAME; that it is, in truth, only the common usage, not justified by rigorous observation; AUTHORITY IN A WORD."

"Let those, who engage hereafter in the study of therapeutics, pursue an OPPOSITE COURSE TO THAT OF THEIR PREDECESSORS."

"Men have for ages devoted themselves to therapeutics, and the science is still in its INFANCY."—LOUIS, *On Bloodletting*, c. 3, 1836.

"From the earliest ages, the study of therapeutics has been carried on, and therapeutics are yet in their INFANCY."

"The physicians of antiquity have left us but very imperfect descriptions of the diseases which they have observed. [Hippocrates?] They have handed down to us numerous rules in therapeutics, but have given NO PROOFS of the truth of them. Their doctrines have been supplanted by others, ALL of which have PRETENDED TO BE THE ONLY TRUE ONES. MODERN physicians have been BUT LITTLE MORE SUCCESSFUL," &c. "Their observations, with a FEW exceptions, cannot be of any aid in the advancement of science, nor can they serve for the INSTRUCTION of any who may read them."—LOUIS, *on the Proper Method of examining a Patient. Tr.*

"It cannot be TOO OFTEN REPEATED, that the science of medicine is faulty in its foundation."—LOUIS, *on Phthisis, &c.* 241.

"We cannot TOO FREQUENTLY REPEAT that medicine will never make any progress until all minds will be convinced that we must collect and compare ALL the facts, upon a subject, whatever they may be."—LOUIS, *on Typhoid Fever, vol. ii. p.* 351.

"It is not improper, I think, in this place, to remark that we ought not to depend upon the authority of the ancients in regard to questions relative to the seat of diseases." "It is not true, moreover, as has been said too often, that facts do not become OLD." "The immense majority have become so, and, moreover, those which we collect in these times, will, in like manner, in their turn, BECOME OLD." "The best work is good only in relation to the EPOCH at which it appears."—LOUIS, *on Typhoid Fever; Advertisement. p. iz.*

"Si Hippocratis aphorismos, præsagia, coacas, etc. cum posterorum observationibus comparaveris, quæ fuit superioribus sæculis, EADEM NUNC ESSE MORBORUM NATURAM, et eodem ab olim ordine procedere illorum periodos liquido constabit. Ex his omnibus deduci jure poterit, Medicinam non adeo incertam esse nec adeo levibus, ut vulgo putant innixam fundamentis, sed EX REGULIS CERTIS, multoque usu confirmatis pronunciare."—BAGLIVI, *Prax. Med. l.* 1. c. 2.

"The reader will pardon us, perhaps, for having insisted so MUCH on the care we have bestowed on the collection of OUR facts, and upon the DISTRUST with which part of those DAILY PUBLISHED OUGHT TO BE RECEIVED," &c.—LOUIS, *on Phthisis, Preface, p. lxvii.*

"I have sought always to give clearness to MY views, which, together with RIGID EXACTNESS, is the most indispensable of all qualities."—LOUIS, *on Typhoid Fever, Adv. p. xiii.*

"What importance is it, however, in respect to effects for us to know what is the CAUSE of trouble in our functions, when this trouble is of a serious nature?"—LOUIS, *Ibid. vol. ii.* pp. 145, 132, 142, 144; &c.

"Need I repeat that an excellent mode of arresting diseases is to CONFOUND them; or at least to make no distinction in the periods, at which such and such remedies were employed." — LOUIS, *on Bloodletting*, *p.* 31. (1)

"Get behind me Avicenna, Galen, Rhazes, Mesue, Montagnana; behind mo, Doctors of Paris, Montpelier, Suabia, Cologne, Misnia, and Vienna. You, Islands of the Sea. Thou Italy; thou Athens; thou Greek; thou Arab; thou Isaclite; — behind me, FOR THE MONARCHY IS MINE."— PARACELSUS, *in Paragraph. Præf.* — 1571.

> "'Tis yours a Bacon or a Locke to blame,
> A Newton's genius, or a Milton's flame." — POPE.

"After the manner of the Ottoman princes, says Lord Verulam, he thought his throne could not be secure unless he killed off all his brethren." — KAMES, *on the Progress of the Sciences. b.* 3, *ap.*

> "——— I'm posterity, — and so are you;
> And whom do we remember? Not a hundred."
> "Even Plutarch's Lives have picked out but a few,
> And 'gainst those few your annalists have thundered."
>
> BYRON'S *Don Juan.*

> "Peace to them all! Those brilliant times are fled,
> And no such lights are kindling in their stead."
>
> COWPER'S *Tirocinium.*

> "The 'good old times'— (all times, when old, are good,)
> Are gone; the present might be, if they would."
>
> BYRON'S *Age of Bronze.*

"It has been the folly of a great many, upon dissecting a corpse, to take the part that was most affected to have been the CAUSE of the person's death, when it was only a consequence of other disease which might have been cured if the original cause had been first considered. But, we ought to consider, that when a man has been racked to death by disease, there may happen strange alterations of the solids. Thus, some have ascribed a DROPSY to the LIVER as the cause of it; others to the STOMACH; others, again, to the MESENTERY, SPLEEN, KIDNEYS, &c.; because they saw some one of these parts mostly affected." — SPRENGELL'S *Commentaries on the Aphorisms of Celsus*, *s.* 9, *a. ix.* — 1735.

"The PANCREAS has been considered the seat of the febrile cause, and this opinion has been thought to derive support from ANATOMICAL observations; and others have considered the fomes of the disease as lying hid in the recesses of the MESENTERY." — SENAC, *on Fevers*, *p.* 9. *Tr.*

"There are many who seem to think that the CAUSE of an effect is that which has immediately preceded it. All their logic is founded on this principle, — this happened after such a thing, of which this is the effect. The lightning is often attracted by the trees, under which a traveller shelters himself during a storm. Therefore, say they, the traveller is the CAUSE of the lightning falling on these trees." — ZIMMERMANN, *Experience in Physick*, *vol. ii.*

"When the fair Queen of France or the Dutchess of ——— catch a cold, and are told by their physicians, that the distemper is bilious or nervous, or any other medical name, which makes a plain matter mysterious, they repeat the terms; and bilious and nervous complaints become as universal as sashes and feathers." — MOORE'S *Medical Sketches.*

"Magnam vero artis partem esse arbitror, etiam de his, quæ scripta sunt recte, posse considerationem facere, ac judicare. Qui enim hoc novit, et his utitur, non videtur mihi in arte multum falli posse;" &c. — HIPPOCRATES. (2)

"Naturæ non hominis voce loquitur Hippocrates Medicorum Romulus." — BAGLIVI; *Prax. Med. l.* 1, *c.* 1.

"Medicina non ingenii humani partus est, sed temporis filia." "Necessitas medicinam invenit, experientia perfecit." "Non in humani profecto ingenii acumine sita est ars præstantissima, quam diligens, accurata, et sagax nota Naturæ; atque animadversio peperit; sed potius variis cujusque ætatis Doctorum laboribus coacervata Sapientia dicenda est, hominumque multorum mens in unum quasi collecta." — BAGLIVI, *Prax. Med. l.* 1, *c.* 1, 2.

"The more general, and perhaps unanimous opinion is, that medicine is advancing with steady progress to perfection." — WM. BROWN, *in Edinburgh Medical and Surgical Journal*, *Oct.* 1839.

"Sic hypothesibus, quibus hodie tantopere delectemur, quid incertius?" — BAGLIVI; *Op. p.* 26

(1) The Italics &c. are ours, (See p. 145, *note* 3.)

(2) De Morb. Epidem, l. 3, s. 3. Ægrot. 16, v. 358 — 361. Ex Jani Cornarii Versione, 1737.

In approaching the works which we have selected for the subjects of this commentary, we have been actuated by various motives. These will appear, from time to time, as we advance with our undertaking. But, we may say now, that we have especially in view an exhibition of the ascendancy which false philosophy may obtain, in the intricate science of medicine, at the most intellectual era of man, and to exemplify the inductive and practical results which spring from morbid anatomy, when assumed as a paramount guide in pathological inquiries. We hope, also, that before our task is done, we shall have proved to the reader, that the method of observation which is condemned by our author is the only one by which medicine can make any advances, and that it has been the means of a vast inheritance to the present generation. If we endeavour to show that our author is wrong in ascribing the typhoid fever to a lesion of the glands of Peyer, or that it is not an anatomical characteristic, or to prove the fallacy of any other induction, our objects reach far beyond the specific question at issue. We are especially interested about the general influence of our author's philosophy, and his habits of reasoning, upon the interests of science, and the well being of mankind. Our author has occupied the proud elevation of presiding over the modern anatomical school; and it becomes, also, a matter of deep moment to inquire, through his remarkable labours, into the practical results of morbid anatomy, and how far it may have advanced, or retarded, the progress of medicine. The present essay, therefore, may be considered so far a continuation of the last; but, having other important objects in view, we shall not hesitate to repeat any facts or conclusions which may be necessary to these purposes. We have also bestowed some notice in our first volume upon our author's numerical system in its relation to bloodletting; but, we shall transiently notice it again in other aspects. We are fully sensible of the evils of repetition; and these we endeavour to avoid as far as may be consistent with the integrity of each essay, which, to a certain extent, must be rendered independent of the rest.

We have thrown together, at the beginning of this article, for the purpose of convenient reference, numerous extracts, which have also a general application to our inquiry. The reader will have observed amongst them a series of unsparing condemnations of all past and existing experience, and the great fundamental principles which have been established upon that experience.

Of this we have already said something in our Philosophy of Bloodletting, and shall have more to say hereafter. Our first purpose will be to inquire how far our author has justified this act of ostracism, and how far he has fulfilled his engagements to science, by adhering to his promised system of accumulating "rigorous facts," and leaving to posterity those general inductions which he professes must await an extensive range of observation conducted upon the "numerical system"; our author having also made the want of sufficient data the ground of rejecting all former principles, — nay, discarding all former observation, that the principles may appear the more utterly baseless. That the world should have passively acquiesced in this unreserved obliteration of all its medical knowledge and principles, (executed, too, in no very gracious manner,) was neither just to itself, nor watchful of its dignity. That it should have received the exorcism with a commendation proportioned to its abruptness and insensibility, must remain forever the most extraordinary record of all human affairs; and, when after ages shall look back upon the present, groping its way in a midnight darkness of its own creation, and rejoicing, as it were, with the prattling "infancy" of a once noble and stupendous science, and witness, as its results, the experimental processes by which the new being was to be carried forward to maturity; — the myriads of victims who furnished their quota to the morbid-anatomist; the attempts at converting morbid into healthy blood by chemical agencies, first in a "porringer," and then, by analogy, up to the living organism; the conflict between the remaining disciples of Nature and the abuses of the laboratory; the almost universal substitution of the forces of physics for those specific powers which had hitherto rendered physiology and medicine intelligible and consistent sciences; besides a multitude of other strange devices, contributed and cordially received from all manner of workmen, as choice materials for the new foundation; — when, we say, after ages shall look back upon this dark spot on the brightest escutcheon of the world, it must be regarded without sympathy, and as an act of voluntary humiliation.

Here, then, in connection with the foregoing subject we have the first, and greatest act of generalization by our author, without either a "rigorous" or a shallow fact to sustain it. Much of the denunciation of past and present observation, as exhibited in our introductory quotations, is embraced within the limits of one

short chapter, which, as will be readily supposed, is chiefly made up of those judicial decrees. But, as we shall see hereafter, *this generalization of experience and principles* was indispensable, as a preliminary step, to the ultimate objects of our author.

We shall now pass to our author's celebrated work on what he designates as Typhoid Fever, or the Typhoid Affection. But, it should be observed in the first place, that in the "Advertisement" to this work, Chomel is spoken of as having the supervision of the hospital. This distinguished observer, however, should be in no respect associated with our author's performance. The latter was the active man, as appears from the history of most of the cases. Thus, for instance, he says "I found him in the following condition," &c. Again, "whether I was enabled to learn anything *very exact* about this symptom, [in a general sense,] or whether *I neglected* obtaining information respecting it, it is certain that mention is made of it in the cases of *sixteen* patients only," &c.(1) It is also of constant recurrence, "*I prescribed*," "thinking I wanted to feel her pulse," &c. And, then all the minutiæ about "*red*" and "*white*," the constant repetition of "rigorous facts," show that our author was the acting attendant. But, this would be unimportant, since our author makes the cases entirely his own, erects generalizations upon them, and defends the whole treatment. (P. 633, *note.*) And here it is important to bear in mind that all our author's conclusions as to this affection were founded upon "one hundred and thirty-eight observations of typhus fever, *fifty* of which were relative to individuals who died of it;" and, that he "compared" this exact number of fifty cases "with the alterations found as consequences of other acute diseases, in *eighty-three* subjects whose histories I learned." It should be also observed that these cases were "collected" during the period of some five years, or "between the years 1822 and 1827." Upon these cases, and the lesions observed in other diseases, our author founds those conclusions which will appear in the sequel, and which have exerted, in connection with his parallel works on Phthisis, and Bloodletting, such an extensive influence upon philosophical and practical medicine. In the meantime, let us also observe in relation to the Typhoid Affection, that it is stated by our author, that,—

(1) Vol. ii. pp. 290, 187, 180, 136, 202, 39, 45, 128, 129, 171, 206, 7, 17, 27, 162, 234, 307, 382, 384, 391. Vol. i. pp. 45, 64, 82, 95, 121, 148, 237, 87, 85, 244, 159, &c.

"In these analyses I have not made use of any records of disease which did not appear to me to be sufficiently exact, and whenever I have made *deductions* from those which were exact, I always kept in mind the thought of the author of Emile. '*I know that truth lies in the facts, and not in the mind that judges of them, and that the less I introduce what is merely my own into the deductions I make from them, the more certain I shall be of approaching the truth.*'" (1)

The reader will ultimately feel the whole import and intended force of the foregoing paragraph; and, whilst our author is everywhere engaged in drawing the most unqualified generalizations from these limited observations, the reader is as constantly drawn into the belief that our author is only concerned about the exhibition of "rigorous facts." And yet be it said that our author, to carry the only purpose which could render these "138 observations" in the least instrumental to his fame beyond the day of their promulgation, announces in his "*Advertisement*" what is everywhere the final object of the analytical investigations,—"the hope of arriving at *conclusive* results." Who the subjects of the observations were, what their habits, what their artificial constitutions, what the treatment of their diseases, upon which the most important conclusions have been founded, to unsettle all past experience, to turn the world from its former habits of observing nature, to enforce the injunction "*let those who engage hereafter in the study of therapeutics pursue an opposite course to that of their predecessors,*" and to carry the vast influence of general inductions to the highest temporal destinies of all mankind, will form a subject for consideration.

On advancing from the *Advertisement* to our author's text, the first thing which strikes us with astonishment, is his departure from his system of "rigorous facts," and his neglect of his own rule in respect to the usages which he imputes to others, and that patient regard for the "*numerical*" mass of observations which we are told can alone justify any general conclusions;—the first thing, we say, which excites our surprise, is the broad affirmation that a lesion of the glands of Peyer may be taken as the anatomical characteristic of typhoid fever, because it was present in 50 cases of acute disease having certain other analogies, and was absent in 83 other cases where those analogies are said to have been more or less wanting. Here, in this 2d generalization, is one important foundation of our author's renown. He had thus, as it were, identified himself with an unexplored

(1) On Typhoid Fever, &c. vol. i. *Advertisement*.
We use Bowditch's translation, Boston, 1836; and the Paris edition of 1829.

disease, and presented it as an isolated affection which may be always distinguished from the group with which it is allied by a comparatively unimportant lesion of structure. That it was this generalization, and others like it, and not the project of the "numerical method," which has unsettled the medical world, and which has placed our author where great general inductions or "conclusive results," when once received, never fail to carry the promulgator of truth, or the aspirant at fame, all subsequent medical literature fully attests. (1)

But, let us inquire how far our author has been sustained, in the foregoing generalization, by the observations which he has elicited from others. We allow that this may seem a work of supererogation to the most enlightened of the profession in Europe; but, it is necessary to the purposes of this essay that the subject should be considered. The novelty, too, in a general sense, appears to maintain its original sway; and we may, therefore, render some service to physiological and pathological principles by showing that any muco-intestinal inflammation is liable to develope disease in the mucous follicles. Indeed, we maintain that this conclusion should have been deduced from the well-known facts in relation to the intestinal inflammations which supervene on phthisis, and where the glands of Peyer are found to have undergone structural lesions. It is one of the great uses of science to protect the understanding against the delusions of novelty.

A single example, however, upon our author's premises, of an alteration of the glands of Peyer in any acute disease analogous to their alteration in typhus, must establish the fallacy of his induction, and restore physiology to its legitimate rights. Not only such an example, but a multitude of them, occur in the writings of an author who will not be suspected of any disposition to interfere with M. Louis' generalizations. This author is Chomel, so largely interested in the works on Typhus, and Phthisis, now under consideration. The facts are rather old, though but little known, and more frequently neglected. Chomel states that alterations of the glands of Peyer were common in the epi-

(1) Of 614 quarto pages of the Mémoires de l'Académie Royale de Médecine, for 1838, 410 are devoted to an inquiry into the *anatomical* relation which the glands of Peyer bear to typhoid fever. Here, too, we have the humoral pathology in all the exaggeration of a new discovery, and here may be seen how little consideration is paid to the researches of Bichat.

demic Parisian cholera of 1832; so common indeed, that he was disposed to carry M. Louis' philosophy of the dependence of typhus upon the glandular affection to the cholera itself, and to establish an affinity, if not the identity, of these diseases. It is certain, that such had become the ascendency of the Glands of Peyer in "pathological anatomy," that as in the "typhoid affections," those glands were regarded by the sçavans of Paris not only as the veritable *seat* of cholera, but as the *cause* of its morbid phenomena. Thus:—

"On se rappelle que, dans l'épidémie de 1832, les premiers observateurs qui eurent l'occasion d'ouvrir des corps de cholériques crurent avoir trouvé dans la lésion des follicules intestinaux, et le siège de la maladie, et la cause de la plupart des phénomènes morbides qui la caractérisent."

But this is not all which Chomel supplies. He has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina, and other affections, (sujets morts d'affections differentes;) with the exception of the ulceration. He states one case, in erysipelas of the face. (1) In the Mémoire by Montault, it is stated that Louis, Dance, and Rayer, allow of the follicular affection in scarlatina, but without ulceration. (2) But, what claim can the last unimportant lesion of structure have as a diagnostic sign, supposing even that it never happens in a disease which terminates so rapidly as scarlatina? The pathology consists in the inflammation, not in one of its terminations. Did not the "typhoid affection" generally continue many days before it ended fatally? Did the ulceration always happen in the typhoid affection? Was there not a far more violent inflammation of the intestinal mucous membrane than what occurs in scarlatina? And is not the attempted distinction that of a morbid anatomist, rather than of a pathologist having a due regard to vital actions? We may finally say, however, that the "typhoid lesion" of the glands of Peyer is now considered one of the anatomical results of scarlet fever. But, notwithstanding it is allowed by Montault to occur in this disease, and in cholera morbus, and to denote some affinity amongst them and typhus, he asks,—

"La valeur de l'altération folliculeuse dans la fièvre typhoïde n'est-elle pas désormais *consacrée* sous le nom de *caractéristique?*" — *Ut cit.*

Again, numerous observations have been made by different writers, which show that the glands of Peyer are often unaffected in diseases having the characteristic vital signs of the ty-

(1) Leçons Clinique Médicale, pp. 171, 178; 1834.
(2) In Mém. de l'Acad. Roy. de Méd. 1838, p. 256.

phoid affection; which signs, it must be allowed, are fundamental in the present inquiry. In Glasgow, Lombard, as is well known, ascertained that the glands are diseased in only about one third of the dead subjects. In Dublin, the proportion was less. In Liverpool, about the same. In Birmingham, it was only frequent. Lombard had imbibed the French doctrine as to the locality and follicular character of the disease. Subsequent researches convinced him that,—

"It is more a general disease affecting the whole constitution, than a malady depending on any local inflammation, or any local change of structure." (1)

As to his doubtful views of "bilious fever," we see not how the present inquiry is affected by them. (2)

In London, it is stated by Dr. Tweedie, that the proportion of cases in which the follicular affection occurs, is not more than one fourth.

"On the other hand he has met with follicular *ulceration* in individuals the symptoms of whose previous disease had no resemblance to those of typhus fever." (3)

Dr. Perry states the following as the result of 300 anatomical inspections in the genuine typhus, in which the follicular affection is denied to exist by M. Louis and his followers.

"That dothinenteritis, or enlargement of the mucous follicles of the small intestines, and enlargement and ulceration of the aggregated glands of the lower third of the ileum, occur in combination with contagious typhus, and are to be met with in about *one in six*, of those who die of typhus. *It also exists as a disease per se.*" (4)

Dr. Craigie reports 41 dissections, at Edinburgh, of the true typhus, in 7 of which the glands of Peyer were affected. (5) Similar observations are distributed by Prof. Alison, and other distinguished observers, through British and Continental works. With these we shall not encumber our pages. The thing has been settled in Great Britain; and, as we have already said, our present object is of a more general and important nature. That most comprehensive Journal, the British and Foreign Medical Review, in adverting to the controverted identity of typhus, and typhoid fever, remarks that,—

"In our country, we do not require any arguments to persuade us of the

(1) Dublin Journal, 1836.

(2) See his Etudes Cliniques sur divers Points de l'Hist. des Fièvres Bilieuses et Typhoides, 1838.

(3) Cyclopædia of Prac. Med. Lon. Art. *Fever.*

(4) In Edin. Med. and Surg. Journ. vol. xlv. Jan. 1836, p. 68.

(5) Edin. Med. and Surg. Journ. vol. xli. p. 227; vol. xlvi. p. 1; vol. xlvii. p. 285.

identity of these supposed distinct diseases. The evidence is constantly before our eyes." "We think that the condition of Peyer's and Brunner's glands can as little as the previous circumstances of contagiousness and period of life, be referred to as any ground for the distinction of typhus and typhoid fever." (1)

And thus, Roupell: — "The impression on my mind, arising from the correspondence between the leading features as already detailed is, that the two fevers are identical." (2)

Passing to the scene of active operation, where the "typhoid affection" is still seen as originally described, we find M. Gaultier de Claubry, in nearly 200 pages of the Royal Society's Transactions, endeavouring to prove the identity of symptoms and organic lesions in "the typhoid affection" and the typhus of camps, by comparing the latter with the history of the typhoid fever as related by Louis and Chomel. (3)

If we now glance, for a moment, at another hemisphere, we

(1) Oct. 1839, p. 431. (2) Treatise on Typhus Fever, 1839.

(3) Mém. de l'Acad. Roy. de Méd. 1838; p. 1 — 184.

It is said by Dr. Jackson of Boston, in his "Report on Typhoid Fever," (1838,) that, "it is plain that there are at least two species of continued fever, both in Europe and in this country; and farther researches may very possibly show more." The foregoing British Reviewer thinks

"There are no sufficient reasons given in Dr. Jackson's work for maintaining the distinction. The point mainly relied on by him is the affection of the intestinal glands and mucous membrane."

We are bound to say that we have no doubt of the accuracy of the enlightened American observer as it respects the existence of "two species of continued fever" in New England. We were entirely satisfied of the distinction during our pupilage in Boston; and at Montreal we had constant opportunities, for a period of five years, of witnessing the typhus of English authors, and another form in which the usual typhoid symptoms were not presented. We could never perceive any essential difference in the cases which presented the typhoid character. Assuming the vital signs as the only criterion, the most superficial observer would recognise their identity; whilst he would as readily contradistinguish the other form of continued fever. We have witnessed both affections in different parts of the New England States.

But, suppose that the glands of Peyer are affected in a certain number of cases, whilst they are absent in others where the vital signs are the same, and allowing to the anatomist the privilege of erecting a distinction upon this adventitious lesion, what will pathology or therapeutics have gained?

Dr. Hale, in his excellent "Report on the Typhoid Fever of New England," (1839,) is disposed to question the accuracy of the principal authorities whom he quotes adversely to his own conclusions. We cannot think the justification sufficient. It is objected, for instance, that the "*particulars* of Perry's 4000 cases, and 300 inspections, are not given so as to enable us to judge how far he might be influenced in his observations by theoretical views, or with what degree of care and thoroughness his observations were made." We would not have objected to this from M. Louis; but, whilst Dr. Hale gives us an analysis of only 4 of his 197 cases, we confide most implicitly in his general statement as to the remaining 193. (Vol. i. pp. 294 — 298, 303 — 309.)

shall find that we have the same laws in physiology and pathology, as rule in the "Old World."

"It has been our custom," says the accurate Dunglinson, "to examine the condition of the mucous surface of the intestinal tube in fatal cases of fever; but our experience does not enable us to pronounce with Dr. Gerhard, that follicular lesion belongs solely to the form of fever which he has described. On the contrary, we have *frequently* met with it in the *remittent fevers of the country*, — in those very cases, indeed, in which it has not been witnessed by Dr. Gerhard." (1)

Professor Geddings of Baltimore, "has repeatedly met with the follicular lesion in those who have died at *an early period of bilious remittent* and *yellow* fevers," and almost constantly in protracted stages of all fevers. (2)

This supposed peculiarity of the "typhoid affection" is notoriously of common occurrence in the bilious remittent fevers of New-York. We have been informed by Professor Stevens that such is the result of his experience; as also by Dr. Vaché, the able physician of the Bellevue hospital.

"*Dr. Washington*, (in a supposed case of scarlatina,) exhibited a post-mortem specimen, in which the glands of Peyer and of Brunner were very much hypertrophied, although the patient, a little girl five years old, had been sick only 27 hours before death." "*Dr. Buel* related an analogous case in a child, which was apparently well 24 hours before death." (3)

But let us take the candid and distinguished author of the late Report on the Typhoid Fever of New-England, who, in inclining to the general views of M. Louis, suppresses nothing that may advance the truth.

"There is," says Dr. Hale, "another class of cases in which these glands (Peyer's) are affected in a manner *precisely similar* to the affection in that disease, (the typhoid fever.) This occurs in young children, in the diseases of the first dentition."

Dr. Hale then relates six cases. In the first, (dentition, convulsions,)

"Nearly 30 patches were counted." "Many of them were obviously diseased; and, as in typhoid fever, the disease increased towards the lower part of the intestine. They were thickened, and red. Gentlemen recognised the appearances as *precisely resembling* those observed in fever."

Second case, (dentition, cholera infantum,) 25 patches of diseased glands in the small intestine. Third case, (dentition, diarrhœa,) 30 patches of Peyer's glands, elevated and red; six ulcerated, and within nine inches of the cœcum. Fourth case, (dentition, dropsy in the head,) Peyer's glands in the lower part of the ileum thickened and red, and several ulcerated. Fifth

(1) American Medical Intelligencer, April, 1837, p. 11.
(2) Baltimore Medical and Surg. Journ. Oct. 1833, p. 76.
(3) New York Journ. of Medicine and Surgery, Jan. 1840, p. 207.

case, (dentition, inflammation of the lungs,) same appearances, but without ulceration. Sixth case, (dentition, &c.)

"The child had suffered severely from cholera infantum, and during convalescence, was attacked with hooping-cough, which was fatal to him." "Glands of Peyer exhibited obvious marks of having been diseased and cicatrized."

Dr. Hale makes the important remark, that, —

"These are *all* the observations I have had opportunity to make on this point; and I do not know that the attention of any other person has been directed to it."

Here, then, according to the habits of our principal author, was ample ground for establishing the principle that the diseases of dentition have their seat in the glands of Peyer, whose morbid condition is the cause of all the bad symptoms, especially of the hydrocephalic affection and the convulsions. (*See farther on.*) In the same way, it would be equally a rational conclusion, that, as in the case of the malignant cholera, there is a close affinity between the diseases of dentition and typhoid fever. But, Dr. Hale remarks, that, —

"I do not regard them, (the six observations,) as sufficient to establish any general pathological fact, except that of *an exception* to the general exemption of Peyer's glands from any morbid affection in acute diseases other than typhoid fever." (?)

After what we have hitherto shown, and from the existence of other analogous records, it is scarcely necessary to advert to Dr. Hale's remark, that, —

"I know of no case of any acute disease, besides typhoid fever, in the adult patient, in which the glands of Peyer have been found diseased." (1)

Whilst M. Louis was employed in investigating the yellow fever of Gibraltar, in 1828, he met with cases of a disease having his reputed anatomical lesion of typhoid fever; but so coincident were these cases with the general epidemic, that he contradistinguished them mainly by this lesion of the glands of Peyer, and an absence of what he regards as an affection of the liver peculiar to yellow fever, but which has been rarely, if ever observed in other countries. As to the vital signs, we see nothing to distinguish the cases from some which are set down as examples of yellow fever. (2) Greater modifications are observed in all epidemics. Nor is it a very probable coincidence that two epidemic fevers, specifically distinct, and with which our author's celebrity is so nearly connected, should have prevailed simultaneously at the place of our author's exploring expedition. (Vol.

(1) Report to the Massachusetts Medical Society on the Typhoid Fever of New-England, 1839, p. 38 — 41.

(2) On the Yellow Fever of Gibraltar, pp. 85, 128, 138, 152, 297, 309.

I. p. 451, *note.*) We shall see, however, in respect to our author's Parisian cases of typhoid fever, that there is great reason to believe, that whilst some were in reality a constitutional fever, more were constituted by simply local affections. But, the glands of Peyer being affected in all the 50 cases, our author carried out, in this instance, his general doctrine, that, —

"*An excellent mode of arresting diseases is to confound them;*" (1) since, "*what importance is it, with respect to effects, for us to know what is the* CAUSE *of trouble in our functions, when this trouble is of a serious nature.*" (2)

Such then is the fruit of our author's first generalization of 50 cases; designed for every climate, constitution, habit, and other predisposing and exciting causes; — to which we shall ultimately add a certain contribution from the treatment of the foregoing cases.

We shall now rapidly glance at the prolific results of those 50 cases of typhus; or, perhaps, we should rather embrace the favourable as well as the fatal instances, — the entire number swelling the amount to "130 observations." To this should be also added our author's comparison of the foregoing cases with a very limited number of other diseases; to wit, "83 subjects whose histories I learned." Although the inductions are avowedly founded upon the foregoing number of cases, we are disposed to allow any latitude of observation which it may be supposed our author can have made. Nevertheless, it would be more correct to apply to our author the rule by which he restricts all others, and of which the following is one of the numerous instances of its derogatory insinuations: —

"Corvisart does not state how he proves the truth of the assertion which he tries to explain. The reader may seek for the proof in his work, but, where will the proof be found?" "It may be answered to this, that Corvisart had not observed those cases merely, the histories of which he has published, but that his *assertions* rest upon a *much larger number.* But, even in this case, Corvisart *should have counted*, and if he had done so, *he would have stated the fact, since the question was simply one of number.*" (3)

"*Common sense,*" says M. Navier, in his encomium upon the *numerical system*, "shows that if *the conclusion that an operation saves eight out of ten patients is deduced from only a small number of facts, it becomes of little weight.*" (4)

As we shall advert to the conclusions of our author which respect other diseases, we may also state here, that his *generalizations* in the work on Bloodletting relate, in the 1st Chapter, to 78 cases of *pleuropneumonia*, of which 28, or more than one third,

(1) On Bloodletting, p. 31. (2) On Typhoid Fever, vol. ii. p. 145.
(3) Memoir on the Proper Method of examining a Patient. *Tr.* (4) In Ibid.

proved fatal; "*all being in a state of health when the first symptoms were developed.*" Of the subjects of *Erysipelas*, there were 33; and of *angina tonsillaris*, 35. In the 2d Chapter are 29 cases of *pneumonitis*, and 11 of *Erysipelas*; which were published subsequently to the first series. In the work on Phthisis, "those of which we now offer the result," says our author, "are in number 123."

In the meantime, in pursuing our author's generalizations, the nature of the subject, and the treatment employed, should be duly considered, and that the generalizations are to be taken as the consummation of the "numerical method," and as subversive of all former experience; at least, so far as the generalizations extend. And when we consider their collateral, as well as their specific import, and regard the equally summary generalizations in respect to several other most important diseases, we cannot fail to perceive that our author has aspired at the hope of completing the analysis, if not of fixing the treatment, of the principal maladies which have engaged the profound attention of philosophers for the many ages which he covers with oblivion. We believe that we state this proposition in strict conformity with our introductory extracts, and with what will subsequently appear. Certain we are, that we would avoid all injustice, and that we are in no other respect actuated by our author's condemnation of the past, and by his departure from the spirit of the "numerical method," than by what is due to his cotemporaries, to the mighty dead, to posterity, to science, and to truth. We are fully sensible that nothing but the extraordinary circumstances attending the connection of our author's works with the medical literature of the age, the example of hasty and fallacious generalizations which he has but too successfully exhibited to the profession, the collision of his doctrines with the most obvious philosophy, and their unhappy nature in a practical sense, could justify the accumulation, in the body of our work, of such an unexampled assemblage of inductions bearing the stamp of "mathematical exactness." But, it is indispensable to the success of our enterprise; and when we shall have brought them to the solemn consideration of the reader, yet leaving them mainly to his own intelligence, we cannot but think that they will be regarded as a fearful beacon to the present and coming generations.

Let us, however, in the first place, present a fair example of

our author's inductive philosophy, by which we may the better appreciate the merit of his generalizations, and perhaps come to understand something of the source of those dogmas in physiology and medical science which have so extensively usurped the sway of all former experience and philosophy.

All our author's general conclusions, as we have said, repose in a great measure upon analogical reasoning; this reasoning being generally from the dead to the living body. And, whilst our author thus resorts to the direct aid of analogy to carry out his generalizations from a very limited number of cases in morbid anatomy, and sometimes from a solitary example, he is as much opposed to it when employed by others, as he is imperative as to the exclusive right of generalizing himself. Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure; and when he endeavours to insinuate the belief that diarrhœa cannot exist "without *appreciable lesion* of the intestinal mucous membrane," he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body. He therefore says:

"We may presume that it may exist without, from the analogy existing between diarrhœa and more or less profuse perspiration." But, "of this we cannot be positive; for in OUR OPINION ANALOGY IS ONLY USEFUL TO POINT OUT FRESH SUBJECTS FOR INVESTIGATION, *to lead us to the discovery of facts*," (1) &c.

Such is ever the winding up of our author's syllogisms. You are left with the belief that analogy is but little better than an absurdity; whilst you are crowded with generalizations of a most novel and important nature "that have nothing to recommend them" but a feeble analogy founded, at best, upon the debris of a few wretched inmates of a Parisian hospital. Not the slightest distinction is vouchsafed betwixt the different degrees of this species of evidence, of which there is every shade from an almost moral certainty, to the slightest degree of probability. Thus, in the example which our author fears may encroach upon the dominion of morbid anatomy, who is there that will not concede that "profuse perspiration," arising *from disease* without "any appreciable lesion" of the skin, is not a substantial ground for induction, that "diarrhœa," — aye, and many other morbid results, — may take place independently of any "appreciable lesion" of structure? And, to show you how analogy may grow into a matter of fact, and in this very instance, we will point you

(1) On Phthisis, s. 259.

to serous effusions in the brain, thorax, abdomen, where the secreting membranes often exhibit their perfectly normal state.

Our author has no difficulty with analogy where a lesion of structure may embellish the philosophy of disease. Thus:

"Analogy," he says, "is in favour of what we advance. For, when hemorrhage occurs in any internal organ, it is almost constantly a symptom of more or less considerable alteration of structure."

From this assumption, he reasons analogically that,

"Hæmoptysis, (with certain exceptions,) whenever it occurs, renders tubercles in the lungs infinitely probable." (1)

Here, as we shall see in many other instances, our author makes a demonstrative, though a bad use of analogy; since, his premises being just otherwise, analogy operates against him. Nor is this all; for the induction which he would thus establish in regard to hæmoptysis is without any foundation.

The following is a very common example of our author's use of the numerical system, and analogy. He makes intestinal perforation peculiar to the "typhoid affection;" and its resulting symptoms diagnostic of the disease where its character had been before latent. The generalizations are founded upon 5 out of 8 observations. "Very severe symptoms announced it in 5 patients; it was, if I may say so, *latent* in 3 others." From this large amount of facts our author draws the "rigorous conclusion,"

"If the disease," [that is to say, the nature of the typhoid affection,] "have been *latent till that moment*, and there supervene suddenly in a patient with diarrhœa, pains in the abdomen, aggravated by pressure, accompanied by loss of expression in the features, and more or less speedily, by nausea and vomiting, *there must be a perforation of the small intestine.*" (2)

The disease may then be assumed to be the true "typhoid affection;" although the foregoing are exactly the symptoms of common intestinal inflammation, attended by perforation, and often occur without perforation.

Here, again, is not an unusual instance, as we shall show, of our author's use of *analogy* and the "numerical system."

"It is only necessary," he says, "to have met A SINGLE EXAMPLE, TO FEEL CONVINCED that *in many instances, inflammation and tubercles in the lungs are independent of each other.*" (3)

Our present subject having very important bearings, we shall present an instance, in which it will appear that our author inadvertently admits the BROAD PRINCIPLE upon which he has proceeded in laying the foundation of his renown, and tacitly allows that his "opinion," on another occasion, that "*analogy is*

(1) Ibid. s. 233. (2) On Typhoid Fever, vol. ii. p. 374. (3) On Phthisis, s. 478.

only useful to point out fresh subjects for investigation," was designed for some special purposes. Thus:

"How, then, can we believe that the effect of a blister is to check an inflammation, when this blister is one inflammation superadded to another? I acknowlege that this method of reasoning was not *rigorous;* it was *mere reasoning by analogy.* [Now mark.] But it was an analogy drawn not from animals to man, not from the man in health to the man in disease; *but from the sick to the sick himself;* almost A CERTAINTY." (1) (See *Appendix on Analogy*, pp. 574, 576.)

As there are some important principles involved in the foregoing quotation, we shall give it some attention. In the first place, it is allowed by an unexampled advocate of "rigorous facts," that when we reason from "the sick to the sick himself," the analogy amounts "almost to a certainty." This is practically our author's rule, though it be often wholly farther than is true. But, it suited our author's purpose in the foregoing instance to have it so; whilst the analogy is very defective in the sense in which he employs it. We will show, indeed, by this very proposition, that our author has here given to analogy that factitious value which he has infused into all his generalizations. We shall show, too, by this instance, how analogy in any given case may possess all the value which our author has here ascribed to it;—that it operates, however, in two opposite ways, and that the true one is directly against the induction of our author as to the bad effect of blisters in inflammations.

It will be seen by our quotations hereafter, that our author has declared a general hostility against bloodletting, and that in the enteritic and other formidable inflammations of the "typhoid affection," in pneumonia, &c., he vesicated his patients with little or no antecedent bloodletting; especially in the "typhoid affection." Now, all experience shows that blisters, under such circumstances, have generally aggravated the disease. This, therefore, is one instance in which analogy is sound and important; and we may safely reason from it, and condemn the practice which makes a complete substitution of blisters for bloodletting. Were we to stop here, then, we should say that our author is right, and reasons like a philosopher. But it happens, on the other hand, that all the experience which our author abolishes has shown that blisters are generally inoffensive, and diminish inflammation, after bloodletting has been properly premised. It is therefore, in this acceptation, "almost a certainty"

(1) On Bloodletting, p. 46.

from analogy, that, when blisters are employed in subordination to bloodletting, they will promote the cure of inflammations. We also entertain the belief, that when we come to other important curative means which have been hitherto employed in treating inflammations, &c., but which are condemned by our author on the ground of his "numerical" experience in from 1 to 20 cases, and contrast his results with those of former times, the reader will be apt to agree with us that it is quite time to recur to that experience for which our author would substitute his own.

In the last of the foregoing quotations, we have also an example of our author's winning mode of reasoning, and by which he has carried the world, as it were, by enchantment. First it was *mere* analogy. Then we have the contrast betwixt *man* and *beast*, and next between the man *in health* and the man *in disease*. All this quite prepares us for the transition from "*mere analogy*," so utterly worthless in the hands of others, to "almost *a certainty*" in our author's. For, what other intelligible motive could our author have had, since nobody has ever thought of applying blisters to brutes, or even to the healthy man, for the purpose of inferring their effect upon "the man in disease"? And all the world knows that what will cure a sick man may kill a well one. (1)

But, from what our author has here said of the "certainty" of our conclusions when reasoning "*from the sick to the sick himself*," it may be inadvertently supposed that our author's generalizations and inductions are predicated of that mode of reasoning. Such, however, is rarely the case. The whole of our author's stupendous fabric reposes upon morbid anatomy, and upon reasoning from *the dead* "to the *sick* himself." It is, indeed, this very fact which has placed our author at the head of the anatomical school, and which has thrown such a *vital* charm about the debris of the body. Whether, therefore, it will not be considered safer and more luminous to follow Magendie's rule of reasoning "from animals to man," till the Hippocratic philosophy be restored to medicine, we believe will become a subject of grave deliberation by the schools.

(1) We thank our author, however, for the implied condemnation of the system which has been long going on, and now waxes more than ever, of applying the results of experiments upon the brute creation to the modified laws and organization of man, and of a prevailing habit of reasoning from "the man in health to the man in disease," and vice versa. (Vol. i. p. 691 — 698.)

Here is another example of adherence to "rigorous facts," and of the uses which our author makes of analogy, where questions of the most vital and general nature are concerned. Thus:

"Experience shows us that, in spite of these striking and indisputable differences between persons most resembling one another, 999 out of 1000, who differ in age, sex, temperament, &c., live on the same food, prepared in the same manner."

In the next sentence the implied application of the foregoing principle is even made to violate the rule which we have just quoted from our author, that analogy is worthless in the relations which the sick man bears to the man in health. Thus:

"Experience *also* shows that certain medicines, administered in the same disease, to individuals of different age, strength, temperament, &c., are almost *uniformly successful*." (1)

We will have but one more illustration at present of our author's habits, and of their dangerous tendencies, whether philosophically or practically considered. The analogy is founded upon the "*extremely limited*" effect of bloodletting and leeching in 13 cases of angina tonsillaris; *mainly*, however, upon 3 cases. Our author then argues that, —

"From the *same facts we should be led to question* the great advantage of the application of leeches to the *epigastrium in gastritis, or to any other part of the abdomen corresponding to the viscera presumed to be diseased.*"

The next words are, as if in self-rebuke,

"Indeed, what confidence can be placed on the *a priori* precepts commonly advanced on this subject?" (2)

Nevertheless, our author's "rigorous facts and conclusions" being allowed to be paramount to all former experience, or what he denominates "*a priori* precepts," we may find in his writings the groundwork of many of the prevailing errors which we have endeavoured to indicate.

In now proceeding to state our author's generalizations, we shall endeavour to avoid such as appear to be more or less qualified. We shall place a marginal index to each quotation, as we can only cite our author's conclusions, abstractedly; and this we have sometimes done by taking a part of a sentence, but always in our author's language, and without in the least affecting his intended meaning as we have understood it. The *Italics* and *Capitals* are ours; which we have employed here, as at other times, in the place of comment.

(1) On Bloodletting. p. 58.

(2) On Bloodletting, p. 21.

3. "It follows, certainly, that the typhus, ataxic, or *putrid* fever, is no more a *gastro-enterite* than a pneumonia is a gastro-pneumonia."(1)

4. "So that, all that we can deduce from the facts I have given, and *this conclusion* is very important, is this, to wit: *in every case* in which an acute affection *of any nature* gives rise to a febrile excitement lasting some time, the mucous membrane of the stomach becomes, at a period which varies according to the nature of the disease, the seat of a lesion," &c. (2)

5. "These frequent lesions of the mucous membrane of the alimentary canal and of a variety of other organs, in patients who died of *acute diseases of any nature*, *prove* that when an affection of this nature *gives rise to a febrile excitement of any duration*, the MAJORITY of the organs of the body become the seats of *more or less serious lesions*." (3)

This is an universal induction as to all fevers from our author's 50 cases of the typhoid affection.

6. "The mucous membrane, of the alimentary canal, [in the typhoid affection] is not oftener, in fact it is less frequently, diseased than some others, the spleen for example, &c. This is an *important* LAW, and it may tend, as it appears to me, to *simplify much the study of pathology.* This is what we ought, perhaps, to have discovered, A PRIORI"! (4) (See p. 682, *Sprengell*, and what our author says of "*a priori*" reasoning, p. 680—681.)

This is also a striking exemplification of our author's disregard of the symptoms of disease, and that the whole of his pathology consists in lesions of structure; since in all the fatal cases of the "typhoid affection" but three, there occurred diarrhœa and pains in the abdomen, and they were generally the earliest symptoms. And, again,

7. "*Admitting* as generally true, &c., it follows that nearly a third part of the subjects *who recover* from a more or less serious typhoid affection, experience some *alteration*, either slight or severe, but *always appreciable*, of the mucous membrane of the stomach."—This "law" is deduced from observations upon the dead subjects.

"This affection, [*gastritis*,] upon which the *new* medical doctrine *depends in a great measure*, is really one of the *least known*, and about which *the fewest* positive results have been given, and whose diagnosis is the most obscure." "I have no definite idea of the value of the group of symptoms designated by the term *gastric embarrassment.*" (5)—Nevertheless, our author allows that, in the typhoid affection, "the mucous membrane of the stomach was, in the greater proportion of cases, more or less seriously altered; sometimes *softened* and thickened, or even *destroyed*, *ulcerated*," &c. (6)

Now observe the following, in connection with the last of the preceding paragraph.

(1) This generalization, it is well known, is extensively controverted.

(2) Typhoid Affection, vol. i. p. 161.—Here it should be understood, that our author means *structural* lesion; which, as will appear, is at all times regarded by our author as the essential pathology of disease.

(3) Vol. i. p. 382. (4) Vol. i. p. 382. (5) Vol. ii. pp. 44, 45. (6) Vol. i. p. 152.

8. "It is nearly correct to state, that the apparent condition of the brain *cannot explain the symptoms* of which it is the source, any more than the *mucous membrane of the stomach* can account for the *anorexia and other gastric symptoms* in the great majority of cases."—Then follows the declaration that, "we must infer that it is in the lesion of the glands of Peyer, and *not in any other*, that we must look for the cause of the delirium," &c.; which is also extended to the gastric symptoms, (1)—The foregoing induction should be considered also in connection with what will be stated as to the morbid anatomy of the brain. (See, likewise, p. 431—433.)

9. "It seems extremely probable to my mind that, in a certain number of cases, the lesion [of the stomach] is not the result of inflammation, because we find no evident traces of inflammation about the part thus softened, atrophied, and pale." (2)

10. "What deduction shall we make from these facts? Must we admit that the *white* and *red softening* have each their own causes; the one *wholly different from the other*? This question seems to me may now be *decided* affirmatively, at least, in certain cases;" that is to say, in the typhoid affection. "I am far from admitting, therefore, that the softening of the mucous membrane of the small intestine is always inflammatory. On the contrary, it seems to me NECESSARY TO ADMIT THAT IT IS OF AN ENTIRELY DIFFERENT CHARACTER IN CERTAIN PERSONS." (3) (P. 548, *Andral.*)

11. "The morbid change of the *spleen* has something *special and characteristic* about it, in patients who are attacked with the typhoid affection." (4)

12. "To what causes shall we attribute these different lesions [of the *spleen*] we have now examined? Considering merely the *softening* and *increased size* of the spleen, *which were generally connected*, one would be led to suppose that they were the product of inflammation," &c. "But other facts, as it appears to me, do not allow us to stop at this conclusion." "*The most marked characteristic of inflammation, pus*, was found in not a single case." "How can we concieve that inflammation, *which was so active that it produced*, sooner or later, *these changes* in size and softening above described, should not have given rise, in a single case, to the formation of a quantity of *pus*?" (5)

"We must conclude from the previous remarks that, *in the present state of science*, we cannot decide *as to the nature* of this lesion." (6) (P. 324—327, *note.*)

13. "If the *softening* of the *spleen*, connected as it often was with *increased dimensions* and a *darker colour* than natural, could be attributed, at first sight, to inflammation, such is not the case with softening of the liver; since it was generally *pale*, was of its normal size, or appeared smaller than usual; was dry, and contained less liquid than usual; so that, if one were acquainted with any pathological state THE REVERSE OF INFLAMMATION, IT WOULD BE RIGHT TO REFER THIS AFFECTION TO IT."

Nevertheless, "the colour of the liver *was redder than usual* in eight individuals, and in five of these was more or less marked *congestion of blood*." "In

(1) Vol. ii. pp. 131, 132. (2) Vol. i. p. 162. (3) Vol. i. pp, 170, 171.

(4) Vol. i p. 258.

(5) Our author allows that "pus has been but seldom found in the substance of the spleen."

(6) Ibid. pp. 253, 254, 255.

one case the liver had a yellow colour, upon which were a great number of *vivid amaranthine red spots*, star shaped, an inch or more large *in the substance* of the organ as well as externally." (1)

We regard this, like the preceding, not only as an important generalization in relation to a particular disease, but in respect to its violation of analogies in this and other complaints, and of the fundamental principles of inflammation. "*Softening*," says our author, "the gravest and most marked of all the lesions found in the liver, was observed in 22 subjects, or very nearly half the cases, and to a very remarkable degree in 4 of them. Like that of the spleen, it was *universal*." — (P. 259.)

It should be borne in mind that most of our author's fatal cases were of a lingering nature.

14. "The *heart* had less consistence than natural in 24 subjects." "It was generally *livid and purplish* on its surface as in its *substance*. The internal face of the ventricles and auricles was, on the contrary, of a *deep violet-red* colour." "As I remarked in relation to the softening of the liver, if we knew any cause of disease EXACTLY THE REVERSE OF INFLAMMATION, IT WOULD BE PROPER TO REFER THIS SOFTENING TO IT." (2)

Here as in the case of the spleen, our author refers to an absence of "*pus* in the *walls* of the heart," as a special proof of the foregoing doctrine. But, the conclusion which our author would here derive from a common result of inflammation is only an evidence that he is more in favour of that analogy which he ostensibly rejects, than of "rigorous facts"; since undoubted inflammation of the heart has little or no disposition to end in suppuration. Even COLOUR is disregarded.

15. "Although the diarrhœa was very great, the mucous membrane of the colon presented no evident traces of inflammation, *inasmuch as the thickening may be considered as the consequence of the rëaction of the meteorism*"! (3)

Our next generalization, or perhaps we should rather say, fundamental principle, and one of no little importance, comes from our author's work on Phthisis. And since our author was now engaged in maturing his plan as to the "Typhoid Affection," we must regard it as preparatory to the discussion of that disease, where it is extensively carried out.

16. "*Intestinal ulcerations were often, at least, in their origin, independent of inflammation.* This was evidently the case with a *great number* of those in the *small* intestine." (4)

Here we pause to consider how far our author has supplied any ground for his principle that softening, thickening, and ul-

(1) Vol. i. pp. 260, 262. (2) Vol. i. pp. 282, 284. (3) Vol. i. p. 324.
(4) On Phthisis, s. 144.

ceration, of different tissues, sometimes depend upon inflammation, and at other times on an "exactly opposite condition of disease," and what, also, is the probable motive for introducing this confusion into the most important branch of pathology.

In the first place, the inductions are founded *wholly upon the debris of the body*. There is no where, that we have been able to discover, any essential reference to the phenomena of the disease during its actual existence. Even the remarkable similarity of those phenomena appears not to have been held in consideration, in forming the conclusions. Secondly, the inductions rest chiefly upon the fact that the foregoing alterations of structure are *white* in one case, and *red* in the other. (P. 548, *Andral.*) This may be very philosophical; but let us see what our author thinks of it when he is engaged in reasoning the reader into his problem, and in supplying the appearance of an impartiality which never fails of a prepossessing influence, and carries us along with greater confidence to the never-failing act of generalization. But, we have even more than this; — a direct contradiction of his own philosophy as it respects the *very important* tests of colour, by which our author comes at last at the conclusion that the foregoing lesions are owing in "the typhoid fever," at different times, to exactly opposite pathological conditions. Thus: —

"*Paleness of inflamed structures takes place sooner or later, as is exemplified in the various shades of colour of hepatized lung.*"

"It ought to be noticed, that continuous with a red and softened portion of mucous membrane, we often find another equally softened, but *without redness. If the first, therefore, is inflammotory, it is probable that the other is also.*"

Here, too, he allows that "thickening of the sub-mucous cellular tissue" was "an evident result of inflammation," although "recent" and "retaining its natural paleness." Whereupon, our author lays down a rule which it was convenient to abandon in expounding the lesions of "the typhoid affection." Thus: —

"This fact, [the foregoing,] with *many others*, shows that the thickness of our tissues is one of the most important circumstances to be noticed, and that to *confine ourselves* to the *description of the colour* of membranes is often *useless* and even *a cause of error to those who might draw conclusions from imperfectly described* facts." (1) And again, in his *Preface*, "*redness*, considered by itself, offers much less interest," than "*thickening, softening*," &c.

Such was the opinion of our author when reasoning abstractedly upon the results of inflammation, in his work on Phthisis.

(1) On Phthisis, sec. 135, 136.

But, he was also simultaneously engaged about "the typhoid affection"; and hence we have in the work on Phthisis some ambiguous conclusions as to the dependence of the foregoing lesions of structure upon "exactly opposite conditions of disease," as their colour might happen to be *red* or *white*. When, however, we come to the work on Typhus, the obscurity is cleared up; and, in a general sense we are told that *red* and *white* must be taken as the ground of an absolute distinction between the pathological causes of such lesions of structure as may be otherwise in all respects alike, and characterized by the same vital phenomena. Nevertheless, it was important to attempt a consistency of doctrine with what had been laid down in the work on Phthisis; and this could readily be done by those occasional guarded contradictions which give to an author a reputation for candour that more effectually secures to him a successful propagation of his favourite, though conflicting hypothesis. Having said this, we are now bound to cite an instance in illustration; and this we do the more readily, as it exhibits, in connection with what we have hitherto quoted from our author, the true foundation of the new philosophy in respect to inflammation and its products, and explains how far morbid anatomy, and specific objects, have been the source of certain existing collisions with the fundamental laws of nature. (Pp. 181 — 204, 316 — 331, 623 — 629, 635 — 640.) Thus, then, our author; —

"I refer to the sub-mucous membrane of the large intestine, which was very firm, and at least *six times thicker* than natural, and of a *whitish* colour. This *thickening, we cannot doubt, was consequent upon an inflammation of the mucous membrane*, but not *recent*, for the *white* colour is inconsistent with the idea of *acute* inflammation." (1)

The reader should here regard in their proper connection and involution, the expressions, "but not recent," and "acute inflammation." Take another passage, in which we have a grouping of the whole subject, and the doctrine so laid down, that our author may turn it either way, according to particular exigencies, or as "the typhoid affection" may require, in a general sense, some pathological condition "opposite to that of inflammation" as the source of many structural lesions, especially those of the intestinal mucous membrane.

"There is no doubt," he says, "that in the first two cases the *softening* was *inflammatory*, and that such also was the fact in the third and fourth, in *those spots* where there was a *red colour*. But was this the fact for *those spots* where

(1) On Typhoid Fever, vol. ii. p. 280.

the mucous membrane was *pale;* and the fifth case in which throughout the whole extent of the canal there was merely a *gray* colour? If we cannot *exactly understand* how *two contiguous* portions of the same organ, which were *equally* thickened and softened, should be *one white*, the other *red*, their thickening and softening being owing to the *same cause*, we can with much less ease understand how this cause can be put aside as not the *true one*, merely because one of its effects was not found, perhaps had disappeared, the two others, the most grave, remaining in a most remarkable degree. Thus it seems to be *nearly certain* that in the third and fourth cases there was inflammation of the mucous membrane through nearly its *whole* extent. This probably was the case in the fifth case, since the *gray* colour appeared to be very often a more or less *remote consequence* of the inflammatory *red* tint, and because in this case the glands of the mesocolon presented all the appearances of severe inflammation. This *last fact has* not as we shall soon see, *all the value one would be inclined to give it, relative to the object which now occupies us.*" (1)

In speculating upon the pathology of the mesenteric glands, he says, —

"Instead of being *red*, these glands had a bluish or grayish aspect, in *one case* in which the disease went its course very slowly; and this circumstance would tend to make us believe, if we remember the facts given previously, that the *red colour had existed before* at a certain period of the disease, but that it had *retrograded afterwards.*"

They were softened, or thickened, and whatever the colour, our author ascribes those results as affecting the mesenteric glands, to inflammation; and this, because their state was more or less "in harmony with that of the elliptical patches, whether ulcerated or not." (2) We state this general fact now, and reserve *his* contradiction for another place. We shall, therefore, make no farther comment at present upon these mystifying passages, than to say that we here have it again, in "*red* and *white*," that the author finds no other ground for this trifling with pathology than the *colour* of parts. (3) From beginning to end, we find that the "changes are rung" upon the "colour" of the part, and almost wholly upon the colour alone. If softening, thickening, or ulceration, be *red* at its margin, it depends *there* upon inflammation; but the centre of the same lesion being *white*, it is *there* owing to "an exactly opposite cause," especially in typhoid fever; whilst these two "opposite" causes act together in concert in producing that part of the lesion which is intermediate between the centre and circumference, and either in propor-

(1) Ibid. vol. i. p. 203. (2) Vol. i. pp. 231, 233.

(3) It is on a very similar occasion that Plato adverts to the advantages of the understanding over sense; where an object was assumed to possess a certain character, "because it had a *florid* colour, or some particular shape;" (Η οτι χρωμα ενανθες εχων, η σχημα.)

tion as the hue may gain upon the *red*, or as this may fade away into the mysterious *whiteness*. How harmonious is all this with those practical views which have resulted from our author's method of interrogating nature.

17. "What importance is it," he says, "with respect to effects, for us to know what is the *cause* of trouble in our functions, when this trouble is of a serious nature"? (1)

18. And as to the treatment,—"need I repeat that an excellent mode of arresting diseases is to *confound them*; or, at least, to make *no distinction in the periods, at which such and such remedies were employed?*" (2)

Certainly a proper commentary on the "numerical method."

> "Such continual zigzags in a book,
> Such drunken reelings have an awkward look,
> And I had rather creep to what is true,
> Than rove and stagger with no mark in view." (3)

After our author published his book on Phthisis, which is scarcely less remarkable for its generalizations from a small number of observations, he was to surprise the world with a variety of new hypotheses and principles founded upon the typhoid affection. It appears to have been a primary object to cover this disease with mysteries; and, since it would have been inconsistent, as will be seen, with our author's plan to admit the doctrine, that the primary cause of the ulcerations, &c., is an intestinal inflammation, we may find in this consideration a motive for the never ending attempts to show by the *red* and *white* alterations of the intestinal mucous membrane, that the lesions of that organ were owing to a condition of disease *opposite* to that of inflammation, "at least in a certain number of cases." This would answer the purposes of the whole project as to the glands of Peyer, which will be set forth in the sequel. It shuts out the hypothesis that the fever and other sequelæ are referable to muco-intestinal inflammation, and, above all, that the lesion of the glands of Peyer is not the result, as it undoubtedly is, of the propagation of inflammation from the mucous surface; although, "*ulceration was large, serious, and almost always present in the small intestine.*" (4) And that our author's doctrine as to

(1) Vol. ii. p. 145. "*Qu' importe* d'ailleurs, relativement aux effets, la cause du trouble de nos fonctions, quand ce trouble est considerable? — *Recherches*, &c. de Fièvre Typhoïde, &c. t. 2. p. 171.

(2) On Bloodletting, p. 31. (3) Cowper's Conversation.

(4) Vol. i. p. 198. — Compare this with our author's statement in Gen. 6, as to the infrequency of disease in the small intestine. We find an instance in which our author probably intends to molify the exclusiveness of his doctrine as to the glands of Peyer, "in a certain case." It is set forth in his very "First Observation," that,—

"The cellular membrane of the patches had undergone an alteration analogous to

the paramount importance of the glands of Peyer might obtain that ascendency which it has enjoyed, by cutting off the possibility that the lesion of the glands may be determined by an inflammatory state of the intestinal mucous membrane, we are told, that here, also, "in a certain number of cases," the various lesions had no dependence on inflammation; that for instance,—

19. "The *colour* of the elliptical patches of the ileum with that of the corresponding mesenteric glands, seems to me to show that in this case as in the other, inflammation had but a small share in the production of the ulcers." So in another instance, "we are led to conclude that in this case inflammation had but a small share in the production of the morbid changes of the patches, and that *ulceration was the principal tendency*"! And yet another: "the inflammatory stage had, doubtless, always been slight, and, as in the preceding case, had exercised but little influence upon the ulceration." (1) And so on.

Having thus settled the fundamental principle that *white* softening, thickening, and ulceration, depend upon a state of disease, "exactly opposite to that of inflammation," "in a certain number of cases," our author applies it, as we have seen in part, with all the advantages of mysterious novelty, to every organ in the body, where the lesions of structure would not be denied by his friends to depend most palpably upon inflammation.

But, as our author might seem to allow that the intestinal affection was really inflammatory "in a certain number of cases," we have a constant recurrence of a general principle, which only ceases at the close of the work, where we are again reminded

20. "Of the *specific* alteration of the small intestine." (2)

Such are some of the first fruits of the "new method of observing."

"The Frenchman forgot," in the language of Beddoes, "that neither fever nor inflammation continues (qualis ab incepto processerit,) to be what it was at first; and by *mechanically going on*, he lost the advantage of setting out right." (3)

that of the mucous membrane; and where the mucous membrane was *thickened* and yet of a *whitish* colour, the cellular was of the *same colour*, and proportionally as *thick*, so that it is impossible *rigorously to demonstrate* whether these two tissues, the cellular membrane and the crypts, were attacked in succession, or simultaneously." (*a*)

Here, too, the reader will remark that there is no discrimination made in regard to the difference in the vital conditions and manifestations of the mucous membrane and the glands of Peyer, but the possible simultaneousness of their attack is decided, as it is every where else, upon the ground alone of a coincidence in their lesion of structure.

(1) Vol. ii. pp. 294, 288, 301. Also, Vol. i. pp. 170, 171, 161, 162, 223, 260, &c.

(2) Vol. ii. p. 449.

(3) Beddoes, on Fever and Inflammation, p. 166.

(*a*) Vol. i. p. 12.

But, as if in farther anticipation of the objection which would be sooner or later made to this scheme of subversion, or to conduct the reader, not yet prepared for a renunciation of his former knowledge and principles, to a less reluctant acquiescence, and, especially in view, as we shall see, of the consummation of the anatomical doctrines that all disease consists in absolute lesions of structure, and that that which is most frequently disorganized is to be taken as the true seat of all continued fevers, it is *incorrectly* stated by our author, that,

"Nevertheless, I am *far from pretending* that we must allow those organs only to have been inflamed, which we find at the same time reddened, thickened, and softened, or merely softened and reddened. The history of the mucous membranes is a proof to the contrary." (1)

We say incorrectly; since the proof to the contrary abounds in every part of our author's work, and will be fully exemplified in the sequel. Before, however, leaving the important subject of colour, by which our author has so successfully invaded the most obvious and important principles in pathology, we shall state one more of those occasional submissions to fact, or of those peace-offerings, by which our author proves his own error, and that truth must ultimately prevail.

"Among those," he says, "who died after the thirtieth day, the elliptical patches, whether ulcerated or not, presented the following modifications.

"Their COLOUR, which had been, as already mentioned, of a RED hue, which varied in intensity, was changed to a MIXTURE OF RED, of GRAY, and of BLUE, in VARIOUS PROPORTIONS, or they had a GRAYISH or BLUISH appearance without any RED TINT, and they were not so thick, and were of a more consistent texture than the RED patches were, so that the less they had of a RED colour the less thick and soft they were.

"These different SHADES OF COLOUR from the RED to the GRAY, these different degrees of softening, of thickening in the manner and according to the LAW OF PROGRESSION already stated, in cases where death took place long after the commencement of the disease, seem to me to demonstrate that these patches in which they were found, had had, at a certain period of the disease, all the characters belonging to the RED patches; and the differences, observed between them and these latter ones, were the consequences of a retrograde course nature had commenced for the ultimate restoration of these patches to a healthy state. The *submucous* tissue presented the same alterations of COLOUR, consistence, and thickness, whether the patches were ulcerated or not." (2)

Frequent affirmations, like the following, leave no doubt with

(1) Vol. i. p. 172.

(2) Vol. i. p. 176. — As to our author's constant disquisition about the *colour* of parts, we may say of them as did his Majesty to the Cardinal of Evereux, — "these flowers are like blue, and yellow, and red flowers in the corn, which make a pleasant show to those that look on, but they *hurt the corn*."

the lover of novelty, or the superficial inquirer, that he is really engaged with our author in the analysis of a disease which proves all former experience as much in the wrong, and all fundamental principles as visionary, as the discovery of the trilobite, or saurian reptiles, or the "tracks of the tortoise on the sands of Scotland," (1) prove that "this earth has existed for millions and millions of ages," and "underwent a remodelling," and a fresh stocking with animals, at eras which are "conclusively" shown by the "*debris*" of such as are assumed to be "extinct." Thus, then, our author:

"We cannot establish a comparison between the *typhoid affection* and *proper enteritis;* and it would be as unreasonable to *confound* all the acute affections of the alimentary canal together, merely because they are all accompanied by diarrhœa, as it would be to confound those of the lungs, because all of them are accompanied by more or less dyspnœa." (2)

We have certainly no disposition to follow the suggestion of our author, on another occasion, that it is unimportant "to know the *cause* of trouble in our functions," and that, "an excellent mode of arresting diseases is to confound them." Nevertheless, we think that "a comparison between the typhoid affection and proper enteritis" may be "established," in the sense in which our author would avoid it; that is to say, so far at least as the pathology of structural lesions is concerned, or even the complex nature of those lesions. We would ask in the first place, however, whether our author has stated his proposition fairly in implying that "diarrhœa" is the only symptom in common betwixt the Parisian disease and "proper enteritis"? Next, we will take a "proper enteritis" that shall have tortured the lowest orders of society for the many days that almost constantly marked the Parisian disease, before its subjects were conveyed, or "walked," to the hospital. We will give them, also, the same mode of treatment, wine, cinchona, quinine, lemonade, the straight-jacket, or consign them alone to the irresistible march of disease; and we think it will not be doubted, that we should obtain not only all the array of symptoms, but all the lesions of structure, and all the mortality, (3) that distinguished the "typhoid affection." Nay, we will waive the advantage of taking our cases from amongst that wandering class who have lately emigrated from the country, where simple habits prevail, to the purlieus of

(1) Buckland's Geology. (2) On Typhoid Fever, vol. ii. p. 272.

(3) More than a third part of the subjects of our author's cases perished; that is to say, 50 of 138.

Paris, and where, in consequence, they became early and constantly subject to diarrhœa; (1) but we will take individuals of your own choosing, and, by subjecting them in the first instance to "constipation of many days' duration," (2) and to an equal amount of neglect before beginning the treatment, we will afterwards so exasperate the disease by the same treatment that obtained in "the typhoid affection," that we will not only exhibit the coincident "symptoms of diarrhœa," but will astonish you with our success in producing a perfect parallel in all the structural lesions that have appeared so characteristic of the Parisian anomaly,— not excepting even the glands of Peyer.

We have already seen how simple inflammation of the veins, from wounds, and various acrid injections, not only put on some of the phenomena of typhus fever, but each result is actually considered to be that disease; whilst pneumonia, phrenitis, *enteritis*, may come, at last, to present the same mimic symptoms. (P. 446 — 456.) In respect to the last affections, the period at which those phenomena ensue is not the stage when we must look for clear distinctions, or for any definite pathology. And here, be it said, is one of the great dangers to which we are exposed in a careless pursuit of morbid anatomy. Place, side by side, the victims of enteritis and of typhus fever, and, forgetting the tokens by which they were once distinguished, our scalpel may reveal nothing but one perfect coincidence in morbid lesions. Each may have his "rose-coloured lenticular spots," his "sudamina," his "ulcerated epiglottis," his "*specific* alteration of the glands of Peyer," his "intestinal perforations," and even his "meteorism"; all, and each one of which, being assumed as pathognomonic of typhoid fever, places our science, according to our author, in a state of "infancy." *And so our author farther on.*

If we may venture, however, upon a generalization, we would say that, if in any epidemic we find an instability of structural, or even of special functional, lesions, the disease may be safely set down as an idiopathic fever. And so, on the other hand, if in numerous cases, we find one unvarying lesion of structure, and, above all, if that lesion was the first in the series, and the earliest symptoms always pointed to that part, as in the Parisian typhoid, we may equally conclude that it is a local affection from the beginning. Certainly, however, cases may arise, in such

(1) See ibid. vol. ii. p. 217, et *passim*. (2) Ibid. p. 180.

local affections, where idiopathic fever may supervene; but only so when the subject is predisposed by constitutional causes, whilst the comparative rarity of such cases separates entirely the local from the constitutional affection. The predisposing cause of each will have been, also, probably the same; since the whole system being predisposed, *occasional* causes may develope some local change of action, before the great explosion takes place, and of which the former may be the *exciting* cause. This is especially true of that class of society who seek for relief in hospital practice, and these are they who are most exposed to the occasional causes of enteritic inflammation. In the constitutional cases there is no uniformity in the primary local developments of disease.

We are aware that it is an important argument with our author in his effort to establish "the *specific alteration* of the small intestine," that,

"We can, by means of irritants more or less energetic and of long continuance, produce an enteritis; but we cannot, at least to my knowledge, excite the typhoid affection by any means whatever." (1)

Did our author consult Magendie? (See our Vol. I. p. 397—398.) Such experiments may have been made in Parisian hospitals with a view to this question, (Vol. I. p. 305;) but, it has only been our painful experience to witness them in the practice of bold and ignorant empyrics; and here we think that we have sometimes met with results not greatly unlike "the typhoid affection." Besides, what are the "irritants" to which our author refers, compared with the vicious habits that prevail in the low society of Paris, the influence of those habits upon country vagrants, (for such were generally the subjects of "typhoid affection;") associated, also, with that powerful predisposing and exciting cause of intestinal inflammation, "*long continued constipation*," and a thousand other *natural* causes that cannot be imitated by art, and which establish a modification of inflammatory action, and a predisposition to disease at large, that shall bring the whole system under its powerful influence; when an *artificial* inflammation of the intestinal mucous membrane may scarcely extend its ravages beyond its primary seat?

In adverting again to the disappearance of redness in ulcerated, and other disorganized parts, we shall not here repeat what we have said in other places as to the causes of its decline, or in respect to the compatibility of its entire absence with in-

(1) Ibid. vol. ii. p. 272.

flammatory action. (Pp. 187—188, 317—330, 557, note.) But, we cannot neglect saying, that our author found in the phthisical subjects, that the redness remained when it "only dated a *few days previous* to the death of the patient." In the typhoid affection, however, the cases were almost always of long duration, (see tabular view,) and besides the natural tendency of the *redness* to subside as the lesions of structure took place, *diarrhœa*, profuse diarrhœa, had existed from the commencement of the disease; aye, and this, too, in most of the instances for many days before the subjects came under the auspices of science. It was accompanied, from the beginning of the attack, with "abdominal tenderness and abdominal pains." This, therefore, more than the lesions of organization, more than the cramped condition or destruction of the blood-vessels, abated or extinguished the *redness* of inflammation. This consideration explains, in connection with the disorganization of the vessels, any want of *redness* in the *spleen*, or *liver*, and rescues the lesion, as disorganization does that of scirrhus, from the hypothetical cause. Diarrhœa is nature's mode of subduing inflammation, as are, also, dropsical, and hemorrhagic effusions. It is a curative result of the action itself; and so is it, also, with suppuration, softening, and thickening, that the destructive process shall have an end, before it ends the patient. Do we not see in the protracted diarrhœa, which attended the typhoid affection, the special interposition of which we are speaking, and that it may by possibility have a benevolent reference to the delinquencies of art? Let us take an example. Thus, in a particular case, it is said that,—

"Although the *diarrhœa was very great*, the mucous membrane of the colon presented no evident traces of inflammation, *inasmuch* as the *thickening* MAY BE CONSIDERED *as the consequence of the reaction occasioned by the meteorism*"!

It would not do to have inflammation here, for the absence of *redness* supplied an opportunity to advance the hypothesis of the "specific nature of the intestinal disease"; and the rejection of inflammation carried forward the more important doctrine which is involved in the next sentence:—

"So that the ALTERATION OF THE PATCHES (Glands of Peyer) of the small intestine, was doubtless in a great measure THE CAUSE of the abundance of the alvine discharges"! (1)

But, "the reaction occasioned by the meteorism"! *Meteorism*, then, is that "specific" cause which our author substitutes for

(1) Vol. i. p. 324.

inflammation, and which is said to be "exactly opposite" to inflammation. What say you, ye disciples of Longinus, or ye, who seek for mental food in the depths of Bathos? Ye, too, who see as different conditions, physical and vital, in a suppurating bile, as the foregoing modes of action in the same lesions of structure? (*Inflammation, Sec.* 2.)

We will have other examples, taken from our author in regular succession, of these meteoric devastations by which the foregoing doctrine is specially intended to be exemplified, as we have already shown by former references. Thus, Obs. 42, a "cotton spinner," who "*walked about till the twentieth day of his disease*," (the typhoid affection,) resorted to the hospital to spend the only remaining day of his life, and to offer his intestinal lesions as a substantial proof of the validity of the contra-inflammatory doctrine of ulceration, &c. He was a corpse on the morrow, and a very important one to science,—especially to *diagnosis*. In the alimentary canal, the following contra-inflammatory appearances were noticed:—

"The *œsophagus* presented through its *whole length* oval *ulcerations* 30 in number, formed by the *destruction* of the mucous membrane.— *Stomach* softened in its upper extremity; an inch and a half from the pylorus *many* small ulcers. A similar ulceration in the *duodenum* near the pylorus.—The *convolutions* of the *small intestine* were *united together* in many points by means of albuminous, membranous, *grayish* concretions, of a *good consistence*, and presented, likewise, patches of a SUFFICIENTLY DEEP RED, owing to the *injection of the peritoneum*.— Mucous membrane of *jejunum* presented in the neighbourhood of the ileum patches covered with gray points, and similar ones were seen in the ileum, and as far as the last four feet they had a natural appearance. Beyond this point there were 20 *ulcers* of irregular shapes, &c. In the ulcers, the *muscular* coat was seen a little *reddened*, more or less *thinned*, and *otherwise healthy*. It was *wholly gone*, and the *peritoneal* coat alone remained in the centre of one of them. Finally it had *disappeared* where the *perforations* existed." These perforations consisted of "two holes, about a line and a half in diameter."—There were some other minor evidences of the *absence* of inflammation,—such as, "the *spleen* was five times as large as usual, had a BLACKISH colour externally and internally, its texture moderately *softened*;" &c. "The *mesenteric glands* were *grayish*, enlarged, and a little soft."

But, the most important lesion is to come. "The last 20 *patches* (glands of Peyer,) of the ileum were *deeply ulcerated*, *two* of them *perforated*, their *circumference* of a *grayish* hue," &c.—From all which it follows, that,—"the *colour* of the *elliptical patches* of the ileum, with that of the corresponding *mesenteric glands*, seems to me to show that in *this case*, as in *the other*, *inflammation had but a small share in the production of the ulcers*."

It will have been observed, that in the foregoing case, all the

inductions are founded upon the mere debris of the body; since the patient did not come under our author's observation till he was in a dying state. This is morbid anatomy in the act of bestowing its light upon pathology; and this is an example of *typhoid fever* because the glands of Peyer were found diseased beneath *perforating* ulcers of the mucous membrane, and because, perhaps, the patient "*walked about during the first* 20 *days of his sickness*," *and till within* 4 *days of his death*. Taking our author at his word, that this is a fair example of "the typhoid affection," is there one who can hesitate as to the nature of most, if not all, of the other cases? The coincidence in the various symptoms and lesions, of the least importance, between this and the other cases, can leave no doubt of their identity. But, here was an individual who was able to carry about, in his ordinary avocations, the most astonishing amount of disease till ulcerative inflammation actually perforated his bowels. But, lest the suspicion might arise that this was a miserable case of *enteritic inflammation* upon which other lesions had supervened, the reader is gravely told, that, "in this case, (as well as others like it,) it seems to me that inflammation had but a small share in the production of the ulcers."

Here is another case, being the one which precedes the foregoing, and to which our author there refers as supplying the same evidences of its contra-inflammatory pathology. It is Obs. 41. The patient was a *carpenter* of "*dark* hair." "He had been ill 18 days," "*came* to the hospital for the purpose of obtaining advice merely," but "*consented only with reluctance* to take a bed there," and in 8 days afterwards fell a victim. Before proceeding to the morbid anatomy, we may add, that the patient "*walked about*" four days before his death.

"*Cerebral veins* much injected with blood. *Medullary* substance, very much injected. The *circumvolutions of the small intestine were united* to one another by means of albuminous, *membranous* secretions. At the end of the ileum, at a foot from the cœcum, there was a *perforation* of the intestine two lines in diameter, situated in one of the *ulcers*. Throughout the whole length of the *ileum* there was a *great number of elliptical patches*, from half to three-quarters of a line *thick*, *composed* almost entirely of the *mucous membrane*, which was *grayish*, and had many *blue* points in *this same part*, some of which (8) were *ulcerated*." "Between these [ulcerations] were others of a smaller size upon *crypts* which were clustered together," &c. "The *elliptical patches* of the ileum were found *thickened*, some *ulcerated*, and one of them had a *perforation* in the centre of it." "Save in respect to some modifications I shall soon mention, the *lesion of the small intestine did not differ* in the least from what is observed in

subjects who die of the typhoid affection, after having experienced the gravest symptoms. We cannot, *therefore*, fail of recognising the character of the disease in this case. This subject had the typhoid affection as much *as he has had pneumonia, in whom we find hepatization of the lungs.* One case is *not less certain than the others.*" "It is important to remark that the alteration of the *elliptical patches* of the small intestine being the only one which was of a grave character, [?] it was, likewise, the ONLY ONE to which we can refer the slight symptoms anterior to those of perforation." The brain, spleen, ulcerations of the mucous membrane, the gluing together of the convolutions of the small intestine, being of no importance. Finally, "we are led to conclude in this case inflammation had but a small share in the production of the morbid changes of the patches, and that *ulceration was the principal tendency*"*!* (1)

One case more. "A tailor, (Obs. 43,) rather liable to colds; said he had been ill 20 days; had left work *eight* days, *but had not kept his bed.*"

It may be well, therefore, to apprize the reader that this is, like the foregoing, a strong example of the typhoid affection. They are called "*latent*" ones, however, by our author, till made *apparent* in the dissecting room. He died on the 45th day.

"Slight effusion under the *arachnoid. Heart* pale and very soft, parietes thin. The concave face of the *diaphragm*, and a part of that of the liver were *grayish* and *covered by false membranes. Epiploon reddish;* it *adhered* to the left side of the superior strait, over a small extent. *The convolutions of the small intestine* adhered together by means of *membranous concretions.* It had on its external surface patches of a more or less *deep red* colour, owing to an *injection of the peritoneum.* Its *mucous membrane had lost half its consistence* in the *jejunum;* it was *soft as mucus* in the *ileum*, a little *thickened* throughout its *whole* extent. Two feet from the cœcum there was a transverse *ulceration.* Six *similar ulcerations* were found in the five inches nearest the cœcum, and in the centre of the first of them there was a *perforation* three lines in diameter. *Mesenteric glands* a *little red*, and of 3 or 4 times their usual size, and were but *half as consistent* as in health. *Mucous membrane* of the *large intestine* was *much softened in the first half;* it was slightly *thickened throughout.* The spleen was *double* its usual size; its texture was *pale* and slightly softened."

The philosophy of the foregoing. "As the *ulcers* of the small intestines were the *most severe*, and, *doubtless*, the *most ancient*, to them *alone*, therefore, must be attributed the *febrile symptoms* which occurred in the commencement," &c.; "so that, the *inflammatory stage had, doubtless, always been slight*, and as in the *preceding* case (Obs. 42) *had exercised but little influence upon the ulceration.*"

"What we have stated in relation to the preceding observation will *suffice*, moreover, *to show how we could have arrived at a diagnosis in this case.*" (2)

Here we desire the reader to compare the foregoing pathological views and reasoning with what we have quoted from our author as to his demonstration of the contra-inflammatory nature of tubercle, p. 631—632.

(1) Vol. ii. p. 282—289. — We give our author's words in these cases; but necessarily omit a part of the detail. The *italics*, as usual, are ours.

(2) Vol. ii. p. 295—301.

Although our author does not take into account the vital signs in making up his principles and generalizations, as we shall have more fully shown, he has supplied a memorandum which others may employ. From this part of the record, we learn that "*abdominal pains and tenderness of the abdomen were the earliest symptoms*;" a fact which we shall leave the reader to connect with those which have preceded;—nor, perhaps, is the early, and uniform diarrhœa an unimportant consideration. As to the doctrine, more important than all, that the vast disorganizations, which we have hitherto considered, are not of inflammatory origin, but depend on something "exactly opposite to inflammation," we are not sensible that our author has offered any other ground for its support than the colour of the part; whilst he concedes that *red* and *white* are sometimes blended in the same lesion. Nevertheless, this is one important source of our author's celebrity for *diagnosis*. But, as neither morbid anatomy, pathology, or diagnosis, are of special value unless they lead to correct treatment of disease, we shall have furnished a full outline of our author's habits as to this ultimate object of all medical science; and should it, upon the whole, appear to result abortively, we suppose that this will be allowed to be a pretty good ground for conclusion that our author was mistaken in imputing error to all else than himself.

Having thus considered the contra-inflammatory doctrine in respect to the typhoid affection, let us now look at a passage from our author, which embraces a hazardous recognition not only of the inflammatory nature of the organic lesions, but, of what was probably truly the case, that the typhoid affection, in most instances, was intrinsically *an enteritic inflammation*, upon whose neglect supervened the terrific lesions which our author has so well set forth in his work as an admonition to others. The passage is obviously intended to carry out our author's design of abolishing bloodletting, blisters, &c., in inflammatory affections, and to aid in the substitution of tonics and stimulants. Our author comes to his conclusion by a circle of speculation,—leaning first to one side and then to the other, till we read that;—

"If, however, we fear that we have made some error in our diagnosis with relation to the state of the stomach, we might begin with them, [tonics, &c.,] a cold infusion of cinchona, and an enema of cinchona; and, if they produce no bad effects, *pass rapidly to the use of strong tonics*, as the sulphate of quinine in the DOSE of *eight, ten, twelve*, and *twenty* grains, in an *aromatic* or gum potion,

with the *syrup* of *cinchona*, or any other, giving *for drink* the *sweetened* infusion of *cinchona*, or a *vinous* lemonade."

And now for the paragraph to which we have referred, and which follows, immediately, the foregoing:

"If at the same period, [that is, after the *febrile* action had abated,] the diarrhœa is copious, the enemata, as well as the drinks, ought to be more or less tonic; for I always suppose the *febrile action* to be much diminished, very slight, or almost none at all, and then we have not so much *an inflammation* to combat, AS ITS CONSEQUENCES, and bitter medicines are indicated." (1)

It would seem, therefore, on meeting a declaration like the foregoing, that, by some mischance, a page from the writings of John Hunter had got bound up with the work of our author.

If we be, therefore, right in this matter, it is important that those, who are coming into the profession, and with whom the works of our author may be made to supersede the experience and the doctrines of his predecessors, and who "*insists upon the distrust with which part of the facts daily published ought to be recieved*," should understand that "the typhoid fever," if not a sequel, was sadly complicated with a neglected intestinal inflammation, upon which depended all the softening, thickening, ulceration, and all the *perforations*;—that, however it may be contended that idiopathic typhus demands the early exhibition of tonics and stimulants, they may have the advantage, at least, of prescribing for another *name*, when *muco-enteritis* is the most essential part of the disease. And then those doctrines, and that treatment, in relation to pneumonia;—but we will not anticipate. We will only now insist, that science is a scourge to humanity in proportion to the evils it may inflict. We will not call it science; for she, it is said, is a "Heaven-born maid." It is her occupation to dispense the choisest blessings to mankind; not only to build up principles in medicine, but such principles as shall stay the havoc of disease. Her ministers are but few, and by them is the multitude guided. "Who," therefore, "but must tremble for the patient, when false or imperfect instruction has kept extreme means of cure out of the contemplation of his physician?" (2) Or, as our *prophet* again has it,—

"Let a professor ever so fairly give a general abstract of facts, instead of filling the hours with his own partial ideas of their relations, what can he furnish beyond a faint outline? The student, the young man going forth to scatter life and death, requires full and free access to the least polluted sources of knowledge. *Images* and *emblems* may fail to infuse into the preceptor DUE

(1) Vol. ii. pp. 446, 447. (2) Beddoes, on Fever and Inflammation, p. 170.

REVERENCE OF ORIGINAL OBSERVERS; but may not the original observations themselves guard the pupil against credulity? And is not the oldest practitioner, from the appearance of unseen modes of disease, often in the very situation of a raw student? In this difficulty, no oral instruction, *no compendium*, no ingenuity, of the individual can supply what is often found among *faithful copyists* from nature. It is a difficulty, which arises *most frequently from fever*, and no man can see many cases of fever without feeling it, unless he be *one of those who take diseases in the gross.*" (1)

Granting, however, what we think to be possible, that "the typhoid fever" of our author was, "in a certain number of cases," an *idiopathic* fever, complicated with the worst examples of muco-enteritis, and intestinal perforations, that are known in the annals of medicine; (2) it appears to us abundantly manifest that the constitutional affection was of insignificant importance compared with the early intestinal lesions, and that to these the whole energy of practice should be directed; that, in short, the disease should be treated as muco-enteritis, as the only feasible plan of arresting the fever, and those terrific secondary lesions which have supplied such a repast to the anatomist.

Our author, therefore, in devoting a large proportion of his work, which is ostensibly designed as a mere record of "rigorous facts," to the subversion of those principles which make up the Hunterian philosophy of inflammation, and this, too, by reasoning upon the cadaverous results of his own cases exclusively, shows us how far he "has kept in mind the thought of the author of Emile," (p. 686;) and when we regard all the collateral speculation in connection with his specific generalizations, we shall the more readily understand the merits of the "numerical system," and, by duly weighing the generalizations themselves, how far, in departing from the spirit of that system, he has "arrived at the *conclusive* results," of which we had some forewarning in the preface to his work.

With every sentiment of respect for the opinions of those who differ from us as to our author's work on the typhoid fever, as well as his other performances, we are constrained to express our conviction that one more obnoxious to the objections we have thus far stated, or more dangerous in its practical tendencies, has

(1) Beddoes, Ibid. p. 238.

(2) It must here be observed, as will be more distinctly set forth hereafter, that our author does not consider it an *idiopathic* fever, in the remotest sense; but, that the whole condition of the disease, and all of the lesions proceeded from a "specific alteration" of a particular part.

not issued from the press since the appearance of Brown's "Elements of Medicine."

Perhaps, however, we have not yet satisfactorily made out our case, and the possibility of which must be our apology for extending this article beyond what might otherwise appear a reasonable limit. We are also impelled to give to the subject a critical, however excursive, investigation, by the high authority of M. Louis on all questions in morbid anatomy, and therefore, according to the philosophy with which that pursuit has become invested, in all matters of a pathological and therapeutical nature; believing it "better to destroy a prejudice than to erect a pyramid."

His researches have become a great landmark in the field of medicine. Their importance is but little concerned about abstract facts; they relate to great fundamental principles. He has contributed more than any other, towards unsettling the philosophical inductions of Hunter, Bichat, and others, whose *very facts* he puts aside as unworthy of record. He has avowedly aspired at pulling down the superstructure which has been reared upon the phenomena of nature as contemplated when revealing her vital actions and laws.

We shall therefore go on with our author's generalizations; resuming them in the numerical order in which we started from the first.

21. "Although the *hard patches* are found in a fourteenth part only of the cases of typhoid fever," [less than 4 cases,] "it is nevertheless evident, that they are of *great value*, since, as they appear to be *peculiar to typhoid fever*, [!] they would be *sufficient* to enable us to recognise the affection *at the first glance*." (1)

22 and 23. "The *meteorism* and *ulcerations* of the *large intestine* are *not less important* than the hard patches." (2)

24. "It [the *meteorism*,] *brings on* more or less marked alteration of the *structure* of the intestine." — "The meteorism could not be attributed to any *appreciable* lesion of the mucous membrane of the colon, no one having been constant." "The thickening is, without doubt, *a consequence of the reaction of the membranes upon the gases by which they are distended*." "Still less can we appeal to this lesion [*ulceration*] as the cause of the meteorism, inasmuch as it was large, serious, and almost always present in the small intestine, which was less frequently meteorised than the colon." (3)

It is clear, therefore, that thickening, and ulceration, as in the large intestine, were not owing in the *small intestine* to a "reaction of the membranes upon the gases."

25. "I would remark that the ordinarily *late commencement* of this symptom

(1) Vol. i. p. 228.
(2) Vol. i. p. 229.
(3) Vol. ii. p. 450; and vol. i. pp. 198, 324.

[*metorism,*] is a new reason for believing that during life, as after death, the large intestine is the principal seat of it; *the mucous membrane of this last having been affected only consecutively upon that of the small intestine.*" (1)

26. "Although, *in the actual state of science,* [!] I cannot assign *any cause for meteorism,* I would, nevertheless, remark that it seemed to me to follow *a* LAW which was analogous to that by which other symptoms are governed." "There is something *specific* in meteorism"! (2)

27. "At a certain degree, the *distension of the colon* appears *characteristic* of the typhoid affection." — "So that we are led to admit in this case a *special cause,* and this shows the IMMENSE IMPORTANCE OF METEORISM in the history of the typhoid affection." — "It is a *formidable symptom;* what means must we employ against it?" "In an individual weakened by long suffering, it must have concurred most powerfully in the production of death, and perhaps HAVE BEEN ITS PRINCIPAL CAUSE." — "The *age* of the patient when placed in opposition to the *meteorism,* ought not to *outweigh* this symptom." (3)

28. "When *meteorism* is absent, there are somnolency, delirium, extreme debility, *sudamina* [our author's italics] and no other affection presents this group of symptoms." (4)

"On this point, *as on so many others, experience is silent,* and, as there is something SPECIFIC *in meteorism,* as we cannot explain its development by the state of the internal membrane of the intestine, *it is proper for us to engage in* EXPERIMENTS, which, sooner or later, may produce *happy results.*" (5)

But we have something still more important, viz:

29. "In those cases in which the *meteorism* is very much marked, we may be able, *at the first glance, to* DISTINGUISH THE CORPSE of a person who had died of this disease, [the typhoid affection,] *from that of one* who had died of any other acute disease." (6)

As *meteorism* is considered *fundamental* by our author, and, indeed, "peculiar to the typhoid affection," we shall bestow some notice upon it *below.* (7) Next to the great basis of our author's

(1) Vol. ii. p. 34.
(2) Vol. i. p. 198.
(3) Vol. i. p. 229; and vol. ii. pp. 35, 65, 365.
(4) Vol. ii. p. 268.
(5) Vol. ii. p. 450.
(6) Vol. i. p. 222.

(7) There is much "pottering," as Sir C. Bell would say, (p. 642,) about this *meteorism.* We do not agree with our author as to its *pathology;* but would suggest whether it be not compatible with the "actual state of science," however it may be only "now in its infancy," to suppose that the *meteorism* might have been owing to the "reaction" of the *contents* of the intestines, which, as will appear from our tabular view, were only expelled by the spontaneous efforts of nature. We are told by Mr. Lee, that "the *intestinal lesions* " *themselves,* "*so constantly* observed in the hospitals of Paris, are frequently occasioned by the presence of indurated excrementitial matter"; (*a*) — to which Larroque nods assent. (Vol. i. p. 305.) But, if this authority be questioned, we will then give you our author himself, who affirms that, "the *altered patches* were *constantly* in contact with materials that had become *irritating* to them." (*b*)

Tisot intimates that this neglect of fœcal matter was common at his time, and says

(*a*) On the Medical Institutions of France, Italy, and Germany, p. 31. — (*b*) On Typhoid Fever, vol. ii. p. 394.

pathological conclusions, (the variety of *colours*,) the foregoing contingency takes rank in our author's cases.

that, "the corrupted *flatulent* contents of the intestines (matières corrompues) accumulating in the canal adds greatly to the malignancy of fevers." (*a*)

We may also say, in behalf of our construction, that, in our author's cases, "the intestinal perforations always occurred *very near the cæcum*," and "the changes wrought in the patches were always greater according to their *proximity to the ileo-cæcal valve*;" (*b*) and the ulcerated crypts were "always in the neighbourhood of the cæcum." (*c*) It was about this neighbourhood, also, that the *specific* flatus was most apt to accumulate, "causing a greater or less number of circumvolutions to be seen at these points where the canal was most loosely attached. This canal was found in front of the stomach, &c. We must remember this fact in practice in order to avoid errors in *diagnosis*"! (*d*)

To the foregoing considerations should be added the quantities of *lemonade*, &c., that were administered to the patients, and which are set down with all the precision and formality that distinguish the "numerical system." Sometimes, it is said, "*three quarts* of lemonade," besides large quantities of other fluids, were drank in a day. These specifications, however, relate to individual cases; and, when our author comes to generalities, he says, "on account of the nature and seat of the disease, drinks *cannot*, doubtless, *be too copious*, and we cannot even assign limits to them in this respect, except those which are indicated by the aversion of the stomach." "Two or three quarts of liquid, in 24 hours, is a *medium* quantity." (*e*)

As a farther illustration we may state, that "out of 58 patients affected with pneumonia of different degrees of severity, 21, or about one-third part, had *diarrhœa*."—"The *fourth* part of the subjects, who had *erysipelas of the face*, had *diarrhœa*." — "*Diarrhœa* occurred in a *fourth* part of the cases of *acute rheumatism*." (*f*) Now, having, always ourselves, early evacuated the bowels, employed the lancet, &c. in the foregoing complaints, and never allowing *lemonade*, we have no recollection of having ever witnessed an instance of *diarrhœa* supervening upon either.

We may say, also, that "meteorism came on *suddenly when perforations* took place;" and is considered by our author as an important diagnostic symptom. Nevertheless, "in the actual state of science we cannot assign any cause for meteorism."

In justice to our author, however, we should say that he had some suspicion that "meteorism" was owing to the cause which we have assigned; or, rather, that others might imagine this cause to the detriment of "the typhoid affection." He therefore anticipates the objection, by saying, —

"If it is not possible to find a cause for the *meteorism* in the condition of the mucous membrane of the colon, *we cannot, however, seek for it in decomposition*, because the meteorism *diminished* in many patients *during the last days of life*, and the effects of decomposition ought rather to have augmented, than diminished, in the cases in which the affection was more or less quickly fatal." (*g*)

We are disposed to think, however, that our author here alludes to *vital* decomposition of the intestines, since he immediately adds, — "we cannot, moreover, attribute this phenomenon, at least, *solely to an alteration of the blood*"! [See vol. i. p. 418, for the doctrine of *vital fermentation*.] We are more inclined to believe, however, that "the *meteorism* diminished in many patients during the last days of life," on account of the ultimate evacuation of the fœces by the uniform diarrhœa, and be-

(*a*) Avis au Peuple, c. 17. — (*b*) On Typhoid Fever, vol. i. p. 175. — (*c*) Ibid. p. 184. — (*d*) Ibid. p. 197.—(*e*) Ibid. vol. ii. p. 444.— (*f*) Ibid. pp. 22, 24, 25. — (*g*) Vol. ii. p. 34.

30. "*Perforation*, [of the intestines,] in the course of acute diseases, is *peculiar* to the typhoid affection. (1)

cause the subjects were then less able to drink "lemonade," or to submit longer to the doctrine that, "the drinks cannot be too copious."

It might, therefore, form an interesting inquiry to ascertain how far philosophy has been puzzled by her own expounders.

Perhaps, however, it may be as well to allow our author to show that we are right in the foregoing conclusion. Thus: —

"The *large intestine was distended with gases*, and the sigmoid flexure of the colon was very much turned towards the right side, and contained MUCH MOULDED FŒCAL matter." It will be seen, too, by our author's cases, that where the diarrhœa had been least, the meteorism was always greatest.

And here it is worthy of remark, in relation to the doctrine that intestinal "thickening may be considered as the consequence of the reaction of the meteorism," (p. 702, G. 15,) that, in the foregoing case, —

"It is *remarkable* that the mucous membrane of the *large intestine, notwithstanding* ITS GREAT DISTENSION, *was of a* PROPER *thickness*, that is, it was REALLY THICKENED; and this can hardly be attributed to the reaction produced by the distension itself, because the mucous membrane, *except* in this thickening, was very nearly HEALTHY." (*a*)

It is worthy of notice, as showing how distinguished observers may come to opposite conclusions upon important matters in science, that Dr. Hosack considers *meteorism*, in diseases as treated in America, a signal of convalescence. A graphic account of this diagnostic sign may be found in his posthumous Lectures on the Theory and Practice of Physic. (*b*) The Report is copied into the American Medical Intelligencer, (c) under the significant caption of "*Reporting Progress*;"—in allusion, we suppose, to the march of "the present state of medical science."

(1) Vol. ii., p. 373.—(See Mr. Taylor on Perforations of the Stomach, &c., in Guy's Hospital Reports, Aug. 1839. Also, Stokes, Graves, &c.)—The reader, however, requires no help from us to show that modern records prove the fallacy of our author's generalization. In eight out of all our author's dissections, in typhoid fever, (especially where the patients continued to "work" for many days after the attack, or "walked" to the hospital on the 15th or 20th day,) there were intestinal perforations: "a considerable proportion, and one which astonishes" our author, (more than it does us,) "when we reflect that so serious an accident has scarcely been known *even to a small number* of physicians, *except within a few years*," &c.

That our author was unacquainted with *past observation*, we can truly believe. Laying our hand, therefore, upon a few "black-letter" books which stand upon our shelves, we may say that, Barbette relates a case exactly similar to our author's, only going a little beyond them. Besides the ulcerations, which Barbette mistook as the result of inflammation, there were *four* perforations of the *small* intestine; whilst, also, this was a case of *dysentery*. (*d*)

In the Acta Physico-Med. Acad. Cæsar. Leopold, etc., is another similar case of perforated intestines, where a quantity of *worms* were discharged into the cavity of the abdomen. This case was also mistaken for one of inflammation. (*e*)

Benivenius describes a case of perforation from simple acute intestinal inflammation, having *all* the painful symptoms related by our author. (*f*)

There is a very remarkable case in the Sepulchretum, (not of fever, but inflammation,) where the ileum was perforated "*ad septem usque foramina*." Here, also, is another case where perforations of the intestines were ascribed to *worms*; (*g*) which "points

(*a*) Vol. i. pp. 362, 363, 223; vol. ii. pp. 177—178, 215, &c.—(*b*) Lec. 25, p. 291.—(*c*) Vol. ii. p. 258.—(*d*) Prax. Med. l. 4, c. 5, Op. Om. 1683.—(*e*) T. i. Obs. 172, 1727.—(*f*) De Abditis et Mirandis Morb. et Sanat. Caus. c. 76, 1507.—(*g*) T. ii. l. 3, s. 14, ob. 20; l. 4, s. 10, ob. 3.

31. "The typhoid disease seems to establish a *marked predisposition* to morbid changes in the *mesenteric glands;* but not merely in these, for those of other regions, particularly those of the *neck* and around the bile-ducts are similarily affected; these last having been changed in no other case of disease than the affection which it is the object of this work to elucidate." (1)

32. "In certain cases we can determine the period at which death took place by means of the mesenteric glands." (2)

33. "The state of the mesenteric glands was, as that of the patches, the effect of a *retrograde course* in the disease;" (3) — (especially considering that the greater the violence and duration of the disease, the greater were the lesions of structure.)

34. "The *gray colour* [of the intestinal mucous membrane] is very nearly the sole one I have met with in a certain class of patients," [eleven] &c. "Therefore, if we consider this fact as an expression of a GENERAL LAW, it follows that the GRAY COLOUR is found *as a consequence of affections like those we are now examining*, only in patients who have died twenty days at least, and generally many more days, after the commencement of the disease." (4)

35 and 36. "*Ulcerations* of the *œsophagus*, like those of the *pharynx*, seem *peculiar* to patients dying of the affection which we are now examining." (5)

37. "Therefore we must consider *ulceration, or partial destruction of the epiglottis* as one of the *secondary anatomical characteristics* of the typhoid affection, as we have already *decided* that ulcerations of the *pharynx* and *œsophagus* are; and that this lesion observed in a patient who should die of any acute affection proves almost *to a certainty*, WITHOUT NEED OF FURTHER EXAMINATION, that the patient had died of typhoid fever." (6)

Especially considering that ulceration of the epiglottis occurred in one-fourth part, at least, of our author's cases in phthisis; and where, also, there was sometimes a complete destruction of the epiglottis. (7) But, what places the whole of this subject in its proper light, is our author's generalization, that,

out a *fresh analogy* between" (*a*) worms and intestinal perforations. And, since our author has a case in which worms were supposed to have been the principal source of the mischief, next to the lesion of the glands of Peyer, it may yet be that our author is right in rejecting inflammation as the cause of perforation, and might have drawn a general conclusion in favor of the worms. We have consulted no other of the old writers. Here is enough for the *fact*, and the *principle*. France, at least in her medical literature, is remarkable for claiming, as a novelty, what has been long familiar to other nations, and even what was once common in her own. Witness bloodletting in rheumatism, (vol. i. p. 326;) the discovery of muco-gastritis and muco-enteritis, of melanotic, and encephaloid, formations, &c.

(1) Vol. i. p. 248. (2) Vol. i. p. 231. (3) Vol. i. p. 232. (4) Vol. i. p. 167.

(5) Vol. i. p. 151. (6) Vol. i. p. 303.

(7) On Phthisis, sec. 289, 292. — We think the pathologist who has any regard for physiology, or for the extensive analogies which relate to ulceration, will be hardly inclined to concur with our author in his fundamental distinction, as it respects this lesion of structure when occurring in acute and chronic affections; and may we not add, as to all the other organic lesions which contemplate this factitious principle?

(*a*) M. Louis, on Phthisis, sec. 371.

38. "We must consider the *ulcerations* of the larynx, and especially those of the trachea and EPIGLOTTIS, as alterations PECULIAR TO PHTHISIS." And again, "from our researches in the first part of this work, these lesions, [ulcerations of the *epiglottis* and larynx,] appear *peculiar to phthisis*." (1)

From *three* observations in *pneumonia*, our author summarily concludes, that, —

39. "These facts, notwithstanding they may seem to be of *little moment*, indicate almost to a *certainty* the influence *pneumonia* has upon *lesions of the* LARYNX." (2)

40. "*Rose-coloured lenticular spots* seem to have in this disease [typhoid fever] something of a *specific* character," and "are something *peculiar* to the typhoid affection." (3)

41. "Since *sudamina* appeared as frequently in the cases of slight attack as in those which were more severe, must we conclude that this eruption, like the rose-coloured, lenticular spots, is something *peculiar* to the typhoid affection?" (4)

Our author endeavours, in various places, to establish a specific relation of the rose-coloured lenticular spots and the sudamina to the typhoid affection. There is nearly as much practical importance attributed to them as to the *meteorism* or to the glands of Peyer. (5) But, in respect to the *sudamina*, our author remarks, that, —

"By a fatality, which I cannot too much regret, I looked for them in nine subjects only, [of the fatal cases,] in six of whom, or in two-thirds, they were seen in a larger or smaller quantity." "They were seen in 14 out of 21 subjects [that recovered] in whom the disease was *grave*, and in whom I looked strictly for them; that is to say, in the *same proportion* as among those who died." (6)

(1) On Phthisis, sec. 60, 285. (2) On Typhoid Fever, vol. i. p. 305.

(3) Vol. ii. pp. 204, 209. (4) Vol. ii p. 208.

(5) See vol. ii. pp. 34, 65, 207, 268, 271, 294, 334, 349, 358, 364, 365, and references in other places. He allows sudamina to be frequent in Phthisis. *Sec.* 136.

(6) Vol. ii. pp. 207, 208. — Like all other generalizations of our author, the assumptions as to the sudamina and rose-coloured spots appear not to have stood the test of the "numerical system." Henroz early exposed the assumption in relation to the sudamina. He first concludes that M. Louis has variously invalidated his own statement, and he brings forward a variety of diseases in which sweats and sudamina coincided, and finally affirms, that sudamina are often absent in the Parisian "typhoid affection," (as is manifestly implied by M. Louis,) and that "they have no greater relation to this signalized disease, than to many others," of which he gives an analysis of more than *twenty*. (*a*) Subsequent observers have arrived at a similar conclusion. Thus, recently at the New-York Hospital, in several cases of genuine typhus fever, contracted in Ireland, "the eruption of Sudamina was observed about the neck and groins; but, this eruption was the same in typhoid fever, and in *many* of the severe cases of *remittent fever*." (*b*)

As to the *red spots*, they are abandoned by all but our author's immediate follow-

(*a*) La Lancette Francaise, No. 86. 1837.

(*b*) New-York Journ. of Medicine and Surgery, July, 1839, p. 101.

42. "The *most characteristic symptoms* of the typhoid affection, the *meteorism*, the brown dejections, or composed of clots of blood, the sudamina, the destruction of the skin in the parts corresponding to the blisters, occurred." (1)

We shall now present an induction by which we are authorized to decide, that a disease may be taken as "the typhoid affection" if it possess certain signs which are common to many other diseases, though the case in question have no other characteristic symptom of the typhoid complaint. Thus:—

43. "I think I may conclude from these facts, that if symptoms, analogous to those which have been pointed out, develope themselves without our observing at the same time meteorism, rose-coloured lenticular spots, or sudamina, I may add *deafness*, we ought not to imagine the existence of the typhoid affection." (2)

44, 45, 46 and 47. "Out of three subjects in whom the *pulse* is *unequal, irregular, intermittent,* OR *trembling*, and very much *accelerated*, we can conclude, therefore, with probability, that there are two in whom the *heart is softened*," in the typhoid affection!

The foregoing generalizations being of the most momentous importance in a practical sense, we have arranged them as *four*, since our author presents that number of criteria. Then follows, immediately,—

48. "From this proposition flows another *as a consequence*, viz. the possibility of recognising, at least by approximation, the moment when this softening commences"! (3)

An important law follows immediately, and is predicated not only of cases radically unfit, as we shall farther see, for any just conclusions in pathology, but is purely artificial on account of the neglect of bloodletting in those cases where it was most demanded by a slowness of the pulse; for these were evidently complicated with venous congestions as well as with inflammations. (See our Vol. I. pp. 145—146, 198—204, 219—232, &c.) Thus, then, our author:—

49. "From the comparison of these facts with the preceding ones, it follows that a moderately *rapid* pulse makes the prognosis more favourable, and ought to make us conjecture that the course of the disease will be *rapid*, while *an opposite state* of the pulse ought to make us fearful as to the *termination and length of the disease*." (4)

50. "Cough is *much more frequent* during the course of the typhoid affection

ers; and their accidental nature has been well shown by the Medico-Chirurgical Review, in relation to Roupell's hypothesis. (*a*)

(1) Vol. ii. p. 334. (2) Vol. ii. p. 358.

(3) Vol. ii. p. 233.—Our author thinks that these "diagnostic signs must add new interest to the study of the different conditions of the pulse."—*Ibid. p.* 236.

(4) Vol. ii. p. 235.

(*a*) July, 1839, p. 96, &c.

than during that of *other acute diseases*." "It is more frequent in the typhoid than in any other acute affection." (1) *Catarrh, pneumonia*, &c.!

51. "The cases in which fœcal matter was *mixed with pure blood*, or those in which it had the aspect and consistence of *coffee grounds*, are worthy of remark, inasmuch as evacuations of this nature were, *almost without exception, never observed in the course of acute diseases, excepting in typhus fever*"! "They will contribute very much to make the diagnosis more clear," in doubtful cases. (2) *Yellow fever, plague, purpura, ordinary typhus, intestinal inflammation*, &c.!

52. "I think I can say, generally, that if a patient with the typhoid affection experiences delirium, then a chill which continues without, or almost without, interruption for 24 hours and more; if soon afterwards his face, extremities, and a part of his body become of a purplish colour, considerable *meteorism* comes on quickly, the abdomen appearing painful for the first time; if these symptoms are found thus united, we ought to decide that very probably there is a *perforation*." (3)

53. "If the disease," [that is to say, the nature of the typhoid affection] "have been *latent till that moment* and there supervene suddenly in a patient with diarrhœa, pains in the abdomen, aggravated by pressure, accompanied by loss of expression in the features, and more or less speedily, by nausea and vomiting, there *must be a perforation of the small intestine*." (4)

54. "The cerebral symptoms *mask all the others*, or *oppose* the development of those which ought to correspond with the alterations which occur in the course of the delirium." (5)

55 and 56. "*Spasmodic movements or rigidity of the muscles* form, therefore, like *drowsiness*, one of the *most important* signs in the typhoid affection." (6)

57 and 58. "Moreover, if there is no *dependence* between the state of the tongue and that of the *stomach*, [in diseases generally,] neither is there any *dependence* between the *lesions* of the *first* and those of the *other* parts of the mouth and fauces." "When there are no cerebral symptoms, the tongue must be examined for itself alone, and not as showing the condition of the mucous membrane of the stomach." (7)

59. "Therefore, all the different conditions of the tongue observed during the course of the typhoid fever must be considered as the result of *one and the same cause* which is common to them with all secondary lesions which come on, like the former, at various periods of the disease;" (8) that is to say, the glands of Peyer.

Our author everywhere dwells with emphasis upon the momentous influences of the glands of Peyer upon every part of the animal fabric, but disregards those most important physiological relations which subsist betwixt all parts of the alimentary mucous membrane, and *especially* betwixt the organs which are intermediate and the terminations of that membrane. (Vol.

(1) Vol. ii. pp. 243, 247. (2) Vol. ii. p. 18. (3) Vol. ii. p. 387.

(4) Vol. ii. p. 374.—It occurred in 8 of 55 cases; but, in 3 of the 8 cases "it was latent."

(5) Vol. ii. p. 451. (6) Vol. ii. p. 170. (7) Vol. ii. pp. 84, 90. (8) Vol. ii. p. 63.

I. p. 60 — 61.) We doubt not, however, that physicians will continue to pay their accustomed heed to the various conditions of the tongue, as among the important vital signs by which they are guided in diagnosis, treatment, &c.; however like all other symptoms, it may fluctuate in disease. It has been rarely surmised in recent times, until our author came forward with *sudamina, rose-coloured spots, meteorism*, &c., that there is any uniform relation between a particular symptom, or a secondary lesion of structure, and a given disease. But, a certain group of symptoms, of which one or more may be absent, has been always known to be more characteristic of one disease than another; and, if this disease happen to be a *real* typhus fever, it has been also known that the tongue is apt to present appearances remarkably uniform in all parts of the world; and differing as much from our author's account of the Parisian fever, as did the other symptoms in most of our author's cases; which the attentive reader of his work will have seen to have been those of a severe intestinal inflammation, that ultimately involved, in its unrestrained career, most of the organic viscera. But, the philosophical physician never regards the appearances of the tongue, or any other symptoms, abstractedly, (Vol. I. pp. 235, 237;) nor does he conclude that gastritis, or enteritis, has no existence because the tongue is in a natural state. His scope has a wider range; and, if he find the tongue unfaithful to its common report, he marks it down "as an exception to the general rule." (Vol. I. p. 626, *note*.) But, again, enteritis, or muco-enteritis, is complicated with idiopathic fever; and here new phenomena arise in respect to the tongue, — such as were not often present in our author's cases; more complex influences now add greater diversity to all the functional lesions, and new localities of disease, as well as the constitutional form, more or less diversify the appearances of the tongue. But, these appearances generally bear a remarkable relation to the other symptoms; though the whole must be analyzed and compared, before either can receive its due importance. This is often the work of mind, and of enlarged observation alone; and if others fail of the task, it is no proof that nature does not supply the means for interpretation while she can receive any benefit from art. We therefore protest against those general insinuations that the appearances of the tongue do not bear, in a general sense, an instructive relation to the condition of diseased organs.

But, says our author, "the tongue *was almost always natural.*"(1) Now this, and all that follows in immediate connection with it, is contradicted by nearly every one of his exemplifying cases. Still, the foregoing affirmation will appear less remarkable when the reader looks back upon our quotations of his 41st 42d and 43d observations, (p. 713 — 715,) and upon the disregard of structural lesions of the brain which will appear in the sequel. The appearances of the tongue were such as are universally known to attend muco-enteritis and its consecutive lesions; whilst those of typhus, and every other fever, in their *fatal and protracted* stages, have ever differed from our author's account of the tongue in the *fatal and protracted* cases of the "typhoid affection." (See our tabular view.) We need scarcely say, in respect to typhus, especially when fatal and long continued, that it is so uniformly marked by a "dry and black tongue," that this appearance of the organ, as well as sordes, is very commonly held to be significant of typhoid fever; although the disease have been only pneumonia, or some other fatal inflammation. Subsultus, and low muttering delirium, it is true, are then generally more or less attendant upon true typhus; but, the violent spasms, and the "straight-jacket"-delirium of the Parisian disease, are *rarely* seen in typhus, but are common attendants, as in cholera, upon enteritic inflammation.

We need not, however, argue this point with our author, since it is the burthen of his work to show that the disease was not only local, but dependent *alone* upon a lesion of the glands of Peyer. Our author, however, has given to some the impression that he regards the disease as a constitutional fever. It appears to us that this can have alone proceeded from his involved manner of writing, the designation which he bestows upon the affec-

(1) Vol. ii. p. 55. — Hear our author at another time, when employed in making out a hypothesis. After stating that, "in connection with these *signs of inflammation* there were, in nearly all the cases, [of *recovery*, too,] pains, pricking sensations, dryness of the throat," &c. "those lesions lasted from two to twenty days," &c. — Now mark: — "Between *these lesions and those of the tongue there was a perfect analogy. If inflammation of the tongue* was sometimes superficial, at others connected with *swelling*, followed by *ulceration*, or accompanied by a *membranous exudation, such* was, likewise, the case *with the organs* forming the parietes of the mouth and fauces, in a proportion at least as great. And *why assign different causes to identically the same lesions, solely because they have not the same seat?*" (*a*) Or, why "assign different causes to identically the same lesions," because *ulceration, softening*, and *thickening*, are sometimes "*white*," and sometimes "*red?*"

(*a*) Vol. ii. p. 77, &c.

tion, and, especially, from his direct and indirect affirmations that the lesions of structure are not inflammatory, but "specific," "an opposite nature to inflammation," and darkly mysterious. That our author fully makes out his point, that the disease generally began in the intestinal canal, and radiated from that point, we think is evident from the great extent and uniformity of the structural lesions of the intestines, setting aside the symptoms. And, what shows most emphatically, (and independently of his express declarations,) the ultimate design of our author that all the lesions of organization should be regarded as the result of a contra-inflammatory pathology, is his repeated declaration of hostility to the school of Broussais, (1) and that we have little or no knowledge of general influences from intestinal inflammation. But, whilst there was nothing novel in the doctrines of Broussais, and although leading to great theoretical errors, especially in regard to idiopathic fever, it cannot be doubted that his system of pathology, and the practice which it inculcates, are to the interests of mankind, when contrasted with our author's, in the relative aspects of peace and war.

We will have one more generalization in respect to the tongue; since it supplies a good exemplification of our author's use of analogy. It is also one of the many of our author's analogical circles of reasoning by which he endeavours to show that the intestinal affection in "the typhoid fever" is of a contra-inflammatory nature. Thus:—

60. "In the cases in which the tongue had a yellowish coat, somewhat thick, the tongue was not RED underneath it, and presented no sign of inflammation. *Wherefore*, we must conclude that the *secretions* may be more or less seriously altered without the organs which give rise to them being in the least inflamed. This is an *important fact* since all the organs covered by the mucous membrane being susceptible of the same or analogous lesions, *we cannot doubt that the diarrhœa generally depends upon some other lesion than inflammation of the mucous membrane of the intestine.*" (2)

But, let us refresh ourselves once more as to our author's ostensible opinion of the worth of analogy. Thus:—

"This circumstance is, in fact, worthy of attention, inasmuch as it shows that it is not possible to decide that inflammation exists from the occurrence of a *single symptom*, without exposing one's self to very grave errors; and deductions from analogy are *not any more certain.*" (3)

(1) Thus,—"This affection, upon which the *new medical doctrine* depends in a great measure, *is really one the least known, and about which the fewest positive results have been given, and whose diagnosis is the most obscure.*"—*Vol. ii. p.* 44.

(2) Vol. ii. p. 89.

(3) Vol. ii. p. 226.

Here our author had in view the various analogies that might be brought to show the inflammatory nature of the lesions in "the typhoid affection."

Our author is copious on the subject of *abdominal pains*, and *chills*, in his work on "the typhoid affection." But, as his remarks on the former symptom relate mainly to the condition of the patients before they entered La Charité, (as indeed they do, also, to the chills,) we shall reserve their consideration for another place. As to the *chills*, we suppose it was intended as a general principle when our author says of a particular case, that,—

61. "The chills which occurred six days before death *indicated the development of a new lesion:*"

But, a little farther on we read, that,—

"The chills [in pneumonia] were repeated in 11 patients during three or four days, and they ceased, *as during the typhoid affection, after* entrance into the Hospital." And so of *erysipelas.* In *rheumatism* and *angina gutturalis*, "they returned *several times* in some subjects in consequence of the external application of cold, or without any appreciable cause." "*A daily, regular chill* occurred in a woman affected with the same disease [rheumatism] *a little before convalescence;*" and so on. (1)

Now the interesting inquiry is, whether each of the repeated chills in these last cases "indicated the development of a new lesion"?

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62. "*Experience* has shown that the *typhoid affection*, well characterized, *is not susceptible of being cut short at once*, which, moreover, is *not less true*, to all appearances, of *pneumonia*, and other *inflammatory diseases*"! (2)

63. and 64. "*General* bleeding was not more useful than *local;* so that we may say with truth, that up to the present time, medicine has no power over this symptom," [delirium.]

"*The little effect of bloodletting, in the typhoid affection*, ought not to surprise us, since we observe the same to be the case in almost an equal degree in diseases of another kind, and in those which are *plainly* inflammatory, pneumonia, erysipelas, &c." (3)

65. "The study of the general and local symptoms, the MORTALITY, and variations in the mean duration of the pneumonitis, according to the period at which bloodletting was instituted; all *establish narrow limits to the utility* of *this* mode of treatment." (4) "Practitioners have been misled in believing it possible to arrest pneumonitis, at its onset by large bleedings," &c. — "The facts relative to the fatal cases seem still farther to limit the utility of bloodletting." (5)

66. The essential "phenomena remain, [in pneumonia,] and are *even augmented in intensity and extent* after the first bloodletting, if this has been practised *soon after the commencement* of disease." (6)

(1) Vol. ii. pp. 216, 224. (2) Vol. ii. p. 443. (3) Vol. ii. pp. 412, 449.
(4) On Bloodletting, p. 13. — Reiterated at pp. 22, 36, &c. (5) Ibid. pp. 9, 28.
(6) Ibid. p. 29

The reason of this we have endeavoured to explain in our Essay on the Philosophy of the Operation of Bloodletting, particularly in Sections 9 and 12. See, also, page 697, and Volume I. page 311. That our author's inefficient abstraction of blood in pneumonia, erysipelas, &c. contributed to the severity of the disease we have no doubt, for the reasons we have assigned in the foregoing Essay; and we adduce the unfortunate results in confirmation of our accuracy. In the work on Typhoid Fever, we have a farther demonstration of this fact; and there is a frequent coincidence of language which shows that our author carried out practically his doctrine of the advantages of "confounding severe diseases." Thus: —

"The fatal course of the disease was more rapid in these patients [five who were bled early] than in those [seven] who were bled less promptly and less abundantly, *and so much the more so as the first loss of blood was nearer the commencement of the disease.*" (1)

This calculation is intended to establish a general fact, at least in relation to the fatal cases, and therefore by induction, in respect to all cases. This doctrine, indeed, is finally carried out to the exclusion of Bloodletting. "*However singular*," says our author, "*this result may appear*, [the foregoing,] *it is, nevertheless, confirmed by a thorough examination of facts.*" This "thorough examination of facts" consists in the foregoing comparison of 5 cases with 7; which forms the whole ground of this most important conclusion. "*Twelve patients*," says our author, "*were bled at various periods of the disease.*" And here our author's statement, that "*venesection was generally well performed in all the cases of which we are speaking*,"(2) should be compared with what is specified of the individual cases in our tables, by which the reader will learn something more of the "heroic" practice of the anatomical school, (p. 665,) and be able to appreciate the justice of our author's inductions as to the effect of bloodletting in the most profound and fatal inflammations. (Vol. I. p. 311, &c.) Again:

(1) On Typhoid Fever, vol. ii. p. 400.

(2) Vol. ii. p. 261.—"In five the blood was covered with a buffy coat." The American Editor remarks, "I know not what vessel was made use of to receive the blood in these (12) cases, but commonly in the *French* hospitals a *shallow* metal basin is used for this purpose." — *Ibid. note.* This is a material point; since our author appropriates the absence of buff "in a certain number of cases," to the contra-inflammatory nature of the structural lesions in "the typhoid affection." We have made many experiments, and have always found a wineglass to develope the buff and cup better than any other vessel.

"The immediate effect of bloodletting, or that which could be observed on the day after the bleeding, is nothing, or almost nothing, or is not evident, upon the state of the symptoms of the typhoid affection;" but, "bloodletting, performed twice during the first ten days of the disease, may shorten its course a little." Nevertheless, "whether bleeding was performed during the first ten days of the disease, or in the following period, the *delirium*, *somnolency*, and *stupor*, were not diminished in any case; in most cases, on the contrary, they were more marked on the day after, than even on the day of, the bleeding." (1)

It should be observed, too, that our author remarks of the amount of his experience, as exhibited by our table, that, "we should prefer *general* to *local* bleeding, the utility of the latter being not *so well demonstrated*, even in the affection of organs, which are superficially situated." Nevertheless, it appears, also, that "*general* bleeding was *not* more useful than *local*," &c. Again:

"I have attempted," he says, "to decide whether the mean duration of the disease did not offer some variety, according to the performance of bloodletting by the lancet, or by leeches, but I have not found any." (2)

What experience our author may have had with the cases which recovered, should be rejected according to his own rule with others; since he has given us a circumstantial account of only 4 of those cases. And this may be equally affirmed of all his cases of pneumonia, erysipelas, &c. in his work on bloodletting, as well as the instances of disease which he brings in comparison or contrast with "the typhoid affection."

But, most of all is it important to remark, that our author conveys the impression that more or less of these subjects were bled at an early stage of the disease, and that he establishes the most important of all practical conclusions upon this error. What were the facts? The subject of Obs. 1, was bled 10 oz. *six* days after the beginning of disease. Obs. 4, on the *twenty-first* day, ad 8 oz. Obs. 5, bled on the *twenty-second day*, two days before death, and once before admission. Obs. 6, on the *sixteenth* day, ad 8 oz. Leeches once. This patient, like Obs. 1 and 4, had the *straight jacket* (3) on the night of her admission to the hospital, and died the next night. Obs. 8, was bled before admission, and 10 oz. afterwards, (*tenth* day) "at her request." 12 leeches. Obs. 9, on the *sixth* day, ad 15 oz. and "20 leeches to ears," and straight jacket. Obs. 11, on the *fifth* day, ad 10 oz. Obs. 17, on the *ninth* day, ad 12 oz. and straight jacket. Obs. 18, on the *sixteenth* day, ad 16 oz. and once afterwards, and leeches, and straight jacket.

(1) Vol. ii. pp. 410, 409. (2) Vol. ii. pp. 407, 443.

(3) We state this fact to show the violence of the case.

Obs. 21, on the *fifteenth* day, ad 12 oz. and 18 leeches afterwards, "about the ears." Obs. 23, on the *eleventh* day, ad 14 oz.; "needed venesection on account of his severe head-ache," and straight jacket. This patient was supposed to have been bled before he entered the hospital. Obs. 26, on the *ninth* day, ad 8 oz. (1) Obs. 28, on the *third* and *fourth* days, once ad 10 oz.; the other quantity not stated. 12 leeches. Obs. 33, on the *sixteenth* day, ad 10 oz. and 27 leeches. Obs. 36, on the *fifth* day, ad 12 oz. and "8 leeches to each ear." Excessive delirium, spasms, and distress. Lived 17 days. Obs. 42, on the *twenty-third* day, ad 8 oz. and before admission. Obs. 46, on the *eighth* day, ad 8 oz. Obs. 48, on the *seventeenth* day, ad 10 oz. Lived 22 days after. Obs. 49, on the *tenth* day, ad 12 oz. Obs. 53, on the *thirteenth* day, ad 10 oz. (2) Obs. 54, on the *sixteenth* day, ad 12 oz. Being about an average general bloodletting for each patient, of 11 oz.; and an average application of the remedy on the thirteenth day of the disease.

And we now again ask our reader to carry with him the circumstances of this disease to what we have said of the experience of Robert Jackson, Rush, and others, in our Essay on Bloodletting, as to the dangerous effects of a partial abstraction of blood, either in violent fevers, or severe inflammations; or even when the amount is large, but taken at small and repeated operations.

A capital mistake is made by our author in affirming that, "of 52 patients who died, (of the typhoid affection,) 39 were bled (furent saignés,) a greater or less number of times; the others were not." (3) In the first place, from the minuteness with which the treatment is stated, even to a whole or "a half rice-fritter," the lemonade, the "pounds of ice" applied to the head, &c., there can be no doubt, that, when bloodletting was employed, it was always specified. If neglected, however, it must go with our author's rule, that such a case should be rejected from the general aggregate. It will appear, therefore, that only *twenty* of all the subjects of "the typhoid affection," who died, were bled; whilst only *five* of the patients were bled more than once, and in three of these instances, the remedy had been applied before ad-

(1) On the next day, (*Id.*) occurs in the original; but this clearly relates to other remedies.

(2) Here, again, (*Id.*) occurs on the following day; but this clearly relates to whey, and oxymel.

(3) Vol. ii. p. 398.

mission to La Charité. If leeching be included, only *thirteen* of the subjects that were not bled, had leeches applied. And since our author proceeds, according to the numerical process, to analyze the results of *bloodletting*, it is the more important, as it respects our author, that the subject should be placed in its proper aspect. We do not dwell upon it with any reference to its practical importance. There is indeed, a general looseness prevailing in our author's summary statements, of which bloodletting supplies an example; for it is stated in another place, that only 12 of the patients who died were bled. ("Douze malades furent saignés à diverses époques de l'affection.") (1) It should be observed that three of the subjects are said to have been bled before their admission to the hospital. But, as nothing is known or stated as to the quantities of blood abstracted, or of the period of the disease when taken, it can form no ground for calculation; or, if it have, then has our author placed this conjectural circumstance above all that enlightened experience, and those well digested and accurate observations which he rejects as unworthy of any confidence.

67. "The influence of bloodletting upon the progress of pneumonitis has been unimportant, unless *sufficient interval* has elapsed between the attack and the venesection; and no doubt, the reason is, that the disease had then more or less *approximated its natural termination.*" (2)

68. Nevertheless, "at the first glance, it would seem rather *unimportant* whether the patients, affected with pneumonitis, were bled, for the first time on the *second*, *fourth*, or *fifth* day of the disease;" and that "this difference is slight." (3) — That is to say, "an excellent mode of arresting diseases is to *confound* them, or at least *to make no distinction in the periods* at which such and such *remedies* were employed, as *I have pointed out in the preceding chapter.*" (4)

The three last of the foregoing generalizations, although conflicting with each other, are amongst the most dangerous of our author's doctrines; especially that which inculcates that pneumonia is a self-limited disease. And, to discourage the more all bloodletting in pneumonia, he concludes the 67th generalization with repeating, — "*These facts, as well as those which relate to its duration, establish narrow limits to the utility of bloodletting in this disease.*" In the first chapter of our author's work on bloodletting we have an account of 78 cases of *pleuropneumonia*, of which 28, or more than one third, "proved fatal." Nevertheless, —

(1) T. ii p. 308.
(2) On Bloodletting, p. 35.
(3) Ibid. pp. 26, 27.
(4) Ibid. p. 31.

69. "Pneumonia is, in fact, *almost invariably curable, though associated with tuberculous excavations*;" though our author has "never seen *pleurisy* entirely cured, with one exception, when coming on in the course of phthisis, not even when occurring before tuberculous softening has taken place." (1)

The last of the foregoing paragraph is a practical commentary upon our author's repudiation of bloodletting and vesication in inflammatory affections, as is the result of the treatment of the 78 cases of pneumonia; a part of which, it is true, were exasperated by an imperfect use of the lancet.

"Age had no appreciable influence, [in pneumonia,] every thing else being equal, upon the results stated, for this was nearly the same on an average among patients bled for the first time, before the *fourth* day, and among those who were not bled until *after* this period." — We have not arranged this as a generalization, since our author states that it "should not, I think, be advanced as a LAW." — Nevertheless, our author remarks, farther on, that, "*it is evidently to the age much more than to delay in bloodletting*, that we must attribute *the great fatality* of those cases which were not bled until *four* days after the commencement of the disease." (2)

70. "These, [11 cases,] and similar investigations, *establish* narrow limits to the utility of bloodletting in erysipelas of the face." (3)

71. Of 23 cases of angina tonsillaris, our author bled, or leeched 13; in 3 of which the disease was, "without doubt, violent, and it will be thought possible to explain the *excess of its duration* by its SEVERITY. I think the explanation *excellent*. But, what is the *conclusion*, except that the influence of bloodletting on the progress of the disease *is extremely limited*." (4)

It should be stated here, that, in a severe case, "bleeding was done *abundantly*, *fifteen ounces*, on the 3d and 6th days of the disease."

72. "From the same facts [the last preceding, in relation to angina,] we should be lead to question the great advantage of the application of leeches to the epigastrium in *gastritis*, or to *any other part of the abdomen corresponding to the viscera presumed to be diseased*. Indeed, what confidence can be placed on the *a priori precepts* commonly advanced on this subject?" (5)

"*A priori precepts!*" The whole of the foregoing, most sweeping and dangerous induction from analogy, upon false premises, to be found in any work of any acknowledged value, is not only, *by its own showing*, an a *priori* precept, but is a total disregard of the "numerical system." Thus does our author, in his zeal to reproach others, bestow the severest commentary upon his own habits. (6)

(1) On Phthisis, sec. 288. (2) On Bloodletting, pp. 6, 38.

(3) On Bloodletting, p. 54. — Perhaps the 21 cases, mentioned at p. 14, should be included. "It may have diminished the duration of the disease ½ or ¾ of a day"!

(4) Ibid. p. 19—21. (5) Ibid. p. 21.

(6) "Instead of aiming," says our author, "to make questions comprehensive,

And how stands it with the five remaining capital remedies that were employed in "the typhoid affection." These were *tonics*, *blisters*, *anti-spasmodics*, *ice*, and the *straight-jacket*. Of the first our author remarks,—

"On this subject, as on that of bloodletting, it is natural to ask whether *tonics* have done more than shorten the duration of the disease; whether they have saved from death any of the patients whose *histories* I have presented. Though the affirmative is not perfectly certain, it yet appears to me very probable." (1) "*Drowsiness*, *delirium*, *groans*, *cries*, *and spasms*, *continued* as before the administration of the *new* remedy," (*tonics.*) (2)

Leaving the tonics to "*allowable conjectures*," (3) we pass on to *blisters;* and here we find that,—

73. "*Blisters* ought to be *banished from the treatment of the typhoid affection.*" "If they exercised any influence upon the duration of the disease in the patients who have recovered, it was by *prolonging* it a little." (4)

And, here, let it not be forgotten, that the foregoing conclusion is given as the result of blisters applied, almost universally, to the *legs*, for the relief of an excessive inflammation of the intestines and of other important organs, even a violent cerebral affection; and this, too, either without preliminary bloodletting, or, at most, its inefficient application.

And so in pneumonia, and various other formidable inflammations; the conclusions being deduced from our author's limited cases, and under the circumstances which we have considered. (P. 697.)

74, 75, and 76. "I have not only *rejected vesication* from the treatment of *pneumonitis;* I have also ceased to employ it in *pleurisy*, and *pericarditis.*" "*How can we believe* that the effect of a blister is to check an inflammation

learned societies should have restricted the limits of the points to be investigated and discussed: and they would have done themselves more honour, in my opinion, if, instead of proposing as a prize question, to determine, by clinical observations, in what diseases the application of leeches is to be preferred to bloodletting; and when it is advantageous to employ both simultaneously; if, instead of questions like this, too comprehensive to be solved by *one man*, they had limited their inquiry to a *rigid exposition* of the effects of bloodletting in pneumonitis, for example, or any disease whatever, but in one alone; for then only they would not have asked impossibilities." (*a*)

The foregoing shows our author's opinion of the advantages of principles in medicine, as they may happen to be deduced by others. If practice were to repose upon the doctrines as here laid down, it would indeed be reduced to the most absolute empyricism, according to the requisitions of the "numerical system." But, has not our author attempted even the "impossibilities" which he would have the world at large avoid?

(1) Vol. ii. p. 435.
(2) Vol. ii. p. 415.
(3) Vol. ii. p. 436.
(4) Vol. ii. pp. 440, 448.

(*a*) On Bloodletting, p. 93.

when this blister is one inflammation superadded to another?" — In "thoracic inflammations, their usefulness is neither strictly demonstrated, *nor even probable.*"

77. "One thing is *most assuredly beyond question*, and we should *never* be weary of repeating it; that the *therapeutic value* of blisters is *not known;* that it must be studied by the aid of *numerous* and carefully noted facts, *just as if nothing at all were known about it.*" (1)

How does this correspond with the preceding denunciation of blisters? And now nothing remains for those terrible *spasms* but "*antispasmodics.*" But, ah! Spem dolosam!

78. "These medicines, the use of which has not been followed by success, ought, it seems to me, *to be banished from practice* under these circumstances." (2)

Coming at last to *ice*, we meet with nothing but the same frigid discouragement. (3) The knell is then sounded over the imbecilities of art, as it had been over the hearse of the dead; since,—

79. The "means which, according to all theories ought to have more or less success, and the *inutility* of which tends at least to prove that, at a certain period, *diseases* pursue their course with an obstinacy over which our therapeutic agents have but an extremely limited influence;" whilst our author "limits" himself, at another time, "to the remark that the effect of *therapeutic means was so little, that we may at present pay no attention at all to it while we make this general description.*" (4)

And this, too, in an acute disease that was rarely fatal at an early period, and in which the patients were often "at work," or moving about, for many days after its invasion, and which was generally prolonged till the most extensive disorganizations followed. And then comes the climax of despair; that utter inability to relieve the most formidable, and two of the most constant attendants,—*delirium*, and spasms, (5)—save only by the "straight-jacket." Even

80. "General bleeding was not more useful than local; so that we may say with truth, that up to the present time, medicine has no power over this symptom, [delirium;] a want of power the more to be regretted from the fact, that the derangement of the cerebral functions, independently of the alterations of the brain, may become, as we have before seen, *one of the principal causes of death;*" (6) notwithstanding, as we shall see, even the *structural* lesions of the brain are denied to have had any participation in the event.

But, whether the cerebral derangement, functional or structural, may or may not consist in a lesion of the glands of Peyer, or may not have a large and constant share, in the midst of so

(1) On Bloodletting, pp. 45, 46.
(2) On Typhoid Fever, vol. ii. p. 451.
(3) Vol. ii. pp. 442, 449.
(4) Vol. ii. pp. 442, 12.
(5) Vol. ii. pp. 449, 451, &c.
(6) Vol. ii. p. 449.

much destruction; and whether, when not relieved as well by art, as by nature, its fatal tendencies be not greater than the chances are favourable, must rest upon our knowledge of its frequency and violence, ("the cerebral symptoms masking all others,") (1) and upon that general experience which prompts us to give to the sufferer the last chance of hope, when delirium, depending on disorganizing inflammation of the brain,—especially straight-jacket-delirium,—may be the most distinguishing symptom of acute and violent diseases. "*Must we then,*" says our author, "*remain quiet spectators of a violent delirium, or of extreme and continued drowsiness?*" (2) Aye, "*who,*" indeed, "*would wait for the forlorn effort of nature when he could command a tampon?*" (3) *Shall* we look on with frigid indifference, because some 8 or 16 ounces of blood have only increased the fury of a disease which is sapping the entire organization of the body; even though there be added another trophy to pathological anatomy, or another triumph for anatomical practice? "*There is an axiom in midwifery, that no woman should be suffered to die undelivered.*" (4) (Vol. I. p. 329—330, *Celsus.*) And shall not they be held responsible, who, condemning all other experience, successfully propagate the doctrines which we have now recorded, and who would have our patients perish, one after another, to the end of the epidemic, without affording the chance of early, or tardy,—but however late,—of vigorous remedies? Shall we not strive, to the last moment of life, against a certain destroyer?

But, "although hope be a good breakfast, but a bad supper," we are encouraged to think that something may yet grow out of *chance*, and, perhaps, something from *observation;* since, "chance and observation," says our author, "have given us opium, cinchona, and vaccination;" and therefore, what "chance and observation have done, *they can do again*, and doubtless they will do." (5) We agree with Menander, that "the *truth* sometimes comes to light when least expected." But, is this the whole conclusion of "the numerical method," the only result of the *calculus* as applied to medicine, the *summum bonum* of the new philosophy? We grant our author that it is the antipodes of the

(1) Vol. ii. pp. 169, 451, *et passim.* (2) Vol. ii. p. 449.
(3) Dewees' Compendium of Midwifery, s. 1059.
(4) Ingleby, on Uterine Hemorrhage, p. 153.
(5) Vol. ii. p. 454; which closes our author's work.

old. "*Improbe Neptunum accusat, qui iterum, iterum, naufragium facit.*"

We are therefore, of the opinion of an illustrious poet, that in this matter of "the typhoid affection," there has been "much ado about nothing;" and, as the philosopher Stilpo relates of Diogenes, that the object has been sought with a lantern at noonday,—at least "in these clear-eyed and censorious times." Nay more; we believe with an eminent man on a similar occasion, that, "if such patients had a Rush by the side of their bed, they would not be transferred from it to the grave." And, *our prophet*, (p. 677,) casting a glance into futurity, has placed it upon record, that, —

"It was narrowness of information, *or neglect of past observers, whose authority ought not to be deemed inferiour to that of the present speculatists, that heightened the terrible calamities produced by this disease.*"

And, our prophet, whose eye ever beamed with philanthropy, and the fire of genius, kindly advises

"*The cultivators of morbid anatomy in France, to appropriate to themselves a few British and American ideas.*"

Indeed he brings in Sydenham as having participated in this *prescience;*

"Whose remark," he says, "has literally held to the *present* day, — that their whole *chance* of escape lies in their constitution, and that must be peculiarly happy, if it save them from the complication of the doctor and disease."

Nor did our prophet make an end of prophesying, till he told us of the *general issue that is to come.* For again it is shown, that, —

"If we consider all *schools* on a level in regard to what they have *supposed*, and what each has *seen;* on so impartial a survey, *later speculation will miserably sink in value.* OLDER OBSERVATION WILL RISE IN HIGHER PROPORTION, (p. 677;) so that the public, on taking its stock of real knowledge will find itself richer." (1)

Let us, then, take warning from this forecast of the human mind; since there is nothing that so establishes conviction as the consummation of prophecy; which is, also, thus proclaimed from the watch-towers of Britain: "The French Physicians seem as if their only object was to find out disease, without any reference to its relief or cure." (2)

And now, if we turn to our author's latest work, the treatise on Yellow Fever, we shall find not only the same spirit of gen-

(1) Beddoes, Researches Anatom. and Practical, concerning Fever and Inflammation, pp. 84, 167, 230, 239. — 1807.

(2) Armstrong, Lectures, &c. vol. i. p. 100.

eralization from isolated facts, but the same localization of the fever according to the organ which most frequently manifests some physical changes, the same practical substitution of empyricism for philosophical principles, the same dependence upon "chance," and the same avowed imbecility of art which we have hitherto observed.

The only remedy of any moment, which is suggested by our author, is that of very moderate bloodletting from the feet; being equivalent to "blisters to the legs" in the typhoid affection. The *liver* is considered the capital seat of the disease; and, upon that subject we read, as in respect to the glands of Peyer, that, —

81. "In *all the cases, its alteration appeared to commence with the first symptoms,* (1) and, perhaps, many therapeutical agents might be directed against it. Unfortunately the NATURE OF THAT ALTERATION IS UNKNOWN TO US, so that *we cannot propose a remedy to be used against it* WITH ANY CHANCE OF SUCCESS. *The discovery of the remedy must be left to time and* CHANCE, and the acuteness of observers; for *experience* has sufficiently proved that no dependence is to be placed on mercurial preparations of any sort." (2) (See p. 692.)

Finally, the most imperturbable silence is observed on the subject of cathartics; nor do we find any special reference to this remedy in our author's writings, except in a case of "the typhoid affection," where our author supposed that a purgative, taken by a patient *seven* days before his admission to La Charité, had been the cause of his death. (3) We have adverted in a note, (p. 720—722,) to the disastrous results of neglecting, in "the typhoid affection," the proper means of removing the feculent matter, which is frequently admitted by our author to have accumulated in the large intestine in large quantities, and to have often "moulded," (4)

(1) This induction appears to have been founded upon a *solitary case*, that of Scott, whose liver was found affected "*three days and some hours*" after the disease had made its invasion, — and this disease *Yellow Fever*.

(2) On the Yellow Fever of Gibraltar, of 1828, p. 337. — See Boston Medical and Surgical Journal, Nov. 13, 1839. p. 228.

(3) Vol. ii. p. 179.

(4) Besides our former references, see vol. i. pp. 31, 45, 51, 52, 67, 73, 80, 85, 90, 98, 106, 119, 135, 147, 191, 199, 213, 220, 222, 241, 300. 348, 350, 362, 370, 376; and vol. ii. pp. 18. 62, 70, 98, 122, 167, 177, 215, 356, 363, 382. In a few cases only our author makes no reference to the subject.

The exhibition of cathartics in the condition of our author's "typhoid" patients being a subject of a more than ordinary interest, on account of the violent muco-intestinal inflammation, we will say, for the benefit of our junior readers, that that inflammation should have been in the first place partially subdued by effective bloodletting, leeches to the anus, and a blister to the abdomen instead of the "*legs.*" An unirritating cathartic, like castor oil, should have been then exhibited, accompanied, perhaps, by some mild preparation of opium, like the camphorated tincture, or by laudanum.

whatever may have been the attending diarrhœa. Whoever will consult our references below, and others at the page as indicated above, will see that feculent matter was so considerable in almost all the cases as to have been worthy of special record; it being stated, also, as having been very abundant and "moulded" in several instances, and "retaining the form to a greater or less extent of the canal." Our author's general affirmation, therefore, that "the contents of the intestine were not generally abundant," is as inaccurate as his general remark that the "tongue was natural," or that 39 of the subjects who died were bled.

We may also say, as indicative of the *contra-inflammatory* nature of the intestinal destruction, that it is frequently stated by our author, that the small and large intestines contained "a considerable," or "a large quantity of mucus." Again:

"Except one case of perforation, in which there were scarcely any traces of mucus in the intestine, all the subjects contained a greater or less quantity of it. It was PARTICULARLY ABUNDANT near the duodenum *in half* of the patients who died between the eighth and fifteenth days of disease, and in a *fourth* part of those who died after this epoch." (1)

Our author concludes the foregoing statement with the remark that, "this difference can hardly be accounted for *by the state of the mucous membrane*." Here, then, as everywhere else, the pathology rests upon the sensible alterations of the organization. But, we will not close this subject without saying that the "abundance of mucus" is one of the numerous and incontestable proofs, that the intestinal lesions depended upon a violent inflammation, and that the diminution of that product, towards the end of life, was the consequence of diminished inflammatory action, and disorganization of the parts by which the mucus is generated.

What, then, are the substantial remedies for inflammations and fevers, which our author has deduced from his various "researches"? Bloodletting, leeches, blisters, cathartics, tonics, antispasmodics, are all excluded. Mercury has no place in his creed. In all the diseases which our author has investigated, he has shorn us of every remedy, excepting tartarized antimony in pneumonitis, and upon this we are to depend alone.

Shall we then be surprised that our author looks upon "medicine as now in its infancy?" and, as to our author's pathology, is it not everywhere apparent that his *primary* considerations are the *ultimate results* of that morbid *action* about which all principles and practice should be most interested? Those results

(1) Vol. i. p. 165.

being ascertained with tedious prolixity, the mind is left to despond over the sad exhibition. If we look for any useful inductions, we find nothing but deformities for science, and discouragement for the hand of art. Such we believe, will ever continue to be the fruits of morbid anatomy as cultivated by the strictly "anatomical school." We are justified in this conclusion by the records which surround us. Tome upon tome are filled with the details of structural lesions, which, too, have generally happened before the subjects have come under medical auspices; whilst the great principles of medicine are founded upon those results, and the ultimate object of the whole, the treatment of disease, is not unfrequently subjoined in the way of an ordinary postscript. (1)

In the midst, therefore, of all this admitted imbecility of art, shall we not rather return to the "experience" of those sages who were guided more by the light than by the darkness of nature,—even if it be allowed that they "considered therapeutics as a simple corollary of *pathology*"? (2)

We had nearly done our author an injustice in saying that he had left the whole treatment of diseases, with the exception of tartarized antimony in pneumonitis, to future "chance and observation," after having abolished all remedies which former experience and philosophy had established as fundamental. The following, however, constitutes an exception:—

82. "On account of the nature and seat of the disease, drinks cannot, doubtless, *be too copious*, and we cannot even assign limits to them in this respect, except those which are indicated by the aversion of the stomach"! "The abundance of the drinks ought to be, moreover, in proportion to the intensity of the febrile action, which is itself generally *in proportion to the extent and seriousness of the specific lesion of the small intestine*." (3)

The foregoing applies to "the typhoid affection," and we have already seen that our author sometimes specifies "three quarts of lemonade" as the daily allowance to an individual in the exasperated stages of the disease; besides a corresponding proportion of other fluids. We therefore feel ourselves bound to protest against this most pernicious practice. Lemonade should never be permitted under any circumstances of disease; least of all in intestinal inflammations, or where the digestive organs may be otherwise deranged in their functions. The objection to it in other affections applies to its great liability to disturb the alimentary canal, and thus to establish either a new disease, or a new source

(1) See Louis, On Typhoid Fever, Vol ii. p. 12. (2) Ibid. on Bloodletting, p. 77.
(3) Typhoid Fever, Vol. i i.p. 444.—

of irritation. And who does not see that our author's unlimited quantities of other fluids could not have failed, by their weight and mechanical distension, of aggravating the distressing cases of "the typhoid affection"?

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83. "The existence of tubercles in the lungs is the cause, and constitutes the special character of phthisis." (1)

The translator states in a note, that,—

"This proposition is flatly denied by M. Broussais, who asserts that he has *frequently* seen cases of *consumption* from chronic inflammation and suppuration of the lungs, where *no trace of tubercles* existed after death. He confidently refers to *ten* cases detailed in his course of lectures during the winter of 1833.

Here, then, we have all that the "numerical system" requires; or, at least, as it is observed in our author's work on bloodletting, and we suppose, therefore, that Broussais' experience will be permitted to corroborate that of most other correct observers. We would also refer the reader to Wilson Philip's philosophical essay on Indigestion, where he will find ample proof of the fallacy of our author. The mucous surfaces in every part of the body are liable to purulent secretions without any ulceration.

84. "The last morbid alteration was *peculiar* to phthisis; viz., *tubercles, wherever they might be found.*" "Their presence in these last viscera [lungs,] seems a *necessary* condition for their development in other parts." — "We will remark that we have found ONE EXCEPTION to the LAW *we have established.*" (2) (P. 181—204; and vol I. p. 626, *note.*)

Many observers have added to our author's solitary "*exception*" to the foregoing celebrated "law;" among whom we may refer to Mr. Calder, who has brought forward no little proof to invalidate what he denominates "the sweeping statement of M. Louis, that the presence of tubercles in the lungs is essential to their being in other organs." (3)

85. "Tubercles are nearly *equally frequent* in individuals of a *strong* as in those of a *feeble* constitution." (4)

86. "The influence of dress, and especially of *stays*, on the production of phthisis, is also perhaps a *mere assertion.*" (5)

87 and 88. "The sex which seems the most exposed to phthisis is least frequently attacked by *pneumonia* or *bronchitis;* and this *in the proportion* 1 to 3." —From which our author makes another most important induction; viz., "the opinion, then, that pulmonary tubercles are the result of *chronic inflammation* of

(1) On Phthisis, sec. 2. (2) On Phthisis, sec. 212.

(3) Lectures on Bloodletting, in Lon. Med. Gaz. July, 1838, pp. 631, 697. 698.—See, also, exceptions in Andral's *Clinique;* and Lombard's Essay *Sur les Tubercules.*

(4) On Phthisis, sec. 479. (5) Ibid. sec. 472.

the bronchial mucous membrane, pulmonary parenchyma, or pleura, on whatever theory it may be supported, is quite unsatisfactory." (1)

It is here worthy of special remark, as distinguishing our author's habits of observing the vital signs of disease, that it was his experience in phthisis, that,—

"*Dyspnœa* was generally *very slight*, not complained of by the patients, and seldom even noticed unless after exercise." "We have never met with more than 3 cases, where the patient was compelled *to lie with the head much elevated*." (2)

89. "These morbid productions [tubercle and mucus] were certainly not the result of bronchial inflammation. To support the contrary idea, *we must prove* that bronchitis of 24 *hours' duration* could have produced tuberculous deposit."(3) —See this case examined at p. 631—633.

90. "It results from this comparison of cases, [our author's cases,] that tubercles and tubercular excavations are nearly without influence over the development of pneumonia in the last stage of phthisis." (4)

91. "The morbid changes observed in the brain, and parts connected with it, are *equally common* after phthisis and other chronic affections." (5)

92. "We do not consider hydatids [of the brain] as peculiar to phthisis, but from what has been hitherto observed, we are inclined to believe *that this is the fact with regard to tubercles*." (6)

The following doctrines are practically, as well as philosophically important:—

93 and 94. "We shall be very brief on the morbid alterations in the lungs, only remarking, that the frequent recurrence of inflammation on the right side confirms what we have advanced relative to the *harmlessness of pneumonia* in phthisis, and that *tubercles* may be regarded, in *this respect*, as *foreign* bodies, the influence of which is almost always *less injurious* than of *internal* causes. (7)

We may also add, that whilst our author is engaged in proving the non-inflammatory origin of tubercles, he is everywhere else, even in respect to tubercles, employed in demonstrating every variety of inflammatory lesions, and in showing that phthisical patients are the constant subjects of inflammation in different tissues.

It may be worth saying, whilst alluding to our author's work on Phthisis, that he supplies the following evidence of the non-inflammatory nature of certain lesions which we have hitherto considered in relation to that affection, and to certain cases of acute disease. (See pp. 631—632, 713—715.) Thus:—

"These adhesions, [between the pulmonary and costal pleuræ,] were the consequences of *chronic inflammation* of variable duration."

And what proves especially the non-inflammatory nature of

(1) Ibid. sec. 472. (2) Ibid. sec. 243. (3) Ibid. sec. 474.
(4) Sec. 40. (5) Sec. 206. (6) Sec. 207, Bowditch's version.
(7) Sec. 331.

tubercle, is the fact, in relation to *these* inflammatory adhesions," that,—

"We have twice found a *false membrane* lining the pulmonary and costal pleuræ, CONVERTED INTO TUBERCULOUS MATTER." And so, of a "*false, semi-cartilaginous membrane.*" (1)

95. "The *causes* of the *fatty transformation of the liver* appear to us equally *obscure* as those of *other organic diseases.*" "This lesion is *almost confined* to cases of phthisis; so that it may, to a certain point, *be considered as depending on this affection.*" (2)

Here should be observed the obscurity with which our author covers the pathological causes of all organic diseases, a knowledge of which is so indispensable in a practical sense.

96. "The LAWS *settled by pathological anatomy* [!] become still more important," &c. "Peritonitis, when of a chronic character from its commencement among adults, that is after the age of 15 years, is, according to the facts which I have recorded, *constantly tuberculous.*" "So that when there exists a case of well marked chronic peritonitis, we are able, independently of the symptoms referable to the respiratory organs, or *even in their absence*, to recognise the existence of phthisis." (3)

97. "When œdema follows pulmonary emphysema, it is the consequence of a coexisting *organic affection* of the heart." (4)

98. "If we cannot say that intermittent fevers *consist only* in a more or *less serious alteration* of the *spleen*," "nevertheless, the condition of this organ merits great attention from those who study intermittent fevers, since it is evidently affected *much more frequently than the other organs.*" (5)

This is one of the numerous instances in proof that our author ascribes all fevers to a *structural* lesion of some organ which he may happen to find most frequently altered after death; and is one of the great evils which morbid anatomy has inflicted, from time to time, upon science and humanity. (See *Sprengell in the introductory extracts.*) But, let us look at an induction which is applicable to cases of recovery from intermittents:—

99. "It is *infinitely* probable that in *all*, or nearly all, the cases in which the left hypochondrium was painful, the *spleen* was more or less *altered*, and it is hardly possible to consider the pain as having been caused by any other organ." (6)

100. Our author found "the *solitary crypts or glands* more or less enlarged, white or reddish, in three individuals who died of scarlatina, the *only persons* who died of this affection of whom I made an autopsy." *Therefore*, "this would seem to *prove* that the enlargement of the glands, (supposing *this was the lesion*,) if not *constant*, is of very frequent occurrence among those who die of this affection." (7)

(1) Sec. 43, 44. (2) On Phthisis, sec. 161.
(3) Louis, on the Method of Observing, *tr.*
(4) Sur l'Emphysème, &c. in Mém. de la Soci. Méd. d'Observation; t. i. p. 232.
(5) Typhoid Fever, vol. ii. p. 251. (6) Vol. ii. p. 249. (7) Vol. i. p. 195.

"The heart was softened *once* in two cases of scarlatina; twice in two cases of variola, and in two cases of phlegmonous erysipelas of the lower extremities."

From the last "numerical" amount our author draws the general conclusion, that,—

101. "If we suppose these last proportions *an effect of chance*, it is not less certain that *softening of the heart* is a very frequent lesion in patients who die during the course of acute diseases of the skin; *more frequent, doubtless*, than in those who die of any other acute disease, without the exception *even of typhoid fevers*." (1)

102. "All that we can *possibly* conclude respecting the influence of phthisis on the heart, is that its volume is *diminished* in common with that of the other organs." (2) On the other hand, Clendening says that, "he has shown that in phthisis, the heart *usually exceeds* the normal dimensions in both sexes." (3)

103. "We therefore think that hæmoptysis, (with certain exceptions) whenever it occurs, renders the presence of tubercles in the lungs *infinitely probable*," or, in other words, — "hæmoptysis, if not certain, is at least *an infinitely probable* indication of pulmonary tubercles." (4)

104. "When hemorrhage occurs in any internal organ, it is *almost constantly* a symptom of more or less considerable alteration of structure"! (5)—*See Andral's opinion upon this subject, at p.* 558.

105. "We have the most decisive proof we can have, of the necessity of a *special* cause for the production of hæmoptysis." (6)

106. "As the mucous membrane of neither intestine presented any CHANGE in any of these subjects, [of other acute diseases,] I inferred, in the second part of this work, that this change was *accessory*, and came on after the commencement of the principal disease." (7) From which it follows, that "typhoid fever" has its seat in the glands of Peyer.

107 and 108. "So that we may admit, that out of 84 more or less severe cases of enteritis, there were 12 examples of an affection of the mucous membrane of the stomach *consequent* upon that of the intestine." (P. 700.) "Pathological anatomy puts all doubt at rest upon the subject *in those who died*, and the comparison of the symptoms experienced by those who recovered, with those experienced by persons who died, renders the circumstance nearly as *certain for the former as the latter*. And not only does this comparison *demonstrate the fact in a general way*, but, it indicates, with much precision, the *proportion* in which the *lesion* occurs." (8)

109. "It results from these facts, [the foregoing,] that this lesion is less fre-

(1) Vol. i. p. 288. (2) On Phthisis, sec. 65. (3) Crounean Lectures, 1838.

(4) On Phthsis, sec. 233, 378.

(5) Ibid. sec. 233, 378, 394.— These two last most important generalizations, like most others, are without any foundation. We appeal, indeed, to the three foregoing sections, which are the only ones of any moment that relate to the subject. They afford no proof of the opinion expressed. The application of analogy, in sec. 233 is not defensible in any writer, on account of an error in the premises. Upon this subject, and the pathology of tubercle, &c., we have an extraordinary specimen of reasoning in a circle, and upon assumed premises, in section 240.

(6) M. Louis, in Mém. de la Société d'obs. de Paris, t. i, 1837. Sur. l'Emphysème, s. 7.

(7) On Typhoid Fever, vol. ii. p. 22. (8) Vol. ii. pp. 53, 54.

quent in those who recover than in those who die, and likewise, that it is less profound: moreover, that its degree and frequency *is in proportion to the violence of the febrile excitement*, whatever may be the result of the disease." (1)

110. "Although *merely negative*, the last fact seems to me worthy of notice, and to *verify a demonstration* which possibly may seem *already complete*." "This almost complete absence of lesions of the tongue in colic from metals is a new proof of the *influence of fever in their production*. And the ONLY CASE in which there was some inflammatory appearance of the tongue fully concludes the demonstration, since it relates to ONE of the FOUR patients who had *delirium*, and at the same time, somewhat of a febrile excitement." (2)

Our author did not happen to find lesions of structure in his cases of "colic from working in metals." (See Vol. I. p. 553, *Hunter*.) The last clause of the foregoing paragraph is worthy of consideration.

111 and 112. "Twenty-eight out of eighty-four, [one-third,] affected with *enteritis* had no pain in the head." "Thirteen, or only a sixth part of the subjects of *colica pictonum*, had headache during two or three days almost constantly in a slight degree," &c. Therefore, "what occurs in the cases of colica pictonum agrees with this statement, [of enteritis,] and *proves* that this proportion is a LAW." (3)

113. "It would seem, therefore, as if the pains [in the left hypochondrium and abdomen] were to be considered as *essential* to the affection, [intermittent fever,] the others being only an accessory symptom"! (4)

114. "These facts [in relation to the *tongue* and fauces in intermittents] seem to me to be *very precious*, inasmuch as they *demonstrate more clearly than any others* the part which the *different organs* of the economy take in *febrile* affections, *even* when intermittent." (5)

Here may be observed one of the numerous and important collisions of our author with himself; since, in the typhoid affection he denies to the tongue any importance in denoting the "part which the different organs of the economy take in the febrile action."

115. "We have never seen apoplexy come on at the close of diseases of long duration; that this fact makes an additional distinction between *softening* of the brain and *apoplexy;* and it points out a *fresh analogy* (p. 695—699) between hemorrhage of the *brain* and that of *other organs*, *which so seldom occurs*, IN ANY OF THEM, WHEN THE DEBILITY IS EXTREME." (6)

First; "*M. Rochoux asserted that he had never met with a single case of a foyer apoplectique unaccompanied with ramollissement of the surrounding cerebral substance, and challenged his opponents to show him such a case.*" (7)

Secondly. Do not the greater proportion of medical philosophers, whether rightly or wrongly, ascribe hemorrhage to debility,

(1) Vol. ii. p. 54. (2) Vol. ii. p. 88. (3) Vol. ii. p. 115.
(4) Vol. ii. p. 250. (5) Vol. ii. p. 257. (6) On Phthisis, sec. 371.
(7) Medico-Chir. Rev. Lon. No. 67, p. 226; 1837.

as its proximate cause? And is it not *truly* the fact, that hemorrhage supervenes more constantly in the protracted than in the early stages of disease, and, therefore, when the "debility" is most "extreme"? Is not this doctrine, also, of some practical, as well as pathological importance, to those who are laying the foundation of a medical education?

We shall now add a generalization which is of fundamental importance to our author's doctrine of the "specific alterations of structure" in the typhoid affection, and the contra-inflammmatory nature of ulceration, softening, thickening, &c. "in a certain number of cases." It is one of the constantly recurring examples of our author's respect for his avowed objections to analogy where it has been made to subserve the cause of science in the hands of others. Thus:—

116. "The extent, severity, especially the rapid progress of the *erysipelas* deserve to be remarked upon, for this rapid course occurs in a number of affections which come on during the last days of acute disease. The permanent character of the RED colour of the skin must be remarked upon as one of the numerous facts which prove that a *really inflammatory redness does not disappear after death.*" (1) (See pp. 703, 705.)

Whilst adverting again to the subject of inflammation as constituted by *redness*, and contra-inflammatory action as consisting in *whiteness*, (p. 701—709, &c.,) it has occurred to us that another extract from our author may enable the reader to balance our author's opinion the better as to the pathology of all the great lesions in "the typhoid affection;" especially, as has been seen of our author's therapeutics, it may be of some practical moment to ascertain how far *white* and *red* are to be taken as settling the pathology of a most destructive disease. It will be also borne in mind, that our author, in his work on "the typhoid affection," holds it to be absurd that "diminished thickness of our tissues" is the consequence of inflammation. We will however, in the first place have a summary extract as to the changes consequent upon the contra-inflammatory action. Thus:—

"Without noticing farther the softening of the heart, or the redness of the aorta, I will remark that the mucous membrane of the large intestine had, notwithstanding its distension, at least, its usual thickness; consequently there was a real thickening. It was also PALE throughout its whole extent, and *softened* in some points only, so that we *cannot attribute this excess of thickening to inflammation.* The muscular coat itself was a little *thickened*, and we shall see here-

(1) Typhoid Fever, vol. ii. p. 160.

after that this DOUBLE LESION must be regarded as the *effect of the meteorism*, or of the *reaction* it excites in the different coats of the colon." (1)

In the second following case, however, "the mucous membrane of the colon was REDDISH and *very much thickened*;" which leads our author to conclude,—

"That *this* thickening was the *combined* effect of *inflammation* and of the *reaction* produced by the *meteorism* upon the tissues which form the parietes of the large intestines"! (2) (Pp. 712, 719—721.)

True, we have already abundantly considered this subject; but, as we desire to make an impartial exhibition of our author's doctrines by quoting from various parts of his writings, we have concluded to reproduce an inquiry of so much magnitude as inflammation, and that "exactly opposite" pathology which results in the same lesions of structure. We shall therefore, go on with the doctrine of *paleness* in its opposite aspect to the foregoing. Thus:—

117. "*Diminished thickness* and PALENESS of the mucous membrane *are not opposed to the idea of inflammation;* for we daily see after the application of a blister, and frequently but for a short time only, the skin become thin and completely destroyed, and we might expect *paleness to accompany approaching destruction.* We have more frequently remarked this state of the *stomach* in women than in men, in the proportion of 12 to 7, which seems to indicate that the most common exciting causes are not excesses in eating, women being less addicted to them than men." (3)

Nevertheless, our author is adverse to the supposition that the same *morbid changes* may take place *after death as during life:*—

"We cannot suppose," he says, "an *identical morbid change* to arise from two causes so wholly different from each other." (4)

Here, too, let us refer to our author's diatribe against "*gastritis*," at page 700. And again:—

"This morbid condition of the stomach was very rarely accompanied by characteristic symptoms, but it was of too definite a character to admit of any doubt as to its nature. For intense *redness* combined with softening, and sometimes increased thickness, could only be the result of inflammation." (5)

Nevertheless,—

118. "From what has preceded," says our author, "we may *conclude* that the *greater number* of the morbid conditions of the stomach is characterized by *peculiar symptoms*," in phthisis. (6)

119. "Vomitings, preceded during a certain time by anorexia and accompanied by epigastric pains, [in phthisis] are generally indicative of a *very serious alteration* of *the mucous membrane of the stomach.*" (7)

Our author allows, however, that,—

(1) Vol. i. p. 19. (2) Vol. i. p. 26. (3) On Phthisis, sec. 89.
(4) Ibid. sec. 93. (5) Sec. 93. (6) Sec. 342. (7) Sec. 342.

"Here, as in 1000 other circumstances, there are FACTS which *defy the sagacity* of the observer, and *seem exceptions* to the *most general laws*." (1)

Notwithstanding our author's opposition to such an affection as "gastritis" in his work on "the Typhoid Affection," Broussais never defended it more than does our author, *per se*, in his work on Phthisis.

In respect to intestinal ulceration, our author institutes a comparison between those of "the typhoid affection" and those of other diseases.

"Out of 68 patients who died of pleurisy, pneumonia, erysipelas of the extremities, and an affection to which I could give no name, the mucous membrane was not ulcerated in a single case; it was much softened only in two." (2)

But, what more does this prove, than that "the typhoid affection" was an intestinal inflammation, and that the other diseases were not?

Although it may have been well to have kept before the reader the general spirit of our author's writings, we have found it difficult to arrange their analysis in that luminous manner which we could have desired. This has proceeded, in part, from the variety of the subjects, but especially from our author's constantly involved contradictions of himself. One of these we shall now exhibit in relation to what has been already stated of the *mesenteric glands* and of the *glands of Peyer*, at page 705. It would appear there, that our author allows the alterations of those bodies to depend upon *inflammation*. But, it is not so in his work at large, where they are imputed, in a general sense, to the same *contra*-inflammatory action which is said to be the pathological cause of the structural lesions of the intestinal mucous membrane, of the liver, spleen, heart, &c. We shall make our citation to the *whole* of the foregoing effect from the very page of our author in which the affirmation occurs as we have already stated it at page 765. Thus:—

"In some, instead of the more or less *deep amaranthine-red colour*, which they [the mesenteric glands] had generally, the glands were *grayish*, *bluish*, or *purplish*; somewhat smaller, and softer, than in the other cases in which their size and softening were as great as in the individuals of the preceding groups. *This state also was in harmony with that of the elliptical patches, whether ulcerated or not*, which in the same cases had a *similar hue*, and were rather less softened than the *red* patches. And, as these *different degrees of change of colour*, of *consistence* and *thickness*, DEPEND UPON THE RETURN of the patches towards a *healthy* state, we cannot doubt that the *bluish*, or *purplish* colour of *the glands*, their *volume*, and their rather smaller degree of *softening* depended upon *the same cause*."

(1) Sec. 342. (2) Typhoid Fever, vol. i. p. 222.

"In some patients who died after the thirtieth day of disease, the *violet*, *grayish*, or *bluish* colour predominated; so that in two of them only were the glands of a *vivid red* colour, and these patients died in consequence of *perforation* of the small intestine. As a change of *colour* had taken place, so the *size* and especially the *softening* of the glands were much less than in individuals who died during the preceding period, and this was in perfect accordance with *the state of the patches*, whose *retrograde march* was, as we have previously seen, much more advanced than in the others. And, if this simple examination does not convince the reader that the state of the *mesenteric glands* was, as that of the patches, *the effect* of a *retrograde* course in the disease; and that they had been, at some *previous* time, softer and of a *different colour*, (see our p. 701 — 704,) he will be satisfied upon the point by considering that the glands, nearest the cœcum, contained in two cases *small collections of pus*." (1) (See *Gen.* 12 and 14.) Thus the lesion was not merely the *same* as in the preceding series, but it followed *the same course*, having been *more severe* near the end of the ileum than any where else." —

Then follows the remark quoted at page 705; and so far, we have nothing, apparently, but inflammatory action, obscured, it is true, in the usual manner by much trifling about colours. (See, also, extracts at pages 708, 717, &c.) Our author then continues; —

"If the state of the mesenteric glands, corresponding to the diseased elliptical patches, was the *evident* consequence of disease of the *latter*, [no assumption this,] it may be asked *what was the* CAUSE *of the morbid changes* in the glands we are now speaking of? Although their slight affection was INVARIABLY THE SAME, except in *one* case *with regard to* COLOUR, the *canse of it was perhaps* NOT CONSTANT." [A short reference to cases follows, which we omit as unimportant.] "But, in whatever manner we explain these last six cases, it is certain that the *increased size* and the *rose* colour of the glands could be attributed simply TO THAT CAUSE ONLY *which produces so many other secondary lesions, and* which has been spoken of several times in reference to those of the *mucous membrane of the stomach and intestines*." (2)

By turning to *Generalizations* 10, 11, 12, 13, 14, 15, 16, 19, 20, and other extracts at pages 709, 711, 712, 713, 715, the reader will have a full understanding of the CAUSE to which our author refers at last, in the foregoing paragraph, and of his consistent philosophy of "the typhoid affection."

And now, after all that we have seen of this confusion of causes of the same lesions of structure, and where the symptoms had been so identical that our author says he "could determine when any new lesion took place," and, again, what has been seen at other times, of the conflict of generalizations with each other, and all the importance which is attributed to colour, but at other

(1) Our author says that "pus is the most marked characteristic of inflammation." — *Ibid. vol. i. p.* 254.

(2) On Typhoid Fever, vol. i. p. 231—234.

times an absolute denial of its importance, (p. 703, &c.) what confidence shall be reposed either in our author's pathological doctrines of "the typhoid affection," or in that far more important disease, pulmonary tubercle? (See p. 629 — 633.)

> "Yet, though he tease and baulk your listening ear,
> He makes *one useful point exceeding clear;*
> How'ere ingenious on his darling theme
> A *sceptic in philosophy* may seem,
> Reduced to *practice, his beloved rule*
> Would only prove him a *confound*-ed fool." (1)

We have reserved for the last, and a distinct consideration, our author's opinion and generalizations as to the agency of the glands of Peyer in "the typhoid affection." It has, indeed, been already shown that our author regards the disease as a *mysterious intestinal affection*, which leads Müller to call it "*typhus abdominalis.*" (2) This fact, however, has been denied by our author's friends, and we observe that so cautious and enlightened a philosopher as Dr. Jackson affirms that, "*M. Louis did not show, nor did he attempt to show, that the disease he described was dependent on the morbid affection of the small intestines.*" (3) We have adverted especially to this statement, not only on account of the distinguished source from which it proceeds, but as it has naturally contributed to the currency of our author's work in America. Our author, however, refers the whole typhoid disease, its entire causation, (even more than in an original sense,) not only to "the affection of the small intestines," but *specifically*, to the lesion of the *glands of Peyer.*

It is certainly of little importance to establish this point, abstractedly; but in a physiological and pathological sense, it is of no little moment, — and this in proportion to the ascendancy of our author's doctrines over that experience and science which he treats with so little reserve. Our author has made all the great lesions of "the typhoid affection," all the constitutional disturbances, and even the cerebral affection, to revolve about the lesion of the glands of Peyer. This is his point of departure, here all

(1) Cowper's Conversation. (2) Elements of Physiology, vol i. p. 284.

(3) Jackson's Report on Typhoid Fever; *Introduction, p.* 8. Boston, 1838.

Dr. Hale observes in his "Report," that "in France, especially by many of the pupils of Louis, it [the affection of the glands of Peyer,] is considered as the chief cause of all the phenomena, without which they cannot be produced. *I do not perceive that M. Louis himself has any where distinctly expressed this opinion.*" — *p.* 71. This being a prevailing belief, we have been, in consequence, abundant with quotations from our author, though neglecting many others of the same import.

his reasoning terminates, and here sits enthroned the whole essential pathology of the disease, and for *no other* reason than that the glands of Peyer were found *rather* more frequently altered in structure than any other part. (P. 682, *Sprengell.*) Little, very little, is allowed even to the disorganized state of the alimentary mucous membrane, or of the liver, or even of the brain itself; (1) and nothing whatever to functional disease, though attended by an injection of the arteries and veins, unless there exist manifest lesions of structure. Indeed it is with our author one constant reiteration of "proportions and disproportions between the *symptoms* and *lesions of structure*," (2) — always terminating in the glands of Peyer. How far functional disease, and, indeed, all other lesions of structure, are disregarded, we have already shown by the citation of cases, (p. 713 — 715;) and shall farther show in the sequel. Whatever display of symptoms our author may have made, his reasoning, his pathology, and even his therapeutics, are founded upon the debris of the body, — we mean upon the glands of Peyer. Our author, therefore, must stand the ordeal by which he has tried and condemned the whole of his profession; for, we do not now advert to his summary mode of disposing of the past, or his admonitions to beware of "the daily facts" of his cotemporaries. (*See introductory extracts.*) As to the glands of Peyer, the consideration alone which our author has bestowed upon them is a sufficient index to all his medical opinions, and explains abundantly the principles in which the "numerical method" originated, and the therapeutical treatment which he has carried out to the gravest inflammations.

In accomplishing a demonstration of the foregoing propositions, we shall still prefer imposing the task upon our author himself; whilst in so doing we enable our author to carry on his work of generalization.

120. "I have considered it [the *alteration* of the elliptical patches] *as inseparable* from the disease we are now studying, and as *absolutely forming its anatomical characteristic.*" (3)

For a specification of the foregoing doctrine, and the complete

(1) The reader will soon find, that little or nothing of the cerebral symptoms are ascribed to the condition of the brain, and this, too, when that organ is affected in its structure, but that we must look to the lesion of the glands of Peyer for the cerebral phenomena.

(2) See Vol. ii. pp. 72, 132, 139, 145, 277, 318, 319, 320, 325, 335, 350, &c., &c.

(3) Vol. i. p 381.

dependence of the typhoid affection upon the lesion of the glands of Peyer, see our author's *forty-first* Observation, which we have quoted, in part, at page 714.

"We must not consider it [the lesion of Peyer's glands] merely as *peculiar* to typhus affections, but as forming their anatomical characteristic as much as *tubercles do that of phthisis*, (1) whatever may be the cause of their development." (2)

121, 122. "We must infer that it is in this last lesion [of the *glands of Peyer,*] and *not in any other*, that we must look for the *cause of the delirium*, and more especially of the *somnolency*," in typhoid fever.

"And, as the cerebral symptoms could not be attributed to any other cause, ['the lesions of the elliptical patches of the ileum,'] we must regard them as one of the CONSEQUENCES of it, as *in the other cases of the typhoid affection.*" (3)

123. "The condition of the brain, *in the actual state of the science of pathology, cannot explain* the cause of this symptom [delirium] *in fatal cases.*"

"All the facts seem to prove that the delirium of the typhoid affection cannot be explained by any appreciable alteration of the brain." — Or thus: — "We cannot explain the delirium by the apparent state of the brain."

We shall illustrate the foregoing hypothesis by a specific remark of our author, but of general import; and shall ultimately present an analysis of cases relating to the brain.

"It may be asked if the cerebral symptoms did not have in this case a special cause; if they were not *owing to the irregularity of consistence* of the *cephalic mass.* The *affirmative* of this question it would be difficult to sustain; for, if the *spasms* or convulsive movements had been the consequence of this *inequality of consistence*, they would have occurred on the side opposite to that in which the consistence was most marked, which was not the fact, — *at least at the earlier periods of the disease.*" "The nervous symptoms were so far superior to all others *as to mask them.*" (4) — So also in numerous places. This is morbid anatomy.

124. "It [the delirium] *more than any other symptom*, seems to be dependent upon the small intestine, in the typhoid affection." (5)

125. "It appears that its *treatment* [*delirium*] *ought not to differ* from that of the specific alteration of the small intestine." (6)

126. "We cannot refer the *spasms*, any more than the delirium, to the *state of the brain* except *secondarily;* so that *what has been said* with relation to *delirium* and other cerebral symptoms *must be applied* to the *spasms.*" (7)

We will now have an anatomical confirmation that the brain has little or no connection with its own symptoms, from which it will "conclusively follow" that they depend upon the lesion of the glands of Peyer. The matter is of course settled by a

(1) See Philip on Indigestion. (2) Vol. i. p. 196. (3) Vol. ii. pp. 132, 326.

(4) Vol. ii. pp. 142, 150, 333, 335, 448. At page 150, our author speaks of the connection of delirium with the fever; but, it will be seen that he refers the whole febrile action to the lesion of the glands of Peyer.

(5) Vol. ii. p. 448. (6) Vol. ii. p. 449. (7) Vol. ii. p. 451.

haggling about "colour," while the lesions of structure are quite unimportant when they conflict with the leading hypothesis. (P. 656.) Thus:—

127. "However, it may be asked, notwithstanding the facts above stated, if the CEREBRAL SYMPTOMS *may not be connected* with the APPRECIABLE ALTERATIONS of the brain, with the ROSE colour of the cortical substance, and the VERY PALE LILAC hue of the medullary substance which was observed. I shall repeat upon this point merely what I have already stated, viz., that there is a ROSE colour in the brain equally in those who have more or less severe cerebral symptoms, as well as in those in whom the intellectual faculties remain unchanged, in subjects who die of the typhoid affection and those who die of other diseases; that it arises VERY PROBABLY during the last days of life, and CONSEQUENTLY, it is IMPOSSIBLE to refer these symptoms to it. As to the WHITE and SOMEWHAT LILAC colour of the medullary substance, it is the *only case* of the kind which I have seen, and how can we explain any thing from a single fact?" (1) — Our author shall answer this question *practically* hereafter.

128. "It seems to me impossible, in the *actual state of science*, to find the cause of it [*delirium in pneumonia*] in any appreciable alteration of the *brain*." "We are obliged, therefore, in this disease, [pneumonia] as in the typhoid affection, to recur for an explanation of our difficulties *to the organ* which is always affected, and is sometimes the only organ affected, viz., the lungs, whose inflammation causes so many secondary lesions." (2)

We have quoted the foregoing generalization for the purpose of showing the more emphatically, that our author resorted even to a spurious analogy to establish his hypothesis that all the lesions, &c., of "the typhoid affection" were owing to an affection of the glands of Peyer. He assumes that the delirium in pneumonia cannot be owing to a "secondary lesion of the mucous membrane of the stomach or of either intestine," because "it was healthy in three of the cases;" that is, there was no apparent *disorganization*.

129. "There is hardly any need of observing, after all that we have previously seen, that if the *first symptoms* of the disease showed a lesion of the intestine, the gravest, and, without doubt, the most ancient one, was that of the elliptical patches in the ileum, and that there was an *exact proportion between the two*; and, under this point of view, the affection did not differ in any thing from the cases of it we have *previously studied*." (3)

"There was, in fact, an evident relation betwen the *symptoms* and *lesions*, and both indicated that *the seat* of the disease was, at its origin, *in the elliptical patches of the ileum*." (4) (*See our subsequent remarks as to the sources of our author's information.*)

"All the symptoms of typhoid fever arose successively and in their usual order." "These showed the *seat* of the disease at its commencement to be in the abdomen. The condition of the *elliptical patches* and of the mesenteric

(1) Vol. ii. p. 159.
(2) Vol. ii. p. 144.
(3) Vol. i. p. 148.
(4) Vol. ii. p. 350.

glands corresponding to them, showed, likewise, that *from them had arisen the symptoms.*" (1)

"The gravest lesions, the only ones which can be easily appreciated, being those of the alimentary canal, to these *alone* must be attributed the symptoms which have been referred to." (2) (*Gen.* 81, 82.)

130. After saying that, "I have considered it [the lesion of the glands of Peyer] *inseparable* from the disease we are now studying;" he adds, "the *other lesions* must be considered as merely accessory or consecutive." But mark:— "Still they commenced often *quite soon after the principal disease.*" (3)

131. "The *progress of which ulcerations* [of the vesicated *skin*] is *generally proportionate to that of the alteration* of the *elliptical patches* of the small intestine." (4)

132. "An increase in the size of the *spleen* takes place in nearly all the cases *in which the elliptical patches of Peyer are altered.*" (5)

133. "It is moreover remarkable, that, as the INFLAMMATORY condition of the elliptical patches had *retrograded*, so THE SYMPTOMS FOLLOWED AN ANALOGOUS COURSE *during the last days* of the existence of the patient." (6)

We scarcely need say that this was owing to a general physiological change which had no connection with the glands of Peyer.

Under the head of "*Diagnosis*" we have the following law:

134. "When more *or less* of these symptoms exist in the same subject, we cannot doubt that he is attacked with the typhoid affection, that the *elliptical patches of the ileum are the seat of the lesion* [the typhoid disease] which has been described." (7) (See pp. 712, 715, and Gen. 8.)

The following generalizations show that our author ascribed the whole febrile action to the structural lesion of the glands of Peyer. Thus:

135. "We must allow that the *febrile excitement* which was observed in the *typhoid affection was as much proportioned* to the state of the *small intestine*, as that which occurs in erysipelas of the face is to the *extent* of the skin *inflamed.*" (8)

136. "The FUNDAMENTAL alteration, that of the *elliptical patches* of the small intestine, was more extensive according as the patients died more rapidly, and the febrile action was generally in proportion to it." (9)

"*The intensity of the febrile action is itself in proportion to the extent and seriousness of the* SPECIFIC *lesion of the small intestine.*" (10)

137. "Between the symptoms and lesions, of which we are now treating, [glands of Peyer,] the relation seems to me to be *not less evident* than that which is observed between *those two orders of facts*, as they take place *in other* affections; *pneumonia*, for example," &c. (11)

"But must we suppose this action of the *lungs* and of the *elliptical patches* of the small intestine to be the effect merely of a sympathy, which we cannot appreciate, *in their capabilities of producing disease, or as the* consequence of *a febrile excitement of which the inflammation of these organs was the* SOURCE. This last supposition seems to me to be the most probable." (12)

(1) Vol. ii. p. 72. (2) Vol. ii. p. 334. (3) Vol. i. p. 381. (4) Vol. ii. p. 436.
(5) Vol. ii. p. 317. (6) Vol. i. p. 371. (7) Vol. ii. p. 266. (8) Vol. ii. p. 394.
(9) Vol. ii. p. 240. (10) Vol. ii. p. 444. (11) Vol. i. p. 55. (12) Vol. ii. p. 144.

"Their great importance, [the facts previously given,] as it seems to me, consists in this, that they ought to excite *doubts* in our minds, *as already stated*, in relation to *all febrile affections without a determinate seat*, and which are called *general*, and thus put us in the way of proposing problems which it is of great importance for us to solve." Again: — "The number of these febrile affections without any determinate seat or *local lesion* is becoming every day more limited." (1)

And then follows in a note a reference to one of those "*isolated facts*," which it is our purpose to notice soon; and about which our author hesitates whether he "shall *prejudge* by it the important question of the *nature of fevers*." The fact relates to our author's theory of the dependence of "mild cases of continued fevers" upon *ulcerations* of the small intestine, and of which fact he would predicate the hypothesis. In the meantime, let us add a remark of our author, which his translator observes in a note, "*has been thought to be obscure. Some have thought that he meant to allow that intermittent fevers were entirely similar in their nature to the typhoid affection.*" For our part, we consider it rather more luminous than many of the conflicting subtleties which we have hitherto exhibited. It is the language of a morbid anatomist without having before him, at the moment, any hypothetical problems.

"Thus the morbid changes in organs and functions," says our author, "observed during the course of *continued* fevers were observed, *likewise*, in those which were *intermittent*. *The difference consisted in hardly any thing more than degree.*" (2)

This is plainly an unprejudiced conclusion, is full and to the point, divests our author's cases of all their factitious importance, and settles the value of morbid anatomy. (See *Gen.* 5, p. 700, and p. 710.)

"This case, (Obs. 48,) is certainly one of that *small number* which have induced many persons who are attentive and impartial in their opinion, *even to believe in these latter times, in the essential nature of fever.* In fact, at the first glance, the patches of the small intestine appeared *healthy*, &c.; we might neglect a lesion which seems to be merely a change of *colour*. (P. 748.) But, when once

[1] Vol. ii. pp. 318, 319. "It is well to remark," says our author, when speaking of intermittent fever, "that *rather frequently* there were in the same subject many *symptoms* announcing derangement of many functions, &c.; and this is a new reason why we should not place the seat of the disease, sometimes in one part, at others in another, and why we should not consider as accessory or consecutive symptoms those which we have in succession described." (*a*) This is intended, as an argument, by way of contradistinction, in behalf of the localization of "the typhoid affection" in the glands of Peyer, where even structural lesions of organs were various, but of rare occurrence in intermittents. The inconsistency is obvious.

(2) Vol. ii. p. 260.

(*a*) Vol. ii. p. 261.

these different transformations have become known, we cannot do this, and we *cannot doubt* that the *symptoms were in this case, as in the others, connected with a* PECULIAR *alteration of the elliptical patches of the ileum.*"

The fever had its origin, and continuance, in the alteration of the glands of Peyer, even when that alteration was "latent." This refers to a particular case,—

"Perhaps the reader will ask, if these febrile symptoms, without diarrhœa, or pains in the abdomen, observed during the first ten days, were connected with the commencing alterations in the elliptical patches of the ileum. The affirmative to this question will not appear doubtful, when we remember that the most severe lesions were those of the small intestine," &c.

The reader will not have forgotten that our author has abolished all former experience upon the ground of its consisting in "*a priori* reasoning." Nevertheless, our author having *assumed* the important principle involved in the foregoing extract, proceeds to argue from that solitary case, (an ivory-turner,) where the patient had suffered the disease for "*three weeks*" before he came under any medical observation, and who had "*continued to work during the first fifteen days,*"—we say he proceeded to deduce a law, important in its consequences, from that miserable victim of numerous structural lesions of the intestines, of the liver, spleen, (1) &c.—Thus:—

138. "If, then, FROM THE COMMENCEMENT of the affection *until its fatal termination,* ALL the morbid phenomena *were dependent upon,* or connected with THIS SAME lesion, it results that this *can remain* LATENT, during a considerable length of time, or at least *give rise to no characteristic symptom.*" (2)

Nearly a page is devoted to a minute detail of symptoms which took place antecedently to the arrival of the patient at La Charité, upon which the whole conclusion turns, and an account of which were obtained from the ivory-turner himself.

It is also important to our present purpose to say, that the foregoing statement proves that our author not only refers "ALL the morbid phenomena" to the lesion of the glands of Peyer, but that no other lesion has any influence upon them. This will appear more fully in the sequel.

139. "At this period, in fact, [the commencement,] the *elliptical patches* were, if not in all, at least in *nearly all* the cases, the *only part* of the canal which was diseased, and consequently, the *only part* to which the *diarrhœa* could be referred." (3) (*See our tabular view of the cases, for the periods at which the patients entered the hospital.*)

(1) "The spleen was three times its usual size." It will be recollected that our author locates the intermittent fever in the spleen.

(2) Vol. i. p. 33.

(3) Vol. ii. p 18.

It is scarcely necessary to call the attention of the reader to the proof which our author constantly affords of his conviction that all pathology consists in *lesions* of structure, and that that lesion constitutes the pathology, in all affections, which happens to be most prominent or most frequently presented. But, we have another object in this commentary, which is that of remarking that our author actually conveys the impression that the "diarrhœa" proceeded from the glands of Peyer. We cannot, however, suppose that such was our author's belief, though it is far from certain. Others, however, will be apt to think it proceeded from the intestinal mucous membrane, even though it be allowed that it was not disorganized. How, then, we ask, does it appear that the morbid action of this membrane, and consequently the diarrhœa, depended upon the lesion of the glands of Peyer, or that the condition of the glands was not secondary to the morbid state of the mucous membrane? (See page 712, *last citation.*)

In Obs. 40, "there was almost complete destruction of the mucous membrane of the colon throughout nearly the whole extent of it." "It is sufficient to consider the condition of the different organs to be satisfied, that the *large intestine* was the *original seat* of the affection."

This is a solitary case of fever devoted specially to the "*Diagnosis*," in which an embarrassing lesion of the large intestine, and the absence of any lesion of the glands of Peyer, militated against the location of the typhoid fever in the latter organs. It shows us, however, how intent was our author upon locating the disease in the intestines; and nothing saves him in this instance from advocating the doctrine of Broussais, but the assumption that the structural lesions in "the typhoid affection," are either non-inflammatory, or "owing to a cause exactly opposite to inflammation."

140. "A fact worthy of notice is this, that the *hard patches* were much more frequently found, cæteris paribus, in patients who died between the eighth and fifteenth days, than in those who died afterwards." Thus, "numerically:"

6 out of 10 patients of the first series.
2 " 7 " " second "
5 " 20 " " third "

Do these proportions depend upon any LAW, or must we suppose them to be dependent upon CHANCE? The *former* supposition seems to me the *most probable*." (1)

Near this place we have the following episode:—

"In what light must we view, therefore, the *long commentaries* which many authors make upon the isolated facts which they publish, and this without any

(1) Vol. i. p. 181.

knowledge of the general LAWS connected with them? And what good can result from these vain demonstrations of *profound learning* and *sagacity?*" (1)

We have already quoted certain cases of our author, (p. 713—715,) to show his proof of the contra-inflammatory nature of the structural lesions which distinguish "the typhoid affection;" where, also, it will be seen that the whole condition of the disease was referred to the mysterious lesion of the glands of Peyer. —Again, in Obs. 49:

"*Cerebrum moderately consistent*, slightly injected. *Corpus striatum* of the right side yielded to pressure like rather *elastic paste*, as it were. Such was, likewise, the case with the *annular protuberance*." "Mucous membrane of the *stomach* was of a *bright red colour* in its superior extremity. For the space of two inches near the pylorus it had *many partial ulcerations*, from six to ten lines large. In the two last feet of the *small intestine* were 12 elliptical patches, more or less prominent, each of which presented in its centre an *ulceration*, by which the *muscular fibres* had been exposed and remained a little *thickened*. The mucous membrane was more or less *softened* in their intervals. The *mesenteric glands*, *corresponding* to the ulcerations, were a *little red*, and *enlarged*. The mucous membrane of the large intestine was a little *softened throughout;* of a *dark red* colour in a small part of the right colon, of a *bright red in the rectum to within 2 inches of the anus*. The *mesocolic glands* were somewhat *red* and *enlarged*. The *liver* was of a *deep red* colour, *more consistent* than usual. The *spleen* was firm, of a *dark red* hue, and but slightly enlarged."

The foregoing are farther examples of the contra-inflammatory nature of the various lesions in which they consist. (See pp. 700—702, &c.) This case is set down among the doubtful ones; but our author verifies it as a case of "the typhoid affection." Nevertheless, this will in no respect affect the principles about which we are concerned. Our author says of the case,—

"There was, in fact, an evident relation between the symptoms and lesions; and *both indicated that the seat of the disease* was, at its origin, *in the elliptical patches* of the ileum."

The reader will do well to turn to this case for the proof of the foregoing affirmation; and to consult other cases with the same reference. But, we are now most interested about the *brain*, and the advantages of our author's mordid anatomy. Our author continues:—

"But was the condition of the brain of no influence in the production of certain *serious* symptoms, *delirium*, *subsultus tendinum*, and the *profound indifference* of the patient. It is *impossible*, as it seems to me, to answer this question either *affirmatively* or negatively. For, on the one hand, we observe these *same symptoms*, in a much *more remarkable degree* in a great number of cases of the typhoid affection, in which the *brain does not present* ANY APPRECIABLE LESION; and on the other, we are *ignorant* of the acute or chronic course of this

(1) Vol. i. p. 215.

ELASTIC SOFTENING." "If we must come to some decision upon the point in debate, we ought, on account of all these considerations, to regard *this softening* as of very recent date, and the *symptoms* anterior to the last days of life AS BEING ENTIRELY INDEPENDENT OF IT." (1)

This patient was ill 36 days, of which he spent 30 in the hospital. Delirium began 25 days before his death. Here, too, we may notice one of the numerous and forcible proofs that our author has no conception of disease which is not denoted by some lesion of structure after death; and if such do not exist in the brain to account for any violent cerebral symptoms, they are referred in all cases to the lesion of some other part which is most frequently present in any given disease. Hence, therefore, our author ascribes them to the lesion of the glands of Peyer in "the typhoid affection," and "to the lungs in pneumonia." The reader, too, will have remarked the extraordinary use which our author has made of *analogy* in the foregoing case; rejecting the "*pasty* state of the corpus striatum and annular protuberance" as the cause of the cerebral symptoms, because the same, or other lesions of structure are not found in other cases. What say Rostan, Abercrombie, &c., to this conclusion; and may we not appeal to mania, epilepsy, and the whole genus of "nervous" affections for a reply? Do we not constantly meet with violent delirium attendant on idiopathic fever, or arising from sympathetic influences exerted by every remote organ, where there is no vestige of cerebral vascularity? And who will doubt that all the cerebral phenomena were immediate emanations from functional disturbance of the brain? Is it not an absurdity to entertain the question? Examples are common where slight irritations of the surface occasion absolute frenzy; but, the moment the offending cause is removed, the most entire tranquillity may be restored. (Vol. I. p. 425, *case*.) The vital powers of the brain may be disturbed without even much functional derangement. They may be extinguished in an instant, as from a blow on the stomach, &c. (Vol. I. p. 424.) And although the delirium, the spasms, &c., which are constituted by absolute, but inappreciable, change of function, by inflammation, or disorganization, of the brain, or by a momentary lesion of the cerebral

(1) Here our author has a note, saying, "we cannot too frequently repeat, that medicine will *never make any progress* until all minds will be convinced that we must collect and compare all the facts upon a subject, whatever they may be."— *Vol. ii. p.* 351.

powers, be nearly analogous, the pathology being different, they may require very different modes of treatment. He, who sees only *the* symptom, or thinks it "unimportant, in respect to effects, to inquire into the *cause* of trouble in our functions, when this trouble is of a *serious* nature," will be likely to "confound them." The distinctions, however, are pointed out by the whole assemblage of the morbid phenomena, associated with their remote causes, constitution, sex, &c. They can never be explained by the anatomist. Nor should it be forgotten, that in nearly all our author's fifty cases, there was either *injection*, *softening*, *an accumulation of serum*, or some other unnatural appearance in the brain.

"The pia mater was injected in a little less than half the cases;" "the medullary substance of the cerebrum was more or less injected in all the cases excepting seven;" and the cerebellum was only "more frequently in a *healthy* state than the cerebrum, and when it was *not so*, it generally was affected in a *similar manner* and in the same cases." (1)

And when we consider farther, that the violence of delirium, somnolency, and spasms, was such "upon the manifestations of those [symptoms] dependent on the severest lesions, that they *masked* them in nearly all the cases," (2) we have, in connection with our preceding inquiry, a fair exemplification of the respect which is paid by the anatomical school to the vital signs; and, may we not add, even to the physical. (See Gen. 80.) And even yet more: a fatal blow is attempted against the analogy which is supplied not only by the cerebral symptoms, but by the tenderness of the abdomen, and all the never failing abdominal pains, (which were the earliest symptoms,) (3) and the various structural lesions of the brain, of the intestines, liver, spleen, Peyer's glands, &c., which are declared to be either independent of inflammation, or to depend upon something "directly opposite to inflammation." (4) Nor is this all: for, when we regard, in connection with the foregoing facts, that other of which we have just spoken specifically, where structural lesions of the brain are considered, along with vascular injection, effusion, &c., inadequate to explain the cerebral symptoms, that the glands of Peyer "may be all in all," we can better appreciate that quibbling (5) about the non-inflammatory nature of the softening,

(1) Vol. i. pp. 326, 327, 333. (2) Vol. ii. p. 169.

(3) Vol. ii. pp. 10, 18, 72, 142, 144, 265, &c.

(4) Vol. i. pp. 170, 171, 226, 228, 254, 255, 260, 261, 284, 288, 294, 301, 344, 350, 351, &c. See, also, work on Phthisis, sec. 144, &c.

(5) Compare vol. i. pp. 155, 156, 157, 202, 203, 12, 159, 223, 260, 163, 324, 363,

thickening, ulceration, and perforation of the intestines, in "the typhoid affection." It was not alone, however, in respect to the typhoid affection, that cerebral, and other symptoms, and morbid anatomy itself, were constantly disregarded. In another work we read of

"An old-clothes-seller," in whom were found "*several lacerations* of the dura mater giving passage to *granulations* springing from its laminæ. Slight *infiltration* of the sub-arachnoidean tissue. Brain, pale, moist, and of about the *consistence* of that of the *fœtus of six or eight months*. A spoonful and a half of clear fluid in each lateral ventricle, &c. The *tuber annulare* and the *cerebellum* were nearly as soft as the *cerebrum*." (Vol. I. p. 363, *case*.) There were numerous structural lesions in other organs, such as a "vast excavation," extensive tuberculous formations in the lungs, ulcerations in the stomach and intestines, 200 calculi in the gall bladder, &c.

The death of this patient was "sudden:" and yet our author says, that, —

"Here, as in the two preceding observations, the *cause* of the *sudden death remains unexplained*. We *shall not attempt* to attribute it to the *softened condition of the brain;* [nor *lacerations?*] *for we are quite ignorant what degree of firmness is necessary for the support of life"!* (See Gen. 80.) And again our author has it, "the condition of the brain was not the only *remarkable* circumstance in the fact before us." "It remains," says our leading pathological anatomist, "to be shown when a *general* softening of this organ, *always easily recognised* by those accustomed to pathological researches, may be regarded *as a morbid condition*." (1)

In the next following case, (Obs. 50,) "the brain was *very soft throughout its whole extent*," &c. And here as in the three foregoing cases, our author is "without any desire to assign any cause for the sudden death of this patient." Nevertheless, "*this great deviation* from the *normal* state [of the brain] appears to us an evident *morbid* condition." It may be important, therefore, to look for more satisfactory causes, and to allow our author the advantage of his implied philosophy, as in the case of "meteorism," by stating the fact, that, "as was the case in the majority of the instances of *sudden death*, there were VIBICES on the extremities." (2) In the 42d case of the typhoid affection, already related (p. 713,) where the structural lesions were vast, including "two holes in the ileum," our author inquires, "to what shall *we* attribute the sudden and *unexpected* death of the patient?"

We shall now offer a most important generalization, which strikes a fatal blow at all pathology and at all therapeutics; whilst

168, 170, 171 — 184, 272, 198, 167, 234, 161, 162, 292, 379, 190, 193, 201, 226, 228; and vol. ii. pp. 280, 288, 294, 301, *etc.*; and, on Phthisis, sec. 135, 136, 144, &c.

(1) On Phthisis, Obs. 49. (2) On Phthisis, p. 435.

it is manifestly designed for no other purpose than to carry out the hypothesis that the vast intestinal lesions in "the typhoid affection" are of a "specific nature," and not dependent on inflammation. Thus: —

141. "We are led to believe that the mucous membrane of the intestine is but little altered in ENTERITIS, but that *it is in a condition which differs perhaps, but little from that of the* SKIN WHEN IT IS THE SEAT OF COPIOUS PERSPIRATION." (1)

Taking this generalization by itself, we should say that it lays open the whole ground of our author's objection to bloodletting and blisters in all great internal inflammations; for who would bleed or blister to arrest a "copious perspiration." No instance of inflammation could have been adduced which demands the abstraction of blood more imperatively than what is called by our author "*proper enteritis*"; (2) the importance of which is forcibly represented by Dr. Elliotson in our first volume, page 301. And may we not affirm nearly the same of our author's fatal cases of muco-enteritis? If the reader will now refer to our page 695, and there observe what our author says upon "analogy" in relation to this very subject, he may be still more inclined to agree in our conclusions. Are we then, unjust to our author, or to the school of morbid anatomy? We refer, indeed, more to the latter than to the former; and we dwell upon the subject especially with a view to sustain our conclusions in the preceding essays. But, to divest the foregoing generalization of its practical tendencies, and to illustrate still farther the true spirit of our author's pathology, we will here give its antidote as supplied by our author, viz: —

"We may *also* observe, that *facts* are wanting to prove distinctly that DIARRHŒA may exist without APPRECIABLE LESION of the *intestinal* mucous membrane." (3) (See, also, Gen. 7, p. 700 and p. 712.)

And now let us hear "pathological anatomy" as it prevailed at an era which is considered a dark age by our author and his school.

"Can any thing," says Nugent, "that has occurred in dissection, determine whether inflammation, or dryness, or whatever else may appear, were ORIGINALLY connected with the hydrophobia, or were only new symptoms generated in the course of the malady? What, in general can be more different than the *cause* of the *distemper*, and that of the *death*, of the patient? And yet, are not *these*

(1) Vol. ii. p. 231. — It will have been observed that we have frequently placed our author's language in a conspicuous view. We have done this from its having been apparently overlooked.

(2) Vol. ii. p. 272. — This is what is here meant by our author, to contradistinguish it from the typhoid affection. (See our p. 709.)

(3) On Phthisis, Sec. 259.

two causes frequently confounded together, and mistaken one for the other? Of this, let *one* instance *serve* instead of a *thousand.* Stones, passing the ureters, bring on spasmodic strictures. These, if not relieved in time, bring on an inflammation; this a mortification, and the mortification, death. Here, on opening the body, the *mortification* presents itself, and appears to have caused the death of the patient; and so it has, — though, in reality, it was neither the cause, nor any part of the cause, of his *distemper.*" (1)

"*Sunt aliquot quoque res, quarum unam dicere causam*
Non satis est, verum plures, unde una tamen sit." (2)

And here may we not again stop to inquire, whither is morbid anatomy conducting us? May we not pause for a moment, to consider, as Austin has it, whether we stand upon our heads or our heels?

"*Qualis in oculis hominum, qui inversis pedibus ambulat, talis in oculis sapientium.*" (3)

Finally, one case more, as a fair example of the philosophy of "the typhoid affection." It is Observation 5.

"An unmarried female had been sick two weeks, when she was admitted to the hospital La Charité. At the beginning, headache, violent chills, followed by heat, depression of strength," &c. Diarrhœa had come on. She grew progressively worse and worse, till taken from her suffering, which was on the 25th day of her disease. *Dissection.* — "*Stomach so softened* in the great cul-de-sac, that we could not raise any strips of it. Many *reddish* bands. Throughout the whole length of *small intestine* a rather large number of *venous ramifications* were seen, *distended* with blood; along which its *mucous membrane* was *red.* (See *Venous Congestion*, p. 519—523.) This membrane, near the cœcum, for the space of 3 or 4 feet was *softened;* had many *reddish patches; ulcerated*, of a *grayish* and *yellowish colour;* the last patches *destroyed.* Other *ulcerations* much smaller, appearing as if they had been made with a *gouge. Mesenteric glands*, corresponding to the *ulcerations* of the ileum, were *very much enlarged* and *softened.*"

Referring back to the violent symptoms which began with the disease, we have the following explanation of their cause; —

"These lesions, moreover, seem to have pursued the same march, which we have seen them follow previously, &c. The *ileum* must be considered as having been the principal seat of the disease, and *source* of the *principal* and first *changes in the system* of our patient. We could not, assuredly, attribute them to the mucous membrane of the *large* intestine, which, *with the exception of some ulcerations*, was well, nor to that of the *stomach*, &c. It is not certain that the *lesion*, of which it was the seat, had commenced even at the *twelfth* day of the disease; as the *symptoms experienced at that period could be attributed to lumbrici*, which were vomited at that time and afterwards." (P. 722, *note.*)

Of the last conclusion we shall say nothing, except to remark

(1) Nugent, On Hydrophobia, p. 54, 1753. (2) Lucretius, l. 6.
(3) De Civit. Dei, l. 14, c. 9.

that it is a new feature in morbid anatomy, considering the other facts which were associated with the worms; and, also, that the conclusion appears to be in collision with the universal doctrine as to the dependence of the symptoms upon the glands of Peyer.

Having now seen that our author builds his whole fabric upon lesions of structure, and mainly upon that of the glands of Peyer, in "the typhoid affection," it is proper to say that in a rare instance, where a difficult point is to be resolved, our author, after premising, —

"*Of what use is it to seek for the cause of dyspnœa*, if it be sufficient to interrupt respiration for some time," propounds another quere; "may not the changes in the cerebral functions, whether the brain be altered or not, in the course of this disease, explain sufficiently why death takes place *under certain circumstances*?" (1)

This, however, is no part of our author's doctrine, but appears to be an offering that may be advantageous "under certain circumstances;" especially should one be inclined "to seek for the *cause* of dyspnœa." Let us, however, place the subject in its most intelligible light.

Setting aside the abdominal pains, vomiting, &c., the former of which are said to have been almost invariable from the beginning, and the latter a common attendant, (2) — setting them aside as utterly worthless, — let us see how it stands with *morbid anatomy*. Our author, then, informs us, that, —

"The mucous membrane of the stomach was, in the *greater proportion* of cases, more or less *seriously altered*, sometimes softened and thinned, or even *destroyed*." (3)

A preliminary derision of "that group of symptoms designated by the term 'gastric embarrassment,'" (4) prepares us not only for the contradiction which is involved, but for the whole induction, in the following sentence: —

142. "So that, it is nearly correct to state, that the *apparent condition of the brain cannot explain the symptoms* of which it is the source, any more than the *mucous membrane of the stomach can account* for the anorexia and other gastric symptoms in the great majority of cases."

Turning to the next page we find what has been already stated, that, —

"We must infer that it is in this last lesion, [the glands of Peyer,] *and not in any other*, that we must look for the *cause* of the delirium, and more especially of the somnolency." (5)

And then follows a doctrine which we have already quoted,

(1) Vol. i. p. 374. (2) Vol. ii. pp 27, 28, 38, 39, 42, &c. (3) Vol. i. p. 152.
(4) Vol. ii. pp. 44, 45. (5) Vol. ii. pp. 131, 132. — We repeat the last quotation only as explanatory. See *Gen.* 121.

but which must be here introduced in its intended connection with the foregoing subject, as we have seen of that when speaking of "dyspnœa," and to which we have recently adverted. Thus, —

"What importance is it, however, with respect to *effects*, for us to know what is the *cause* of trouble in our functions, when this trouble is of a serious nature?" (1)

It is abundantly manifest from the connection in which the last doctrine stands, that our author intended to convey a strong impression, that it is neither important to consider the condition of the brain, nor that it had any special connection with "*the symptoms of which it is the source.*" If this be not intelligible, the fault is not with us; nor will we neglect saying, that we have never failed to observe, that the more "serious the trouble," the more important is it to inquire into its causes.

In Obs. 46, the subject had been ill seven days. Entered La Charité, February 3d.

"At the commencement, headache, giddiness, pains in the limbs, chills, heat, &c. During the first three days in March, [a month before,] the patient had *delirium* during the night. February 4th, face purplish; motions difficult, headache, great thirst, anorexia, abdomen meteorized painful, *constipation*. Pulse regular, rather small, at ninety-two. From time to time patient uttered cries, or was constantly muttering to herself. This disposition to talk could be restrained only momentarily by menaces, (v. s. ʒ viii; tamarind whey; lemonade twice; fomentations upon abdomen; emollient enema.)

The night was restless. On the 5th, answers rather less promptly than before; features natural; tongue moist, not red; continuance of pains in the abdomen; frequent sighing; ONE *rose coloured* lenticular spot upon abdomen. (Blisters to legs.)

The patient had intense headache; intellectual faculties pretty good; *delirium* very noisy, more so *than usual* during the night. On the 6th, face rather purplish; prostration; *constant cries*, which did not cease until some time after the visit, when they ceased apparently from exhaustion of the patient; many rose coloured lenticular spots upon the abdomen; other symptoms as on the previous day.

(Lemonade; infusion of cinchona [!] aromatic fomentations; enema of camphorated camomile tea; blister to the back of the neck.)

Some *involuntary dejections* during the day; [previous *constipation*, but no cathartic,] a little less delirium during the night. On the 7th, face, *pale as after a great loss of blood;* speech, almost unintelligible; tongue dry and frequently protruded with difficulty; drank only about a third part of the bitter infusion. (Frictions with aromatic wine; potion with peppermint water ʒii.; orange flower water ʒii., and sulphate of quinine grs. xx. [!])

Until the 15th, the day on which death took place, the symptoms became gradually more severe; delirium and drowsiness were constant; patient did not cease from uttering loud cries during the night of the 7th, to the 12th; for a *very* short time only was she a little calm on the 9th, whilst in a bath; great

(1) Vol. ii. p. 145.

stiffness of the neck and left arm on the 11th; the upper and lower extremities were so, very nearly all the time from the 12th to 15th; tongue, usually dry, was yellow and moist on the 10th; two or three involuntary dejections during the day. Pulse rather large, and at 100 on the 8th; it became more quick afterwards; and, on the 12th, an excoriation of the sacrum was observed.

On the morning of the 15th, the head was inclined to the left side; there was a general tetanic rigidity, which continued until death, at 9 P. M. [on the 19th day.]

Examination of body THIRTY-FIVE *hours after death.*

Head. — Some granulations at the posterior part of the *arachnoid*, near falx. [Brain otherwise natural.]

Spine. — A spoonful and a half of serous fluid at the bottom of the spinal canal; spinal marrow, in a natural condition throughout its whole length.

Neck. — The cellular membrane surrounding the *Pharynx*, save a small portion of the right side of it, was infiltrated with pus, and this infiltration, which was dry, as it were, extended into the upper part of the anterior mediastinum, where there was seen nothing more than a turbid, serous fluid. The *muscles of the pharynx*, on the contrary, were infiltrated on the right side only, where they were from a line and a half to two lines thick. The *epiglottis*, *larynx*, and *trachea*, were natural.

Chest. — [Nothing of any moment was found here.]

Abdomen. — *Œsophagus*, healthy. *Stomach*, small; its mucous membrane grayish and spotted *red almost uniformly*, save along the small curvature for the space of an inch and a half; it was mamelonated along the large curvature over a space three times as large; of natural consistence throughout its whole extent, except in the great cul-de-sac, where this consistence seemed increased. The small intestine contained a moderate quantity of mucus. Its lining membrane was *whitish* and *grayish* in some points only; of its usual consistence and thickness, save for a space of two feet in length in two parts where the cellular membrane was infiltrated. In its last fifth, there was rather a large number of *elliptical patches, only a little thicker than natural*, covered with *gray* points, without *redness*, and presenting some small *ulcerations*, either solitary or clustered together, about three or four on each patch, where the *muscular coat was exposed.* The *first* of these patches, contrary to what *usually happens*, was the largest, was three inches long, and had upon it the largest ulceration, five lines in diameter, and the borders of this last were separated from the subjacent parts for the space of a line. Mucous membrane of the large intestine was a little *red* and softened about the rectum; it was of a natural colour and consistence throughout the rest of its extent, except in the cœcum, where the membrane broke rather easily. The *mesenteric glands* were *very red, very much softened*, about the size of filberts, and in those near the cœcum there was a great number of *yellowish points.* *Liver*, healthy; *bile* of a *reddish* hue and very fluid, not copious. The *spleen* had twice its usual size, and was of a good consistence. Cortical substance of the *kidneys* was *redder* than usual."

We have had various motives for quoting the foregoing case, in partial detail. It is one of the four of our author's "*cases of the typhoid affection in which the* ANATOMICAL CHARACTERS *may at first seem doubtful;*" the structural lesions being generally

far greater. To our mind, however, it appears to be the most probable case of an idiopathic fever, complicated with local congestions and inflammations, in the whole assemblage. It is, also, one which represents the most active treatment pursued by our author, and the philosophy of that treatment. It is a fair representation of his diagnosis from the vital signs; and it exemplifies, as we shall soon see, the advantages which morbid anatomy, as cultivated by our author, is bestowing upon pathology and therapeutics. The case should be studied, in connection with others which we have quoted, as examples of the rest, by the student, by the mechanical, and by the philosophical practitioner.

We shall not follow our author in his speculations upon the foregoing case. In the spirit of that morbid anatomy which we have described both here and in another essay, he remarks that, "*the disproportion existing between the symptoms and lesions was certainly considerable;*" and in the same spirit he concludes that, —

"As to the *causes* of death, if we find them partly in the inflammation of *the cellular membrane about the pharynx* [!] it is well particularly to seek for them in the derangement of the cerebral functions, for the reasons given above, (page 145.)"

This is all that we have as to the cause of death, whilst in respect to the brain it is said,

"As the cerebral symptoms could not be attributed to any other cause, [than the lesion of the elliptical patches of the ileum,] we must regard them as one of the consequences of it, as in the other cases of the typhoid affection."

The importance which is ascribed in the foregoing case to "the inflammation of the cellular membrane about the *pharynx*," should be contrasted with our author's constant exclusion even of the structural lesions of the brain, as well as with such analogous illustrations as occur in Obs. 7th. Thus: —

"The *destruction* of the *epiglottis* which, as we shall see hereafter, is one of the most remarkable secondary lesions of the typhoid affection, (Gen. 37, p. 723,) caused no peculiar symptom, and, without doubt because of the reasons I have just enumerated, why the *false membrane* of the PHARYNX likewise *did not cause symptoms.*" (1)

We have now seen that our author's pathology is apparently at oppposite extremes in respect to the same vital signs, and the same lesions of structure. They are sometimes allowed to depend upon inflammation, but in the main are owing to a condition exactly the reverse; whilst, either for the purpose of pro-

(1) Vol. i. p. 54.

viding for the contingency, in other hands, where no structural lesions may occur, or to reconcile the reader to the anatomical doctrine, the possibility of what has been considered *functional* disease is reluctantly tolerated. Nevertheless, in respect to "the typhoid affection," the structural lesion of the glands of Peyer is the focal point of the whole circle of reasoning. Our author, indeed, is so engrossed with the debris of the body, that he gives us the poor consolation, that it is probable that we have never an attack of diarrhœa, without suffering more or less *disorganization* of the intestinal mucous membrane; (1) however this may be contradicted by Gen. 141, at p. 764.

Thus might we go on, page after page, showing how intent was our author upon establishing the typhoid fever upon the ruins of the glands of Peyer. There is no place, even, where he *contradicts* this paramount doctrine. In numerous instances, some of which we have recited, he enters upon its analytical proof. Our author's only deviation from his perpetual object, is an occasional admission that the *febrile action* becomes *one* of the causes of the consecutive lesions. But, the *fever* itself depends exclusively upon the lesion of the glands of Peyer. Nevertheless, it may sometimes appear, from an occasional display of hesitation, that our author had some doubts as to the agency of the glands of Peyer; but this relates only to their principal instrumentality in producing death. Here is the strongest example with which we have met, whilst it is a flat contradiction of what he has generally said of the agency of the stomach. (See p. 700, &c.) Thus: —

"If this observation is incomplete in regard to symptoms, there is little left by it to be desired in regard to post mortem appearances, and we need only mention the state of the different organs to be satisfied that the condition of the small intestine could hardly account for the death of the patient. THE ELLIPTICAL PATCHES, it is true, WERE ALL MORE OR LESS INFLAMED, the isolated glands numerous and enlarged in the last four feet of the small intestine; still these lesions were slight and but little advanced; there were no ulcerations, and the mucous membrane between the patches was not diseased save in a few points. The lesions of the *mucous membrane of the stomach*, which was more or less thinned, softened, or destroyed in some parts, *could alone explain the reason of death taking place*, or at least must be regarded as the principal cause of it; other lesions, *especially the meteorism*, [a lesion?] having concurred more or less to its production."(2)

It should be now observed, especially as it concerns the important subject of "*red*" and "*white*" ulceration, softening, thick-

(1) On Phthisis, s. 259, *et passim*. (2) Typhoid Fever, vol. i. p. 362.

ening, &c., that our author was one of the first to create, and has contributed most to, the confusion which prevails in respect to the physical products of inflammation. Other morbid anatomists, it is true, have contemplated different objects from that of our author. Inflammation is the only condition of disease, or certainly with rare exceptions, which gives rise to lesions of structure, and other physical results; and had it not, therefore, been assumed, that the same products are owing to various physiological conditions, morbid anatomy would soon have exhausted its resources, and the medical public would have palled with the repetitions of an unvarying philosophy, deduced from cadaverous lesions.

We have sufficiently demonstrated not only our author's philosophy as to the pathology of disease, but, that it is nearly coextensive with the school of morbid anatomy, viz. that of localizing all diseases and imputing their essence to lesions of structure. We shall here introduce, however, a passage relating to the subject, since it embraces an important, but fallacious doctrine: —

143. It probably never happens that individuals, who die of a disease the seat of which is well determined, are free from lesions *in other organs* besides that primitively affected;" meaning structural lesions. (1)

In adverting to the secondary lesions, we are here reminded that we neglected to state an important generalization in its appropriate place. (P. 726, *Gen.* 53.)

144. "With one exception, (the case of puerperal metritis,) I have never met with peritonitis *in the course of acute diseases*, but as a CONSEQUENCE OF PERFORATION OF THE SMALL INTESTINE in subjects affected with the typhoid disease. A peritonitis developed in the course of this disease indicates, THEN, ALMOST CERTAINLY, the existence of *perforation, independently of the violence of its commencement.*" (2)

It may be thought that the foregoing generalization does not vary essentially from that of 53; but it is clearly constructed upon a specific ground, and excludes the combination of symptoms which are required by the other.

Having thus departed, again, from our systematic course, and being justified by our author in this particular, we will recall to our reader (3) the reprobation which our author bestows upon "*a priori* reasoning," and how universally he has expunged all former facts and experience upon this principle. In approaching the following "law," we must request the reader to revert to pages 695 — 699 for our author's opinion of analogy, and especially for

(1) Vol. i. p. 356.

(2) Vol. ii. p. 381.

(3) "But now to gather up what seems dispersed,
And touch the subject I designed at first,
May prove, though much beside the rules of art,
Best for the public, and my wisest part."

his views as to reasoning from "*the man in health to the man in disease.*" At a subsequent time, our author being concerned about his fundamental basis of the pathology of common structural lesions, viz., the different hues of *colour*, we find him saying, that,—

145. "Perhaps it will be said that this GRAY *colour* was the consequence of some old inflammation. But, this would be an assertion *without proof*, and we cannot believe in it, until it shall be demonstrated by *a long series of facts*, that the *gray* colour *which succeeds* the *red* colour of inflamed parts, can remain a very long time, *many years perhaps, and that there is no other source than inflammation*, for a change of colour. Now this is what cannot be admitted BECAUSE the colour of our organs, that of the lungs, for example, UNDERGOES WITH AGE CHANGES WHICH *cannot be attributed to any thing but* SOME PRIMORDIAL LAW, WHICH APPLIES EQUALLY TO ALL PATIENTS." (1)

The reader, who has any physiological principles, will readily see not only how greatly our author has violated his own doctrine, in the foregoing extract, in relation to others, but that his reasoning is utterly fallacious, since morbid conditions observe different laws from the healthy. It may therefore be, that a *gray* colour of a morbid nature may always depend upon inflammation, as much as abnormal lymph, pus, &c. (See p. 635 — 640.) Nevertheless, it is a doctrine of our author, although in opposition to the whole of that pathology which rests upon the shades of colour, and which forms the prominent feature of every case, that,—

"To confine ourselves to the description of the *colour* of membranes is often useless, and even a *cause of error to those who might draw conclusions from such imperfectly described facts.*" (2)

Looking back upon the inductions which our author has made from a number of cases too limited to admit of conclusions beyond the abstract facts which they supply, (allowing them not to have been liable to their insuperable objections,) we find an amount of laws and generalizations from the projector of the "numerical method," and the head of the necroscopic school, far surpassing in number and comprehensiveness the general principles, and we may add, hypotheses, which are to be found in the whole range of medical literature. Nor can we permit our author to go beyond the cases which he professes to have analyzed; since it would be a violation of the "numerical method," and of the rule which he has prescribed for others. (P. 693.) These "laws" and generalizations, too, relate mostly to a single disease; nor is it their least obnoxious feature, that the disease,

(1) Vol. i. p. 193, *note.* (2) On Phthisis, sec. 135.

as we have shown, was not, at least in most of the cases, *a fever, but a neglected intestinal inflammation.* To this conclusion, we think the attentive reader must come. The "laws" are intended, also, in a universal sense; for it is not simply affirmed that, such and such symptoms distinguished the Parisian disease; but, it is broadly laid, that such, and such only, are the distinguishing marks of typhoid fever; and without them, nay, without the lesion of the glands of Peyer, there is no typhus. Many other of the lesions, too, are affirmed to be peculiar to typhoid fever, and, whenever either may be present, that it may be taken as sufficient proof that this identical disease is the one which we are, or have been, treating. There is no reservation made for the modifying causes of other times, other climates, other *experience;* but, the whole is decided in Parisian hospitals, upon the dross of Parisian society, and by the practice of which we have given an outline. And these, too, are the boasted trophies of morbid anatomy, "since it has been cultivated *as a science.*" Who, then, shall again censure Sir Charles Bell for his manly declaration, that, "by pottering in the dead-house we shall not discover the sources of disease?" Upon these cases, too, is erected the most stupendous system of practical errors, resolved into universal "laws," and often extended by a false "analogy," and "*a priori*" conclusions, to other diseases of the gravest nature.

The entire result is a series of generalizations without a parallel in science; and although founded professedly, upon the narrow experience of an individual, who avowedly rejects the observations of all past and present time, and in defiance of that rule which he prescribes, of waiting for a mass of materials that shall alone stand as laws and generalizations, they nevertheless come upon us with such an air of simplicity, such a subduing confidence, that it is probable our author's followers have been taken by surprise, rather than by the nature of his "rigorous facts." And how far is the foregoing in accordance with our author's objection, that *Poliniére, "like his predecessors, lays down general principles with a view to deduce particular indications and rules of practice?"* (1) Or may not the following reproof be properly retorted?—

"One of them [Poliniére,] having seen some cases, in which more or less speedy relief has followed the application of leeches near the affected part, has decided in favour of this practice." (2)

(1) On Bloodletting, p. 68.

Both of the foregoing comments are worthy of consideration. By the first, principles in medicine (save our author's) are rejected. The last excludes the most obvious dictate of experience, whilst it is this limited ground alone upon which our author has proceeded practically, and speculatively. Laennec having treated his 28 pneumonic patients at the Clinique of the Faculty, in 1824, with *entire success*, by a mixed practice of bloodletting, antimony, &c., our author proceeds to invalidate the cases by saying that,—

"The statements are deficient in details concerning the number of bleedings, the age of the patients, &c.; so that we are not able to institute a comparison between these observations and others." (1)

Laennec's observations are therefore cast into the general wreck. But, it may be worth while to inquire by what process our author has achieved so extensively his object of abolishing, or defeating the salutary operation of bloodletting, and blisters, in pneumonia. He has despatched his 78 cases in 11 pages that are equivalent to about 4 of our own; being exactly 4 lines to each case. He has not said one word in relation to those 78 cases as to the quantity of blood which was abstracted, excepting in a solitary case, and in a vague statement in a *note*, where it is said that "from 10 to 15 ounces were taken at each bleeding"! There is not a word as to anatomy, and scarcely a word as to symptoms, &c. "Vesication was employed in a certain number of cases, but had no appreciable influence on the progress of the disease." (2) Upon this general statement, he affirms that, vesication "ought to be banished from the treatment of all inflammations of the chest." To the 32 subjects of erysipelas $4\frac{1}{2}$ pages are devoted.

We are told, however, that, "M. Louis never forces his conclusions *beyond the number of facts* he is analyzing." (3) This, as we have shown by our extracts, is not only a capital mistake in regard to the work to which the foregoing affirmation relates, but to almost every "conclusion" at which our author has arrived. Let us recollect, for instance, some of the common examples of our author's deviation from his own principles. He applied blisters to the "legs" and "thighs" of a certain proportion of 50 subjects of a highly disorganizing inflammation of the stomach, intestines, liver, spleen, brain, &c. grouped under the denomination of "the typhoid affection," and, from the results of

(1) On Bloodletting p. 51. (2) On Bloodletting, pp. 8, 9.
(3) Cowan's Translation of Louis on Phthisis, p. 35, *note*. — Why is this said?

this long neglected complication of disease, and a subsequent general system of treatment which could have only exasperated the whole assemblage of morbid lesions, he draws the "rigorous conclusion," that "blisters should be banished from the treatment" of a disease having those lesions. (Gen. 78, &c.) Nearly the same affirmation is made of bloodletting, which had been practised only in a degree perniciously small. Nor is this the least of the evil; for our author extends the same conclusions, by "analogy," and "a priori" induction, to most other profound inflammations. Exactly the same system is pursued in our author's other works. Take his important Essay on Bloodletting, which is composed alone of mere summary conclusions, and recur to the practical rules which we have abstracted from it. Upon the most general and superficial statement of a few cases of pneumonia, erysipelas, angina gutturalis, bloodletting and blisters, applied in the same indiscreet manner as in "the typhoid affection," are banished from the treatment of those diseases. And here, too, we have the same "*a priori*" inductions in respect to all great thoracic inflammations. We have seen, indeed, that having failed with leeches in 13 cases of *angina tonsillaris*, he argues,

"From the same facts, we should be led to question the great advantage of the application of leeches to the *epigastrium* in *gastritis*, or *to any other part of the abdomen corresponding* to the viscera presumed to be diseased."

True, the next words are, "*indeed, what confidence can be placed on the a priori precepts commonly advanced on this subject.*" (1) But, have those who have studied our author's writings taken advantage of his self-contradictions.

Take, again, another aspect. What is there in the whole range of medical science comparable with the induction, that the great convulsion of every vital power and every function, aye, and every lesion of structure, which pervaded almost the entire body in "the typhoid affection," are inflicted by the "lenticular patches," whose only probable function is to generate a lubricating fluid. And, is not this hypothesis the sole offspring of morbid anatomy, founded wholly upon a comparative frequency of structural lesions? Are not, also, all the vital signs, all the lesions of the great vital organs, all the pathology local and general, entirely overlooked in this signal act of generalization?

However humble the present performance, we cannot but sup-

(1) On Bloodletting, p. 21.

pose that it will call forth some of the able and ardent admirers of our author. "The numerical method," with its stupendous fabric of generalizations, and absurdities in pathology, so fresh from the hands of their projector, must not be sacrificed without an effort to sustain them. But, let us have the defence upon the "mathematical system;" let us have *demonstration*, and not *mere assertion!* We offer this advice in all candour; for, at this thinking age, nothing prevails in the end but *logical conclusions*. Nor let it be said that we have not given a sufficient abstract of our author's own language, or of his self-contradictions; nor, that we have at any time conveyed an imperfect apprehension of his meaning, by insulating his "general laws," and his "rigorous results." More could not have been done without a republication of his work. Some of the general laws, or analogous inductions, have been repeated at other times for other purposes; but, we believe it will be found that we have not arranged any one a second time amongst the generalizations. In two or three instances, as in Gen. 74, 75, and 76, we have considered a paragraph as embracing that number, since as many distinct propositions are obviously included. It may be thought, however, that in the foregoing instance, and in one or two others, our author did not lay down his experience and his conclusion as a "*law*." But such is clearly the import of the expressions, *per se*, and when connected with what precedes, but which it was, of course, impracticable for us to state, the application is as comprehensive as in the examples which he avowedly designs as "*laws*," or "*rigorous* results" of universal import.

Doubtless there are some who will continue to justify our author's generalizations, however they may, on the other hand, applaud the system of dependence upon "rigorous facts," and their indefinite multiplication, to the exclusion of all principles but such as fall from the master. The code which he has delivered may be thought to possess a sufficiently broad foundation; especially seeing that medicine should make some advances in the hands of our author beyond its imputed "infancy." We shall, therefore, still endeavour to convince the most partial advocate of the mathematical calculus, of its unexampled and dangerous power, as exercised by our author; especially when employed under the *protest* that, "in sciences of observation, general principles can be the result only of particular facts, sufficiently and properly estimated." (1)

(1) On bloodletting, p. 75.

In our author's work on typhoid fever, he says,—

146. "In my Memoir upon Sudden Death, I have given the history of *a patient* who had *three small ulcerations* in the small intestine; on which observation I remarked, that if any conclusion could be drawn from AN ISOLATED FACT, *we should be induced to believe that mild cases of continued fever* ARE GENERALLY ACCOMPANIED BY SOME ULCERATIONS OF *the small intestine and inflammation of the mesenteric glands.*" (1) — On turning to the work referred to, we find that he adds, — "Nevertheless, we are in no wise disposed by this remark, *to prejudge* the *important question* of the NATURE OF FEVERS." (2)

"*Thus he obscurely hinted at Orpheus.*" (3) "A most capacious hypothetical casting-net! and thrown abroad with a bold arm! but unluckily, like many others, constructed so as to catch *facts not sought for, and to suffer others to escape through its meshes.*" (4) We care not for the apparent caution with which the overture is expressed. It was necessary to the success of the conclusion, — to its influence upon minds that never think, — that this induction from "a solitary fact" should be conveyed in the seductive shape almost of an interrogatory.

(1) On Typhoid Fever, vol. ii. p. 320.

(2) Mémoires, Ou Recherches Anatom. Patholog. &c.; p. 479. — "Toutefois nous ne voulons aucunement préjuger par cette remarque l'importante question de la nature des fièvres."

(3) Demosthenes.

(4) Beddoes, Researches on Fever and Inflam. p. 131.

Let us carry out our author's numerical system of chances, in relation to the foregoing fact and induction. We find the calculation in a measure prepared for us by the distinguished Hartley. Taking our author's premises for our basis, and the derided facts of a long line of observers, reaching back to the era of Hippocrates, and taking the value of each evidence to be $\frac{1}{a}$, and the number of evidences to be n, then will the resulting probability be $\frac{1}{a^n}$. Now, it is evident that $\frac{1}{a^n}$ decreases with every increase of a and n. Again, if the common deficiency be $\frac{1}{a}$, and the number of evidences be n, as stated, the deficiency of the resulting probability will be $\frac{1}{a^n}$, which is practically nothing, where a and n are considerable. Thus, if a and n be each equal to 10, $\frac{1}{a^n}$ will be $\frac{1}{10,000,000,000}$, or only one in ten thousand millions; a deficiency from certainty which is utterly inappreciable to the human mind. Our author's induction, therefore, amounts to exactly this degree of probability, according to the rule which our author has prescribed. Upon the same principle, it is of very little importance whether we have 1, or 100, or 1000, facts in relation to questions which are not referable to the immutable laws of nature, but depend upon extraneous and accidental causes; such, for instance, as we have endeavoured to show, the general dependence of the intestinal lesion, described by our author, upon accumulated fæces, neglected treatment, &c. And we may safely affirm, that nearly all our author's generalizations repose upon analogous contingencies. On the other hand, the inductions of Hippocrates, of Hunter, of Newton, were based upon the phenomena

And what is the pathological condition of the small intestine upon which "continued fevers" are made to depend, as broadly and positively affirmed by our author in other places, however incautiously he may have located yellow fever in the liver? It is "something exactly opposite to inflammation, if we can conceive of such a condition." And what are the remedies for such a mysterious mode of disease? These our author has already told us.

In respect to the foregoing solitary case, that we are right in our construction, that it was in reality intended to "prejudge the important question of the *nature of fevers*," is apparent from our whole preceding investigation; whilst the statement is reiterated, and stands in marginal relief, in the work on typhoid fever. But, take your most lenient construction, what else is implied than that "a solitary fact" may be taken as the basis of a vastly comprehensive and mysterious law, — so only it be arranged according to "the numerical system." Enumeration is the gist of the doctrine.

"No one," says our author, "*denies* the necessity of a *large body of facts*, in order to decide upon the best treatment of any disease *whatsoever;* but of *what service* are these *facts*, if they are not *enumerated?*" (1)

But, you are more charitable to our author than the sternness of justice may seem to demand? Take we, then, another example of the foregoing nature by which our author, — all opposed as he professes himself to analogy, — prepares you for his vast and multitudinous generalizations.

147. "It is *only necessary*," says our author, "to have met a *single example to feel convinced that in many instances*, inflammation and tubercles in the lungs are *independent* of each other;" (2) no matter, too, of what vital interest this great question may be to the human family.

Let us now take another work of our author, (his latest translated, and not the least commended,) for an exemplification of the foregoing doctrine. It is the fruit of our author's few day's observation of the yellow-fever at Gibraltar, in 1828. Our author discovered an altered state of the liver in all the cases, the most remarkable of which was "*a paleness and anæmic state*" of the organ. This, as we have seen in another place, (p. 740,) was regarded as a new and mysterious pathological condition, and the whole disease is ascribed to this intractable lesion. (3)

that appertained directly to the great laws of nature; and such of them as were so predicated, no time nor circumstance can alter.

(1) On Bloodletting, p. 68. (2) On Phthisis, s. 478.

(3) How far this alteration, and the remarkable integrity of the mucous membrane of the stomach, correspond with what has been observed of the liver and stomach in

But, how does our author reach this conclusion? It is avowedly upon "a solitary" case. Thus,—

148. "We should recollect, also, that the first symptoms of the inflammation of the gastric and duodenal mucous membrane showed themselves sometime after the attack of the fever, *whilst the alteration of the liver was most marked in* A CASE, (Scott's,) terminated by death in *three days* and some hours after its commencement. *From this fact* we may place the commencement of the *lesion* either at the same time with, or soon after, the commencement of the disease itself." — And again: — "How *important is this character* to any one who is called upon *to study* an epidemic at its commencement, or to answer the question, is the yellow-fever ever sporadic at Gibraltar?" (1)

As to the "three days and some hours," if we turn to other observers of this epidemic, we may find a reason why greater lesions of structure were not observed in the cases that may deserve a claim to the consideration of yellow-fever. Thus Dr. Amiel: —

"The rapid progress, and the short duration of the epidemic yellow-fever of 1813, '14, and '28, at Gibraltar, *left no time* for visceral obstructions to be formed." (2)

How well exemplified, therefore, in our author, that disposition which is set forth by Sprengell, in our introductory extracts, (p. 682,) as having more or less prevailed in his day; our author locating all diseases in some special lesion of structure. The typhoid fever is constituted by a lesion of the glands of Peyer;

other climates, our readers can readily judge for themselves. We may refer, however, to what we have quoted upon this subject, for other purposes, at page 312, and vol. i. p. 687, &c. So, also, Boisseau, — "the *liver* is almost always *large, gorged* with blood, *soft*, &c. "*All physicians*, who have written upon the subject of yellow-fever, *agree in saying*, that, in the greater number of cases, the mucous membrane of the *stomach* is more or less bright-red, or of a brownish-red, sometimes ulcerated, gangrenous, and destroyed in part of its extent." (*a*) There is also a remarkable want of coincidence in the vital signs of our author's cases and those which have distinguished the yellow-fever of other times and places. And, our author endeavours to show that some of his cases were of "the typhoid affection," because the glands of Peyer were found diseased; and he assumes the state of the liver and that of the glands of Peyer as the ground, mainly, of distinguishing those supposed simultaneous endemics from each other. There is the same persevering adherence, throughout the work, to the anomalous condition of the liver as an essential condition of yellow-fever, (if it be not regarded as the disease itself,) as to the alteration of the glands of Peyer in the Treatise on the Typhoid Affection. In either case, those affections are the genii of the disease. It is said, for instance, of Scott's case, that "the well marked lesion of the liver was the only one showing the body of Scott to be that of an individual dead of yellow-fever." "We may always have a confirmation of our diagnosis by an examination of the organs and particularly of the liver," &c. (See pp. 692, 739—740.)

(1) On the Yellow Fever of Gibraltar, p. 120. — 1839.

(2) Edin. Med. and Surg. Journ. vol. xxxv. p. 280.

(*a*) On Fevers, pp. 322, 323.

the intermittent, by "an alteration of the spleen," (p. 745;) phthisis, by pulmonary tubercles, (p. 743;) the yellow fever, by a "specific disease of the liver;" and so on.

Finally, numerous examples like the following abound in our author's works. Having examined three subjects who died of scarlatina, in each of whom there were "a greater or less number of *solitary crypts or glands*, more or less enlarged white or reddish," our author comes at once to the conclusion, that,—

149. "This would seem to *prove* that the enlargement of the glands, supposing this was the lesion, *if not constant*, is of very frequent occurrence among those who die of this affection." (1)

It is, however, due to our author to say that he sometimes stops short of "rigorous conclusions" where the cases may not exceed a dozen, especially when the subject may relate to some "*law*" of inferior moment. Like palpable contradictions, and occasional avowals of diffidence, it gives to an author the appearance of moderation and impartiality. Thus:—

ALMOST INVARIABLY, tubercles existed more or less numerously in BOTH LUNGS. We have, however, seen this lesion limited FIVE times to the left lung, and TWICE only to the right. Can we, from this fact, consider the *right* lung as rather less predisposed to the development of tubercles than the *left*."(2)

Of "the numerical method" we have, perhaps, said as much as is incumbent upon us in our first volume, pages 293—309, 332. We have there exhibited an instance of its practical application in the hands of others, (p. 305;) and we will now present an illustration by our author.

"In *any* epidemic, for instance, let us suppose 500 of the sick, taken *indiscriminately*, to be subjected to *one kind of treatment*, and 500 others, taken *in the same manner*, to be treated in a different mode; if the mortality is greater among the first, than among the second, must we not conclude that the treatment was less appropriate or less efficacious in the first class, than in the second? It is unavoidable." (3)

This, then, is the whole philosophy of medical science, as promised by the mathematical system. But what becomes of our author's generalizations, in the meantime? What room for any principles with such uncompromising empyricism? It is the worst species of empyricism, upon a whole-sale traffic; regardless of all pathology, and excluding all thought. Doubtless, however, the foregoing statement of the case has appeared to many a very logical method of reasoning and induction; but,

(1) Vol. i. p. 195. (2) On Phthisis, sec. 15.
(3) On Bloodletting, p. 59.

it might, perhaps, be well to blend both methods of treatment in many of the 1000 cases, or to associate other remedies, or to vary them in every individual case; or, what is very probable, from what we have seen of our author's exclusion of bloodletting, blisters, &c. from the treatment of thoracic inflammations, both systems might be worse than the expectant plan. We cannot allow that our author's method proves any thing more than its Utopian nature; especially if "nothing is more fatiguing than the method we have described," (1) or if, with our author, we may "*repeat* that *an excellent mode of arresting diseases is to confound them;* or at least to make *no distinction in the periods at which such and such remedies were employed.*" (2)

As to our author's prolific "facts," and the generalizations which have grown into proverbs, we doubt not that the time is near when they will prove "like unto garments, straight at the first putting on, but by and by wear loose enough." (3)

Nor will we fail to express our conviction that the "numeri-

(1) Louis, on Phthisis, Preface, p. lxviii.

(2) Ibid. on Bloodletting, p. 31.

It may be worth while to give our author an exemplification of American hospital practice in pneumonia where no numerical system had been adopted; by which it may appear that we have been right in our conclusions in the text, and that the only induction which our author's cases can supply is, that his method was not the best; however our author would lay down the rule, in consequence of his bad success, that our American practice should be abolished. It will be recollected, that of 78 of our author's pneumonic patients, of whom he states, in one line in a *note*, that "from 10 to 15 ounces were taken at each bleeding," (*a*) 28 perished. Now let us look at an institution over which true science holds its sway; the Massachusetts General Hospital. In four years, there were treated 51 cases of pneumonia, of which only eight terminated fatally. It is also the opinion of the eminent Reporter, as founded upon the results of his energetic and philosophical treatment, that

"*Pneumonitis*, when single and uncomplicated, in an adult, not tuberculous, not having any grave disease previously, will *very rarely prove fatal.*" (*b*) (Vol. i. p. 300.) Can, then, the publication of our author's work, in America, prove an advantage to us? What, we respectfully ask its distinguished editor, can be gained by substituting a practice, which, according to the "numerical system," is fatal in more than one-third of the cases, for that which is always successful?

And what says our Parisian author of the *antecedent* state of health of his 78 patients? "*All were in a state of perfect health at the time when the first symptoms were developed.*" (*c*) We need not help our reader to any farther conclusions. Bloodletting was employed in both the instances; but, in the former it was efficiently done, was adapted to the various exigencies of the several cases, and had the benefit of other associated means.

(3) Lord Bacon, vol. i. p. 452.

(*a*) On Bloodletting, p. 9.
(*b*) Jackson's Appendix to Louis on Bloodletting, p. 170. (*c*) Louis on Bloodletting, p. 2.

cal system" was designed for a subordinate part in the drama of the Parisian actor. Standing by itself, its fame would have died with its author; nor would the cases in phthisis, or pneumonia, or the typhoid affection, have obtained notoriety but by the generalizations which are founded upon them. (P. 588.) The principle to which we advert is well expressed by another in relation to Flamsteed and Newton, at page 582. No other "method" could have struck at all past observation, and all existing principles in medicine, and as its author declares, have placed the science in its "infancy." Our author could not "otherwise have killed off all his brethren." (P. 682, *Kames.*) Hunter, or Bichat, could alone have resisted any expedient short of the power of mathematics. "The rational and practical assent exert a perpetual reciprocal effect upon each other; and, consequently, the ideas belonging to *assent* and *dissent*, and their equivalents and relations, are highly complex ones, *unless in the cases of very simple propositions, such as mathematical ones.*"

Thus mounted upon the wreck of philosophy, "the numerical method" became the engine in rearing that fabric whose construction it was destined to serve. This is the charm with which the numerical system is invested; whilst it gives to its author that ascendency in mind which few can truly obtain by the legitimate rules of induction. "After much deliberation," as Isocrates says, "he found the thing could not be compassed in any other manner;" or, as our author has it, "fortunately for the progress of science, the numerical method is considered by the most judicious and experienced men as a *necessary instrument for establishing general principles in medicine.*"(1) Accordingly, former systems, and former facts, fell as by enchantment. The mind sickened at the absence of all principles to guide it, and was therefore the more willing victim when assailed by the irresistible power of numbers. If the demonstration was made with reiterated professions of a regard for facts, it was because the method could have had no existence without them, whilst the perpetual epithet of "rigorous" left no room for skepticism. But, as related of "good queen Bess," "the commissioners used her like strawberry-wives, that laid two or three great strawberries at the mouth of their pot, and all the rest were little ones; so they made her two or three good prizes of the first particulars, but fell straightways."(2)

(1) On Bloodletting, p. 63; On the Method of Observing, p. 175. &c.

(2) Lord Bacon's Collection, &c. Aph. 19.

"This manner of digression, however, some dislike, as frivolous and impertinent; yet we are of Beroaldus' opinion,—such digressions do mightily delight and refresh the reader. They are like sauce to a bad stomach; and we do therefore most willingly use them."

Our author was not insensible that he might become the subject of the same reproach which he exercises towards others; and, by anticipating it, he endeavours to disarm its effects, or to lull it into silence. Thus:—

"One is often obliged to have recourse to hypothesis, when treating of the interpretation of facts in a particular case; and I *remind* the reader of this now, *in order that he may not be surprised at my forming conjectures sometimes, and that he may not think that I shall be as little rigorous when attempting to draw* GENERAL CONCLUSIONS *from facts.*" (1)

We shall now subjoin a tabular view of all our author's fatal cases of the undoubted typhoid affection, which we have endeavoured to compile with care. We have left out the symptoms, with exception of the appearances of the tongue, since they have been stated in the foregoing remarks. The preliminary symptom of diarrhœa we have retained; as that is something of which a hospital patient might render a tolerable account. We believe that nothing of the least importance has been omitted of the treatment of each case. Of the lesions of structure we have already given a tolerable account; but the reader will find upon that subject, that much remains untold. As "blisters to the legs" are of such constant recurrence, the reader will find the philosophy which prompted their application to that part for the relief of the great visceral inflammations, by referring to our author's reprobation of Quesnay for objecting to the already exploded doctrines of "derivation" and "revulsion," as expressed in our author's language at page 681. But, notwithstanding the asperity with which he there speaks of Quesnay in relation to this question, and, although our author's uniform treatment by "blisters and sinapisms to the legs" was dictated wholly by the doctrine of derivation, yet let us hear him, when arguing against the use of blisters:—

"It seems to me, that if the sketch of the lesions which I have just given is not sufficient to overthrow the doctrine of *derivation* it must excite in the best minds many doubts about the utility of the precepts upon which the doctrine depends," &c. (2) And yet our author explodes blisters, by experiments founded upon this principle.

(1) On Typhoid Fever, vol. i. p. 137. (2) Vol. i. p. 384.

TABULAR VIEW

OF THE

FATAL CASES REPORTED IN M. LOUIS' WORK ON THE TYPHOID AFFECTION.

Cases.	How long sick before admission. No. days.	Diarrhœa before admission. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other Remedies.	Diet.	Tongue, in the consecutive order of appearances.	Died after adm. Days.
1	6 (1)	6	None.	2 emollient.	10 oz.	.....	Straight jacket; blisters to legs; epigastric fomentations; infus. cinch. gum potion sulph. quin. grs. xx.	Barley water, org. sir. tart. whey; lemonade.	Moist, red at tip, whitish in other parts; dry at tip; dry, red; blood red; pale red.	10
2	11	.....	Ol. Ricini 1 oz.	2 do.	.....	58	Sinapisms to legs; blisters to do.; ice to head.	Whey; lemonado.	Yellowish and grayish at centre; a little dry, but not red; moist and yellow; dry.	10
3	8	8	None.	1 do.	.....	(2)	Straight jacket; four blisters to legs; fomentation; two days before death, ten grs. musk; and the day before, ext. cinch. 2 drh.	Sweetened barley water.	Dry, but not red, trembling; dry and rough, and red.	18
4	21 (3)	11	None.	Camph. cinchon.	8 oz.	.....	Straight jacket; blisters to legs; gum potion; fomentation; cold infusion of cinchona, potion with wine of cinchona and syrup.	Do.	Natural at tip and middle, whitish in other parts; gluey and *blackish* and yellowish in some parts.	5
5	14	Not known how long.	Ol. Ricini 1 oz.	2, 1 of fern.	Bled once.	.....	Blisters to legs and chest; blister to thighs; polygala, bouill. aux herbes; infus. and wine of cinchona 2 oz.; fomentation of camphorated alcohol.	Do.	Moist, clean at edges and whitish in centre.	11
6	15	7	None.	2 emollient.	8 oz.	Once.	Straight jacket; blisters to legs.	Whey; lemonade.	Gluey and bright rose colour.	2
7	20	11	None.	1 do.	.....	50	Fomentations; sinapisms to limbs; bath; 8 pounds of ice to head.	Sweetened barley water.	Dry and rough; protruded imperfectly.	7
8	7	.....	None.	None.	10 oz. at *request* of patient. (4)	12	Sinapisms to legs; simple oxymel; gum potion; 6 pounds of ice to head.	.....	Natural at edges, and yellowish at centre; dry and pale.	5
9	5	5	None.	None.	15 oz.	20	Straight jacket; 8 pounds of ice to head.	Lemonade.	Dry, thickly coated, and somewhat red in centre.	5

(1) It is incorrectly stated that this patient died on the 20th day of disease. (2) Leeches, once before admission. (3) Worked during the first fifteen days. (4) Bled twice before admission, and leeched. N. B. It should be observed, that leeches, blisters, &c. were sometimes prescribed, but not employed. Where leeches were numerous, they were applied at different times.

Cases.	How long sick before admission No. days.	Diarrhœa before admisssion. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other Remedies.	Diet.	Tongue, in the consecutive order of appearances.	Died after adm. Days.
10	7 1-2	4	None.	None.	.....	.....	Nothing.	.....	Not stated.	1-2
11	4	On the preceding days.	None.	None.	10 oz.	.....	Blisters to legs and thighs.	Sweetened barley water; whey.	Moist, red at edges, and whitish and yellowish at centre; white and moist.	4
12	7	Not known.	None.	None.	.....	40	Nothing.	.....	Not stated.	1
13	10	Not known.	None.	None.	.....	.....	Nothing.	.....	Pale and moderately moist.	1
14	7	Before sickness began.	None.	5 enemas.	.....	20	Straight jacket; blisters and sinapisms to legs; ice to head. For four days, cinchona, quinine 20 grs. aromatic fomentations.	Beef tea; decoc. of barley; oxymel whey; wine.	Dry, trembling, and reddish at edges; yellowish coat, and not entirely dry; dry, and trembling.	33
15	28	28	None.	2 enemas, one of camphorated cinchona.	.....	.....	Blisters to legs; ice to head; tonic potion; ext. Cinch. half oz. limon. muriat. emol. and aromatic fomentations; some Spanish wine, sulph. quinine. 15 grs.	Sweetened rice water.	Dry, like wood, and not red; dry, *blackish*, and thickly coated.	8
16	5	5	None.	4 emollient.	.....	.....	Blisters to legs and thighs. Aromatic fomentations for two days. An infus. cinch. and much wine, "with no manifest benefit," ext. cinch. 3 drhs.	Whey; lemonade, 3 quarts of it on one day; rice fritters; soup.	Moist, bright rose at edges; dry and red at centre; coated thickly in centre, alternately dry and moist and red at edges; red and moist.	38
17	8 (1)	.....	None.	2, one of cinchona	12 oz.	.....	Straight jacket. Blisters to legs. Fomentations; seltzer water, two glasses of wine. Wine of cinch. 3 oz. syrup cinch. 2 oz.; ext. cinch. 2 drhs.	Whey; yolk of egg; beef tea.	Dry and *blackish*; slightly coated; moist and viscid;—thickly coated, *blackish*, thickened and moist; not coated; again dirty.	32
18	15 (2)	11	None.	1 of flax seed, and others.	Twice, once 16 oz.	90	Straight jacket. Blisters to back of neck. Ice to head; once a potion of wine and syrup of cinch.; once a decoc. of simarouba and diascordium[1] 2 scrup. a few drops of muriatic acid added to drink and enemata.	Syrup of gum; sweetened barley water; tamarind; rice fritters; beef tea; rice water.	Reddish at edges and whitish at centre; dry, not deep red, and somewhat thickened; moist and variable in colour; sometimes more reddish than natural; covered with white patches; always moist and whitish; alternately dry and moist; red at first, and greyish afterwards.	59

(1) Walked to the Hospital, without assistance. (2) Walked to the Hospital.

Cases.	How long sick before admission. No. days.	Diarrhœa before admission. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other remedies.	Diet.	Tongue, in the consecutive order of appearances.	Died after adm. Days.
19	12 (1)	12	None.	1 of flax seed.	.....	12	Straight jacket. Blisters to legs and thighs; sinapisms to feet; day before death, an infus. of cinch. and 20 grs. of quinine.	Whey; lemonade.	A little gluey, and red at tip; a little moist and red; dry and reddish.	13
20	3	3	None.	None.	.....	78	Pil. hydrarg. No. 22, (alterative.)	Rice water with gum arabic; whey.	Greyish.	11
21	14	14	None.	2 of flax seed.	12 oz.	18	Blisters to legs; simple oxymel; syrup of tartar; tamarind; sinapisms to legs; fomentations.	Whey; emulsions.	Slightly moist, whitish at centre and vivid red around edges; greenish white and less red; dry.	8
22	8	.....	None.	None.	(2)	.....	Syrup of tart; blisters and sinapisms to legs.	Whey.	Dry and red at tip; dry and ruddy; dry and brownish.	12
23	10 (3)	.....	None.	None.	14 oz.	.....	Straight jacket; blisters and sinapisms to legs; syrup of tart.	.....	Natural at tip, and white at back part; protruded with difficulty.	4
24	22	8	None.	None.	.....	(4)	Straight jacket; blisters to legs; syrup of tart.	Honeyed barley water.	Red, dry, and thickly coated at centre; *blackish* and slightly coated.	3
25	6	5	None.	4 emollient; 1 of camomile tea.	.....	15	Blisters to legs; gum potion; fomentations; blister to chest; infus. cinch. and a drachm and half of the extract in a gum potion.	Sweetened barley water; acidulated rice water.	Natural; dry in centre and moist and red at edges; moist, more or less red at edges, and greyish at centre; dry and thickly coated.	16
26	7	4	None.	3, flax seed.	8 oz.	.....	Lemonade, barley emulsion.	Barley water; beef tea; rice fritters; "wept for them."	Natural at edges, greyish at centre, a little red at tip; dry and ruddy; papillæ prominent and yellowish; moist; small white patches; not easily protruded.	26
27	15	.....	None.	2 of camphorated cinchona.	.....	.....	Straight jacket; tonic potion, with ext. cinch. 3 and a half ozs. and gum potion; blisters to chest; syrup with tartar; frictions with camphorated alcohol.	Not stated.	Dry and not red; imperfectly protruded; last day, dry and *blackish*.	7

(1) Walked to the Hospital without much apparent fatigue. (2) This patient was probably bled once. (See Vol. i. p. 217.) (3) Walked to the Hospital. Had been bled once (4) Twice before admission.

Cases.	How long sick before admission. No. days.	Diarrhœa before admission. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other remedies.	Diet.	Tongue, in the consecutive order of appearances.	Died after adm. Days.
28	3	3	None.	1, cinchona.	Bled twice, once to 10 oz.	12	Blisters to legs; sinapisms to feet; syrup of tart. and orge. ice to head; fomentations with aromatic wine; an infusion of cinch. with gum arabic; "a little wine."	Sweetened barley water; whey; tamarind whey; beef tea, and half a rice fritter; rice water acidulated.	Slightly moist and whitish on edges and reddish in centre; rather moist and violet red in centre; purplish and moist; less moist and less red; a little redder, moist and good colour; a little red and dry.	22
29	10	7 days before sicknesss.	None.	2, one of camphorated cinchona.	.....	.....	Cataplasms; orge. syrup of tart.; gum potion; lemonade; aromatic fomentations; syrup of cinch. 2 oz.; wine of cinch. 4 oz. and sulph. quinine, 100 grs.	Barley water and, muriatic acid.	Yellowish in centre, red at tip; furrowed and thickened; more or less coated or dry with furrows; alternately moist and dry; sometimes thickly coated.	19
30	24	Some time.	None.	2, "half emollient."	.....	40 or 50	Blisters to legs; sinapisms to feet; gum water; fomentations of camphor; alcohol; a pound of ice to the head.	Gum water.	Dry and ruddy; dry, brownish, a little thickened; natural at circumference, and *blackish* at centre; covered thick.	12
31	8	.....	None.	4, "half enemata." (1)	.....	28	Straight jacket; blisters to legs; lemonade; gum syrup.	Beef tea.	Moist, a little villous, otherwise natural; a little yellowish behind; otherwise natural; not very moist.	8
32	5	5	None.	Half an enema, continued.	.....	.....	Blisters to legs; emollient cataplasms; bath.	Rice water, with gum	A little moist, color natural; greyish in centre; always moist.	18
33	15 (2)	Constipation and diarrhœa.	None.	5 emollient, 1 of camphorated camomile tea.	10 oz.	27	Blisters; sinapisms to legs; ice to head; cinch. applied to blisters; ext. cinch. half drh.	Sweetened barley water; whey; lemonade.	Somewhat red at edges, very moist, and villous and yellowish at centre; dry; less dry.	13
34	15	Constipation and diarrhœa.	None.	1, flax seed.	.....	8	Blisters to legs and thighs; sinapisms to feet; a few grs. of ipecac; lemonade; gum potion.	Lemonade.	Red and dry at point; yellowish and villous behind; red and moist; dry and red in front.	10
35	28 (3)	28	.....	2 of flax seed; 1 of camphorated cinchona.	.....	.....	Sinapisms; baths; 4 pounds of ice to head; infus. cinch. wine and syrup of cinch. 2 oz. and ext. do. 3 drhs. aromatic fomentations; musk, 2 scrup.; sulphate, quinine 1 scrup.	Sweetened barley water.	Moist, red at edges and whitish in centre; imperfectly protruded, looked as if roasted; sometimes a little moist and red in front.	17

(1) This patient expressed a desire for Enemata every half hour. (2) "Continued to labor the first twelve days," and "continued to eat meat for the same time, without being incommoded by it." "*By mistake* this patient took a purgative mixture which had been ordered for a *painter*, (this subject being a *locksmith*,) and it operated without *vomiting*," "a *fact* which seems somewhat extraordinary."—*Vol. ii. pp.* 120, 124. (3) "Diarrhœa during the whole of this period." *** The subject of Obs. 29 had a dose of 50 grs. of sulph. quinine on the 24th day of disease.

Cases.	How long sick before admission. No. days.	Diarrhœa before admission. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other remedies.	Diet.	Tongue in the consecutive order of appearances.	Died after adm. Days.
36	4	....	None.	1 emollient.	12 oz.	16	Sinapisms; cold vinegar and water to head; lemonade; infus. cinch.; gum potion, with ext. cinch. mercurial ointment; ether 1 scrup. blister to legs.	Whey; decoc. of barley and oxymel.	Red, moist at tip, whitish and less moist beyond; dry smooth and not red; protruded with difficulty, moist in front, and a little red behind; dry.	17
37	15 (1)	15	None.	2 flax seed.	....	30	Barley water, and syrup of tartar.	Barley water.	Rose at circumference, and yellowish at centre; dry, yellowish, and coated at centre.	9
38	8	3	None.	2 flax seed.	.....	.....	Blister to legs; bath.	Sweetened barley water; whey.	Brown and wrinkled.	3
39	35 (2)	35	None.	1 camphorated cinchona.	.....	.....	Blisters to legs and neck; warm bath for six minutes; infus. cinch.; fomentations of camph. alcohol; three glasses of wine; gum potion; wine and syrup of cinch. a a 2 oz. and sulph. quinine 2 scrup.	Barley water with sir. tart.	Rather red at tip, smooth and nearly dry; nearly natural; white on the 8th and red and dry on the 10th and 11th days; moist.	17
40 (3)	9	9	None.	10 flax seed; 9 1-2 grains opium.	.....	.....	2 gum potions, and opium 2 grs.; cataplasm on abdomen; sinapisms to legs; blister to hypogastrium.	Rice ptisan with gum syrup; yolk of egg, with sugar and water; beef tea.	Clean soft and moist; a little less moist; moist and slightly greyish at circumference; moist smooth, pale rose color at edges.	9
41 (4)	18	Constipation.	None.	None.	.....	3 times.	Lemonade; gum potion.	Rice water; beef tea	Natural, a little red at edges; red and little moist; teeth dry; red at edges, and green at centre.	8
42 (5)	23	None.	None.	None.	8 oz.	.....	Blister to chest; sweetened infus. of violets; gum potion.	Sweetened rice water.	A little red at edges, yellow and villous at centre, and mouth pasty.	1
43 (6)	20	None.	None.	1 emollient, 2 opiated.	.....	80	Cataplasms to abdomen; lemonade; cataplasm sprinkled with laudanum; emollient fomentations.	Strict diet; two half rice fritters; rice water.	Clean moist at edges, and villous in centre; pale and clean.	25

(1) Worked till time of entrance. M. Louis supposes this patient was killed by a purgative which was given to him seven days before his admission. (See our page 740.) (2) M. Louis dates its beginning twenty-four days later than as above. (3) Not a case of the typhoid affection. (See our p. 753.) (4) Intestine perforated. Walked *to*, and about the hospital the five first days. (There is a mistake in the number of days the patient was sick.) (See our p. 714). (5) Intestine perforated. Walked out doors at least for a fortnight after disease began. (See our p. 713.) Bled three times before admission to the hospital. (6) Intestine perforated. He worked twelve out of the twenty days. (See our p. 715.)

Cases.	How long sick before admission. No. days.	Diarrhœa before admisson. No. of days.	Cathartics.	Enemas.	Bloodletting.	Leeches. No.	Other remedies.	Diet.	Tongue, in the consecutive order of appearances.	Died after adm. Days.
44 (1)	21	Irregular.	Nonc.	Nonc.	.....	.....	Gum potion; sulph. quinine; white decoction; a half julep.	Sweetened rice water; half an egg.	Natural at edges, and colour of coffee with milk in centre; very moist.	15
45 (2)	15	15	Castor oil 1 oz. (3)	1 flax seed.	.....	12	Simple gum potion; blister to legs; fomentations.	Sweetened rice water; wine.	Rather red at edges; a little dry and red in centre; moist and and whitish in centre.	8
46	7	.....	None.	1 emollient, 1 camporat.	8 oz.	.....	Blisters to legs and neck, lemonade; fomentations to abdomen; infus. cinhon; friction with aromat. wine; sulph. quin. 20 grs., with orange flower water 2 oz., and pep. water 2 oz.; baths.	Tamarind whey.	Moist, not red; dry, and frequently protruded with difficulty; usually dry, yellow and moist.	12
47	8	4	Castor oil, 1 oz. (4)	Flax seed and assafœtida.	.....	4	Sinapisms to legs; baths; gum potion with musk 10 grs.; blister.	Demulcent drinks, and a little weak beef tea.	Natnral; slightly ulcerated, otherwise natural; constantly moist; natural at edges, rarely yellowish at centre.	42
48	15 (5)	12	None.	Flax seed.	10 oz.	.....	Blisters to legs, thighs and chest; lemonade; gum potion; infus. cinch. syrup cinchon glass wine; aromat. foment.	Beef tea.	Not very moist; moderate red at edges; dry, rather tremulous; dry; clammy; soft, not moist; very moist.	24
49	8	5	None.	.....	12 oz.	12	Blisters to legs; ice to head; demulcent gargle; mustard pediluvium.	Honeyed barley water; rice fritters, a little fowl.	Natural at edges, whitish in centre; color natural; pale, a little dry and villous in centre; pale, moist.	28
50	5	.....	None.	.....	.....	Onco.	Lemonade; emol. foment. ext cinch. half dr.; acet. ammon. half oz. orange flower water 2 oz. syrup of pink 1 oz., repeated.	Rice water, wine in it; rice fritters.	Dry, sometimes brownish at centre, natural at edges; dry and reddish at centre; afterwards natural.	45
53 (6)	8	.....	Nonc.	.....	10 oz.	.....	Cool lotions to head; sinapisms; blisters to thighs; lemonade; "emollients under every form," when perforation took place.	.....	Clammy, natural colour, dry; blackish, and thickly coated; a little clammy, colour natural; dry.	5
54 (7)	15	15	None.	3 flax seed.	12 oz.	.....	Blisters to legs.	Barley water sweetened and acidulated.	Dry and whitish at centre, rose colour at edges; somewhat green.	5

(1) Intestine perforated. (2) Intestine perforated. (3) This was given because the patient had passed several lumbrici before he came to the hospital. (See our pp. 723.) (4) Oil given "from fear that some of the spasmodic movements might be caused by worms." (See our p. 723.) (5) Confined to bed four of the days. (6) Intestine perforated. (7) Intestine perforated. A relapse, having had frequent attacks of diarrhœa, for six months. Obs. 46, 47, 48, 49, are "cases in which the anatomical characters may seem doubtful." Obs. 50 had not "the special alteration of the elliptical patches of the Ileum;" and Obs. 51 and 52 were "simulated cases," and we therefore omit them. Obs. 53 and 54 are *special* examples of "perforated intestine."

We now approach a view of our author's position which deeply affects every case recorded in the history of "the typhoid affection," as well as our author's other works, and saps the very foundation of the "numerical system." Nay; it lays waste the whole fabric of generalizations, principles, and laws, which our author had erected upon the victims of that disease. Our author's philosophy is necessarily concerned about *symptoms*, so far as they are important in showing that the disease began in the intestinal canal, and were significant of a primary lesion of the glands of Peyer. This is of vital moment to the hypothesis as it respects the foundation of the disease, and of the entire superstructure.

It has often struck us with surprise, that it has never been observed, that the subjects of our author's cases rarely came under professional observation till many days had elapsed after the attack. The whole of his information, therefore, as to the primary symptoms, must have been generally derived from the squalid subjects themselves; and when it is considered how little others, who are less "rigorous in their facts," depend upon the narrative of symptoms by the more intelligent classes of society for any inductions as to doubtful or even simple points in pathology; and when, also, it is recollected that the analysis of symptoms in our author's cases must have been exceedingly involved from their long continuance, it is obvious that any account of them as derived from the subjects themselves, or their ignorant friends, must be utterly worthless; whilst in nearly all the recorded cases, the patients were racked with "abdominal pains" at some period before they entered La Charité, and delirium was either an antecedent symptom, or was developed soon after the patients came under our author's treatment.

In several of the analyzed cases, also, it is distinctly stated by our author that he could get no information as to the symptoms which had prevailed for the many days before the patients came under medical observation, as in Obs. 12, 13, 20, 22, 23, 38, &c. And yet, these cases go to make up our author's generalizations, and have as much detail and importance assigned to them as any of the others. But, that this momentous question may not be a subject of cavil, we shall quote a remark of our author which is entirely to the purpose:—

"Abdominal pains," he says, "were observed in different degrees of severity in 39 patients, or rather in all the cases about which *I could learn* any thing, *whether from the patients, or from those who brought them to the hospital.*"

But, still more, and absolute, to this important point.

"It is proper, however," says our author, "to remark that I had no other means of knowing when the pain commenced, whether on the first day or a little afterwards, except THE MEMORY OF THE PATIENTS; *and the recollection of this symptom by persons of the* LABOURING CLASSES, *who do not by any means pay special attention to the subject, supposes that it must have been somewhat inconvenient in rather a large number of patients.*" (1)

So far, then, there is no difference between our author and ourselves; but, our author being engaged, in the foregoing remark, about a particular symptom, neglected to observe its general bearing upon his subject. It will be also seen, by this statement, that our author was the acting man. (P. 685.) Again, in section 24, in the work on Phthisis, where a palpable *physical* appearance was the subject of observation, our author rejects it upon the ground, that,—

"*Patients* and their *attendants*, observe with *too much prejudice* to allow their simple testimony to have much weight in the determination of a fact of this description."

Nevertheless, it is boldly stated by our author, and with the sole view of establishing his hypothesis as to the dependence of the whole array of the lesions in "the typhoid affection" upon the glands of Peyer, that,—

"Every time that there were complications, and one could, *by the aid of the symptoms*, discover the periods *at which the different lesions commenced*, that of the *patches of the ileum was* evidently the first." (2)

But, perhaps we have not yet shown enough for all minds, as to the proof that our author's minute statements of those symptoms which marked the early days of the typhoid affection are entitled to no sort of dependence. And, since they are related with all the order, and circumstantiality, as if observed and noted down by our author himself, according to the mathematical system, and since their record is put forth by our author as paramount to all the personal experience and observations of his predecessors and cotemporaries; and, moreover, as all our author's inductions, of any moment, must turn upon the symptoms before the subjects were presented to his observation, we will add, that remarks like the following are not of unusual occurrence:

"A *mason*," the subject of Obs. 24, "answers, prompt and correct upon many subjects; so that the *few details* which I have given above, and which I obtained *from the patient*, corresponded with the account given me by the relations. But, *in other respects, he had complete delirium.*" (3)

Of Obs. 36, third day after admission, he says "*the answers*

(1) Vol. ii. pp. 27, 29. (2) Vol. i. p. 91. (3) Vol. i. p. 296.

of the patient were not in accordance with the questions I addressed him." On the first day after his admission, it is said there were, "*at intervals, somnolency; answers, correct.*" "*Next day, slight stupor,*" &c. In an important case, Obs. 19, the patient had suffered "the typhoid affection" twelve days before entering La Charité. "He came on foot;" "was taken with *delirium* on the following night;" when "the straight-jacket was made use of." And yet our author states that on the morning of that day, he received from this straight-jacket patient "the details" of his case. (1) Indeed, in our author's *first* Obs. it is fully implied by the patient's remark that, "*he said he had been ill six days,*" that our author obtained from the subject, ("*a man,*") all that detail of symptoms which goes to make up the hypothesis that the first lesion was of the glands of Peyer. In Obs. 14, a very important case, "AN ERRAND-MAN" "*stated* that he had been sick seven days." Then follows a detail of the symptoms as derived from the "errand-man." We shall state them as an example.

"The affection had been *preceded*, during the *same length of time*, by pains in the abdomen, rather severe diarrhœa, (eight to ten dejections during 24 hours,) and had commenced with a troublesome headache, pains in the limbs, dizziness, inclination to sleep, considerable weakness, *anorexia*, thirst, cough, great heat without chills. These symptoms, in addition to the diarrhœa, had continued; the weakness had daily increased; DIZZINESS had become more frequent, and so SEVERE that the patient had been unable to WALK without the aid of a *cane*." "He had had nausea occasionally during 48 hours before entering the hospital, for which there was *no obvious cause*, and *at intervals only* had been confined to his bed."

Now let us look at this intelligent patient on the next morning.

"Face, as of one somewhat *sleepy;* tendency to sleep; senses perfect; headache; MEMORY PERFECT; answered slowly; lassitude; motions painful; *considerable sinking;* tongue *dry, trembling; intense thirst;*" and so on. Next day, "somnolency nearly constant; patient *answered only in monosyllables;* involuntary dejections during the day," &c. (2)

It was doubtless, too, at this first visit, that our author obtained from his patient that account of his case which was so important to the ultimate objects. In Obs. 40, the patient, (a *gardener,*) "*said he had been ill nine days.*" Then follows a detail of the nine days' symptoms after the foregoing manner. And, so of Obs. 41; "*said he had been ill eighteen days,*" &c. Obs. 46, ("*a female,*") "*declared she had been ill seven days;*" with

(1) Vol. i. p. 142.

(2) Vol. i. p. 93. — This patient had a straight-jacket every night from April 27 to May 6.

the usual detail of symptoms during that period. Day after admission it is said, — "*memory sure, answers just and prompt.*" "From time to time patient *uttered cries, or was constantly muttering to herself.*" Four days afterwards,

"Face, pale as after a great loss of blood; speech, almost unintelligible; patient enjoyed in part her reason; *she blushed, and was opposed to having her abdomen examined, which was of a perfectly good shape.*" (1)

The subject of Obs. 6, was "*an unmarried female, with well developed limbs.*" Had been sick fifteen days. The straight-jacket was applied the night she entered La Charité; *and she died the next night.* Like the rest, she appears to have told her own tale of sufferings. But, we allude to this case as our author considers it valuable from the circumstance that "*no error in diet had been committed,*" and as there had been no previous tampering with her. (2) In Obs. 13, there was an "almost total want of data, in regard to the state of our patient, previously to his admission into the hospital." (3) In this case, we learn from our author the necessity of the most accurate account of the prëexisting symptoms, to make out his fundamental hypothesis. But, although he allows that the "almost total want of data" "prevents us from assigning, *with exactitude*, the time at which commenced all the different lesions observed after death," and, although the patient "was dying" the morning after he entered the hospital, our author assigns to the case a conspicuous rank among the 49 others. In Obs. 26, (*a carpenter,*) we have *from the patient*, the usual detail of the progressive symptoms. "*The patient,*" says our author, "*assured me that he had a little delirium.*" Obs. 53, ("*a woman*") "*uttered loud cries during a part of the night*" on which she entered La Charité. Gave a very circumstantial account of her symptoms, but "*she had said nothing of pains in the abdomen.*" See, also, Obs. 42, 43, at pages 713—715.

And yet, in cases of the foregoing description, our author proceeds to draw "rigorous conclusions" even as to the primary seat of the disease, which, we need not repeat, is always the glands of Peyer. Nor should it be forgotten that, very generally, when the patients entered the hospital they had become the subjects of extensive structural lesions, with very complex and exasperated symptoms, and that they were frequently moribund, or nearly so. To this must be added the consideration that no efficient treat-

(1) Vol. ii. p. 323. (2) Vol. i. p. 44. (3) Vol. i. p. 87 — 92.

ment was adopted in any case of the disease; but, that in instances where all were of so grave a nature, the treatment was scarcely more effective than that which our author is stated to have pursued in the other cases, at page 642; which, says an impartial witness, "scarcely differed from pure expectancy." It will be readily seen, therefore, that our author's inductions as to the effect of remedial agents, even in their relation to his own cases, are purely factitious, and that the only conclusion which can result is, that "the expectant plan" is, of all others, the most fatal in "the typhoid affection;" unless it be allowed that the mortality was not a little increased by our author's use of bark and wine; the enormous quantities of lemonade, &c. For the same reasons, also, the structural lesions can serve no other purpose than that of showing us to what a deplorable extent they will proceed, when severe inflammations are neglected or maltreated. It is not an uninteresting employment to observe the gravity with which our author descants, in his general section relating to the treatment, as well as in the individual cases, upon the effects of some eight ounces of blood that were abstracted from patients in the high advances of a general disruption of the organization, (the brain included,) or where leeches had been applied to others in a moribund state.

Finally, the following paragraph will show how deeply our author was concerned about the primary symptoms:—

"As the victims of the typhoid affection succumb generally, at a period more or less remote from its commencement, it has been thought that the alteration of the elliptical patches of the small intestine may be only one of the somewhat remote *consequences* of the affection." (1)

We might now leave the farther consideration of this matter with our readers; referring them for information to our tabular view of the cases as to the periods at which each subject came under medical observation. The question, as to the history of the cases during the long period which preceded their connection with the hospital, is unaffected even if it be supposed that the patients were aided by friends in their recollection, or systematic detail, of their symptoms. Our author, indeed, excludes the authority of *medical* observers upon this subject, unless the symptoms be reduced to *mathematical tables*. We have been thus circumstantial, since it is abundantly obvious, that the whole gist of our author's work must turn upon the primary symptoms of

(1) Vol. i. p. 55.

disease; whilst these had become long involved in a consecutive series determined by various complex disorganizations, and which, from being the last and most prominent and distressing, would very naturally be imposed upon our author as the *first* in order. Every practitioner has this daily experience among intelligent people; relying, indeed, (especially in violent diseases,) more upon the recollection of the friends of the patient, than of the patient himself. When the subjects of our author's cases had advanced as far as the hospital, the complication of *structural* lesions had become so great as to render them perfectly useless for any questions in pathology which have not been understood from the days of Hippocrates; and this more especially since, after the cases came under professional observation, "the cerebral symptoms masked the manifestations of those dependent upon the severest lesions *in nearly all* the cases." (1) The principle of analysis, therefore, could have no application even at the stage of disease when medical observation could have alone taken any cognizance of the cases; so that, our author's doctrine must have been carried out to the last, viz., — "*what importance is it with respect to effects, for us to know what is the cause of trouble in our functions, when this trouble is of a serious nature?*" (2)

"The *diagnosis* of diseases," says our author, "depends upon an *exact* knowledge of the symptoms *peculiar* to each one, and, likewise upon the *order* observed in the *occurrence of the symptoms*." (3)

The foregoing remark is prefatory to the analysis of a solitary case appropriated specifically to the "diagnosis." In that instance, as we have seen, the patient, (a gardener,) on reaching the hospital, "said he had been ill *nine* days. He assured me that he had committed no excess." In the case immediately preceding, (Obs. 39,) the same course is taken, and the symptoms fixed upon the glands of Peyer in the usual manner. This patient had suffered diarrhœa for 35 days before he came under medical observation. Our author's comments have their usual interest; whilst they supply the usual proof that he considered "the typhoid affection" to be constituted by a lesion of the glands of Peyer, — Thus: —

"The *course* of the disease was the same as in the preceding observations,

(1) Vol. ii. p. 169, *et passim*. — "Double preuve de l'influence des symptômes cérébraux sur la manifestation de ceux tiennent aux plus graves lésions qu'ils *masquent* dans *presque* tous les cas." — Recherches, etc. t. 2. p. 198.

(2) Vol. ii. p. 145. (3) Vol. ii. p. 265.

since the *lesions* of the small intestine, which had already *retrograded* at the TIME OF DEATH were evidently the *oldest* (!) Nevertheless, in this case, as in the others of which we spoke above, it may be asked AT WHAT PERIOD THE TYPHOID AFFECTION COMMENCED, the patient having had diarrhœa almost constantly from the time of his arrival at Paris, *five weeks before his admission into the hospital.* I fixed this epoch AT THE MOMENT when the general symptoms supervened upon the diarrhœa, and I think it the true time; *for a diarrhœa without fever, without evident loss of appetite, without the least cerebral symptoms, could not be referred to a lesion which constitutes the anatomical characteristic of the typhoid affection.*" (1) (Pp. 712, 759.)

The reader, in considering the foregoing philosophy, should bear in mind, what, indeed, is avowed by the extract itself, that our author insists that "the typhoid affection" is in no respect a "general fever;" whilst it is also worthy of observation that he produces a case, almost immediately preceding, (Obs. 37,) where "the typhoid fever" began from the commencement of the symptoms, and terminated fatally on the 24th day, but where the patient "was able to *work* during *fifteen* of them, and to get out of bed on the day of his death;" "another, who died on the *fourteenth* day, came to the hospital *on foot* on the *tenth*;" and that, "we shall see among the latent cases *still more remarkable* examples of this fact;" (absence of "debility.") The *symptoms* of this *working* patient, who was "*a vender of lemonade*," are described with the most "rigorous" formality from the beginning to the end of those fifteen days. And, whilst upon this subject of the primary symptoms, upon which all our author's essential pathology turns, however the cadaverous lesion of the glands of Peyer may be most conspicuously important, we may quote the following general statement as illustrative of our present purpose, and as a farther means of enabling our readers to judge of the true nature of "the typhoid fever:"—

"Of the 47 patients [who recovered] FROM WHOM I WAS ABLE TO LEARN ANY THING DEFINITE in relation to this point, [*debility,*] 7 had been confined to the bed from the first day of the disease; 9 *merely quitted work* at that period," &c. [Then follows the usual localization of the disease in the glands of Peyer.] "Therefore, in *these* individuals, *as in those who died,* we can refer this *debility only to the commencing specific disease of the elliptical patches,* [!] *or to the same cause to which we must refer this last*;" that is, the subject of Obs. 37.—*Again;* "a few patients were confined to the bed between the 10th and 20th days of the disease only, and one began to lose his strength so as to wish repose from labour not before the 26th day." (2)

Finally, all that we learn from our author as to the authority for those "rigorous facts" which consisted in the symptoms that

(1) Vol. ii. p. 217. (2) Vol. ii. pp. 180, 181.

attended the patients during the many days which preceded their admission to the hospital, and which alone are of any value to science, consists in affirmations like the following,—"*recital of her sensations well given, simply told;*" (1) or, "*I tried to obtain* some account of the state in which he had been previously to his entrance into the hospital, and I learned from those with whom he usually worked, that five days before, that is on the 3d of August, he had been persuaded to leave work," &c. On the 8th this patient with a typhoid *fever* "*came down stairs with a bundle under his arm, saying he was going back to his own part of the country.*" But he went that day to La Charité, and died the next. (2) Five pages are devoted mainly to the structural lesions which thus fell prepared into the hand of science.

But, suppose again, that the "recital of the sensations were well" and truly "given;" what are the *structural* lesions worth in such subjects, and where violent inflammatory disease had run on for weeks without a medical prescription? What were the symptoms worth after the cases came under our author's observation; all complicated as they were by structural lesions of various organs; and of which, let the case to which we have just referred bear testimony? (Pp. 531, 534, 536, 540—541.) In Obs. 23 we have a *mason*, who "fell ill on the 19th Nov. was brought to La Charité on the 28th. He had been *able to walk* there, and *seemed to have a perfect command of his reason when he arrived, saying he had been at Paris* 4 *months, and had been ill ten days, that he had been bled,*" &c. Then follows a very minute account of his symptoms as they were on the following day, which had become of *the very worst* character. He survived just 4 days. About five pages are then devoted to the structural lesions as forming an important ground, with the moribund symptoms of the disorganized parts, for fundamental principles in pathology and therapeutics.

It is not our intention, however, to follow our author through all his cases. If the reader desire farther examples of the foregoing nature, we assure him that he will find in our author's work that his commentator has "tempered justice with mercy."

We have not, however, yet done with the 50 celebrated cases of our author. Something remains to be said as to the foundation of those great principles which relate to the lesions of or-

(1) Vol. ii. pp. 101. (2) Vol. i. p. 82.

ganization, and those "exactly opposite conditions of disease" which result, one in *red* and one in *white* ulceration, thickening, softening, &c. of the alimentary mucous membrane, and that *contra-inflammatory* pathology upon which our author affirms that the structural lesions of the heart, liver, spleen, (1) &c. *always* depend in "the typhoid affection." (P. 701—702.)

It will be recollected that our author, in making up his pathological doctrines, has no farther regard for the symptoms of the typhoid affection, than to make out his case as to the *primary* seat of the disease; and of these symptoms we have shown that he could have had no just knowledge. All the rest reposes mainly upon the *colour* of the parts, *red* and *white* being the standards, and as other shades may approach either of the foregoing, they are appropriated accordingly; being either the result of a "retrograde course," or, at other times, as we should gather from our author, of some intermediate pathological condition between the *red* of inflammatory action and the *white* of a *contra-inflammatory* state. (See pp. 705, 708.) Be this as it may, our author's "Remarks" upon every case consist especially in speculations about the various colours which were observed. It will be also recollected that our author assumes by an analogical induction, that "a really inflammatory redness does not disappear after death."(2) (See *Gen.* 116, p. 748; also, 319—330.) We now come to the *direct* method by which our author ascertained the foregoing fact; and we purpose showing by his statement in this particular, that his cases cannot be allowed to form the basis of any of the pathological conclusions at which he aspires. The objection consists in the lateness of the period, after death, at which the cadaverous examinations were made; since absolute putrefaction must have advanced considerably in most of the subjects, and "meteorism" must, indeed, have be-

(1) We have seen, in respect to these three last organs, that our author arrives at his conclusion that their lesions depend on a cause "*exactly the reverse of inflammation*," upon the ostensible ground that no *pus* was found in the parts. Now let the reader turn to page 713, and observe the case, (Obs. 42,) of which we have there given an abstract. Of the immense intestinal lesions in that case, our author says, "the COLOUR of the elliptical patches of the ilcum, with *that* of the corresponding mesenteric glands, seems to me to show that in *this case*, as in the other, INFLAMMATION *had but a small share in the production of the ulcers.*" Let us now see how far our author observes his rule in relation to *pus*. It is said of the foregoing case, which was a "*perforated*" one, that "between the bladder and rectum there was a large tumbler full of *very thick concocted pus.*"— *Vol. ii. p.* 291.

(2) Vol. ii. pp. 160, 216, &c.

come "formidable." (See Gen. 27, p. 720.) It should be also considered that many of the subjects perished in the hot seasons. As this, however, is a matter of some moment, we have added a "numerical" table of all the cases.

Obs.	Hours after death.	Obs.	Hours after death.
1	23	28	40
2	25	29	34
3	18	30	30
4	36	31	41
5	19	32	28
6	28	33	19
7	20	34	21
8	20	35	23
9	28	36	30
10	30	37	23
11	28	38	21
12	21	39	36
13	43	40	21
14	24	41	20
15	36	42	24
16	22	43	43
17	32	44	33
18	22	45	37
19	29	46	35
20	24	47	22
21	26	48	34
22	22	49	29
23	40	50	38
24	19	51	36
25	17	52	15
26	17	53	26
27	27	54	14

May we not, therefore, on the whole, conclude, that our author's examples of "the typhoid affection" form the most remarkable excrescence that can be found in the annals of medical literature? If there be a greater, *let our author have the benefit of the contrast.*

> "'Tis such a light as putrefaction breeds
> In fly-blown flesh, whereon the maggot feeds,
> Shines in the dark, but, *ushered into day*,
> The stench remains, tho lustre dies away." (1)

When speaking of the accumulation of fæcal matter in our author's patients, (pp. 721, 740,) we neglected to state a *law* in relation to this subject, namely:

150. "In patients who die of *chronic* diseases, the consistence of the fæces does not give us any knowledge of the mucous membrane of the *colon*." "So that if *moulded* fæces do not *prove* the membrane to be in a *healthy* degree of *consistence*, at least it shows that *it is not* RED." (!)

The foregoing law has an important application to another law; our author going on thus:—

(1) Cowper's Conversation.

"The thickening was not connected with softening, save in one of these cases. These facts are not unimportant, and it appears to me they tend to support the opinion previously given in relation to the *non-inflammatory* nature of the softening of the mucous membrane of the alimentary canal, in a certain number of cases." (1)

Nevertheless, we are willing to allow that our author's fabric forms no inconsiderable part of what has been denominated the "science of morbid anatomy," "in a certain number of cases." What we have hitherto said, however, relates only to fundamental defects. There are numerous minor ones, which should never fail of placing a writer, who appears with our author's pretensions, in the rank of an unsuccessful aspirant. But, one of the greatest of these lesser defects has contributed, next to the boldness and novelty of the enterprise, most efficiently to his success and renown. We advert to his constant instability of opinion, and the palpable self-contradictions, which abound in every part of his writings; especially in the work on typhoid fever, — and this even in relation to great fundamental principles which are laid down, and reiterated, in strong and exclusive language. Of this the reader will be abundantly satisfied by comparing the generalizations, and other extracts, which relate to common subjects. There prevails, also, an ambiguity of reasoning, an involution of analogies, conclusions from hypothetical suppositions, which have added not a little force to the more summary generalizations; and, if any manifestation of diffidence appear in respect to the latter, it facilitates their way to more universal favour.

Having heard an apology made for our author in a distinguished quarter, that Chomel was the physician of La Charité, and having supposed that this defence might be attempted from what our author states in his Preface, we set this objection aside at page 685. And, although it be everywhere apparent in our author's work, that he is alone responsible, we will now state his own direct affirmation to this effect, viz: — "Of 88 patients," who recovered of the typhoid affection, "62 were bled. *We abstained* from bloodletting in the others, either on account of," (2) &c. And here we may say, that if 62 of 88 who recovered were really bled, it would appear to be something of a circumstance in favour of bloodletting, when contrasted with the small number that were bled amongst those who perished.

> "'Tis time, however, if the case stand thus,
> For us plain folks, and all who side with us,
> To build our altar, confident and bold."

(1) Typhoid Fever, vol. i. p. 223. (2) Vol. ii. p. 405.

What we have hitherto said, may seem harsh towards one so venerated in science, and so encumbered by laurels; whose name is on the lips of the heathen, as it is a household-word throughout the Christian land. But, besides the obligations imposed by humanity, and by that very science in which he towers prëeminent, to examine with fidelity the system by which a monarch in letters may rule the destinies of his subjects, we have the example of our author himself, of rejecting, with the severest asperity, not only the doctrines, but the labours, of the past and present ages. But, we know no difference betwixt the "authority," and the "opinions," or the "facts," of the living and the dead; though he who values worth may be most disposed to raise his shield when the "mighty dead" call for protection.

"We must address ourselves," says an elegant scholar, and an able statesman," "to enduring public interests, or universal natural feelings, in preference to those which are local and *personal*, artificial or temporary;" "for, in order to advance those large public interests, we must look to the *grand laws*, political or moral, that govern human happiness." (1)

Our author spares no predecessor, personally, or by indirection, from Hippocrates to the dawn of his own empire, — few of his cotemporaries, — scarcely, *posterity;* for, such as do not swear allegiance are doomed to the *guillotine*. Like the rich man, in Quintilian, (2) he poisons all the flowers in the garden, that the neighbouring bees shall get no more honey from them. "Every word he speaks is satire; nothing fats him but other men's ruins." It is all "*tristia de bonis alienis, et gaudium de adversis.*" All are blotted, as it were, from the records of science. It is said by Thucydides, that men are more angry at injustice, than when compelled by force; and that so much should have been even tolerated by authors who are constantly employed about the experience of the past, — whose labours and whose fame are identified with that experience; that they should have encouraged the fatal blow which is thus laid at the very foundation of their own performances, is a new trait in human nature. The virgins who watched in the Roman Temples were doomed to death, if the flames of their altars were allowed to expire. And shall they who watch at the altars of science permit their desecration, or suffer indignities to their gods to pass unavenged?

We will not recapitulate the ground upon which we justify

(1) Verplanck, Oration on the Advantages and Dangers of the American Scholar, p. 37.

(2) Declam. 13.

these remarks, and which may be found especially in our introductory extracts. The sum of the whole is this: —

"The pretended experience of authors is worth nothing, and, after all their assertions and denials we are no farther advanced than before." And, as to the existing and coming generations, "the reader will pardon us for having insisted so much upon the *distrust* with which part of the facts *daily published* ought to be received."

> "Tum pater omnipotens, aliquem indignatus ab umbris
> Mortalem infernis ad lumina surgere vitæ,
> Ipse repertorem *Medicinæ* talis et artis,
> Fulmine Phœbigenam Stygias detrusit ad undas." (1)

Shall we not, then, conclude that we are bowing to a mind that is warped by an ambition that knows no road to fame, but over the ruins of others; which sees nothing of greatness, of toil, or integrity in the god-like Hippocrates, — firmer, if less sacred, than the rock of Mount Sinai; in the immortal Sydenham, or, in that "Second Sydenham," who went to the fountain of nature as the only unerring source of truth; in that man of immeasurable facts, the unexampled and exhaustless Haller; in that profound and philosophical observer, Morgagni, or that other Star of Italy whose beams are still resplendent as ever; in the *matchless* Hunter; in that Meteor which but so lately shot along the horizon of France, and which, *for the moment*, excited the admiring gaze of the two hemispheres; in him, "*cujus nomen tamdiu stabit quamdiu movebitur sanguis*;" in all those luminaries who have left an imperishable lustre upon the Grecian and Roman Empires; and those resplendent Lights which broke through the mists of ignorance and superstition, — and even that Galaxy which remained unextinguished when all else was chaotic darkness. Shall we have no gratitude for those earliest sages from whom our richest gems are gathered? *Nothing* for others who have so exhausted the mine that we do but hunt the rubbish? Shall we not stand up, — albeit though alone, — in this Western Hemisphere, — so late the sole domain of the savage and the beast, — and *thank* our forefathers that we have so soon transformed a benighted continent into a land of science? Shall no selfish principle prompt us to the just or generous deed? Shall we not fear from our example, that none may rise up hereafter "to call us blessed," "none so poor as do us reverence?"

> "We call our fathers fools, so wise we grow,
> No doubt our wiser sons will call us so."

(1) Virg. Æn. vii. v. 770.

Shall we not fear that some coming generation may point to our own as one of injustice and egotism? And, when they shall have thrown off the trammels which have restrained their predecessors, and separated them from the illustrious fathers whose rich bequests they are stealthily enjoying; and, when thus unencumbered by the arrogance of affected superiority, they shall open their eyes upon the glare of those simple ages when Hippocrates, and Aretæus, and Celsus, and their kindred spirits, expounded nature according to her phenomena and laws,—shall we not fear, that they may convict us, by overwhelming proof, of surreptitious invasions upon the rich domain which we have affected to despise,—convict us of imputing hypothesis where it has scarcely a vestige of existence, but where, on the contrary, little appears but the unadulterated simplicity of nature; whilst, by our own showing, they will blush at our inflated systems, or our crude, undigested, materials? Shall we not fear that, when they come to regard those primitive times of facts and philosophy, they may array them in opposition to our own; and that, in thus doing, the veil may be raised by which we conceal the poverty of our resources, the motives of our clamour for "rigorous facts," the cunning with which we have grouped our limited and often spurious observations, or our artificial distortions of nature, and reduced them to all the method of theory, whose most remarkable attribute will be found in the system of the age, and by which we have slid into that holy temple of renown which was designed for holier deeds? Will they not say of us as Seneca of his own,—

"We loathe the very light because it comes free; and we are offended with the sun's heat, and those cool blasts, because they are cheap, and not of our own making. This air we breathe is so common, we care not for it; nothing pleaseth but *our own.*"

Therefore,—

"Do we acknowledge that which Cicero saith, borrowing from Demosthenes, that *bona fama propria possessio defunctorum;* which possession we cannot but note, that in our own times lieth much waste, and that therein there is a great deficience." (1) Or, as Plato has it, οιομενους ειναι σοφωτατους ανθρωπων.

Aristotle had something of this kind in view, when he said there was a certain ancient maxim handed down from their fathers to all men,—that all things are from God, and by God are kept together for us,—"και δια Θεον ἡμεῖς συνεχονται". Xenophon says he was employed about the treasures of ancient wise men,

(1) Lord Bacon, on the Advancement of Learning, vol. i. p. 38.

which they had left to posterity in their books; and that it was his practice to read them to his friends. (1) Lucian affirms the same thing,—that he recited verses, the "experience," and sentiments, of wise men, and useful discoveries, and ancient exploits. (2) (Pp. 580, 582, 586, and Vol. I. 331.)

> "E'en in their ashes live their wonted fires."
> "The glory dies not, and the grief is past."

Our author is pleased to identify "genius" with "chance," or to deride it as "inspiration." Υβριζεις; νυν γαρ ευτυχων τυγχανεις. (3) Well, we are content, having none of the commodity. But we have some *philanthropy;* and therefore, in behalf of genius, we ask but the privilege of exposing the "numerical system." "It is allotted to most men," says Demosthenes, "that when successful, they shall cease to be wise;" or, as he again has it, "to succeed beyond desert, is the occasion of imprudently thinking wrong; wherefore, to have preserved good things, seems often to be more difficult than to have acquired them;"—"Το ευ πραττειν παρα την αξιαν," etc.

Shall we, then, go on with our hosannas; defaming, also, the experience of the past and present, as vague and worthless; crying "Eureka! it is true!"

> "With packhorse constancy to keep the road,
> Crooked or straight, through quags or thorny dells,
> True to the jingling of our leader's bells"?

How little is all that we have seen of our author in accordance with that philosophy which has made the present age; how little with that fundamental principle of the *Novum Organum*, as exhibited in the force of a metaphor, that,

"The empyrical philosophers are like to pismires; they only lay up and use their own store. The rationalists are like the spiders,—they spin out all their own bowels. But, give me a philosopher, who like the bee, hath a middle faculty, *gathering from abroad, but digesting that* which is gathered by his own virtue."

Whilst we thus solemnly reflect upon the subject now before us, let us lay it well to our hearts,—let the conviction be deeply impressed, that selfish ambition must bring its day of retribution; that he who may trample upon the virtues and the rights of

(1) Και τους θησαυρους παλαι σοφων ανδρων, ους εχειποι κατελιπον εν βιβλια γραψαντα, ανελιττων, κοινη συν φιλους διερχεται.

(2) Προϊοντοις σοφων ανδρων γνωμας, και εργα παλαια, και λογους ωφελιμους, ραψωδεομεν αυτοις.

(3) Sophocles.

others, — upon those dispensations of Providence by which He conducts the heathen to an inheritance of light, — by which He makes his intellectual beings objects of mutual sympathy and regard, — that, where the foundation is thus laid upon ruins, he must sooner or later be buried beneath them. Like the fabulous Phœnix, those ruins must again assume their fair proportions, and breathe with greater life than ever. Like a momentary interception of the sun, their splendour shall acquire fresh lustre, as when the opaque cloud shall have been dissipated into airy nothingness.

But, we speak not of those errors which everywhere exist. There are weeds in every garden, and the more prolific the soil, the greater may be their thrift and variety. And, although it behoove the husbandman to prune them with an unsparing hand, he still sees something of beauty, and something to lament, in every falling object. They are all hallowed by nature, and by the associates with which they made their way into life. When, however, he again returns to the scene of his reluctant labour, and finds an unalloyed magnificence around him; finds, that what had met his destructive arm is already administering to the growth and embellishment of gems that shall be coveted in all climates of the land, — then it is that he enjoys the full consciousness, that like all the advantages of life, — like all the operations of the mind, — like all the virtues of the saint, his flowers, though encumbered with weeds, may yet be made to derive health, and beauty, and sustenance, from them.

We have ourselves been engaged in a crusade against what appeared to us the mistakes of great and learned men. "*Raro magni errores nisi magnis ingeniis prodire*," as Petrarch has it. But, in respect to such men, our remarks have had no intended relation to the diminution of their worth. They have been merely applied to some distinct questions in science; and, although we have differed from none without assigning our facts and our reasons, we have still the humility to think that we may yet find ourselves unequal to the task.

"Ten censure wrong, for one who writes amiss;"

And, —

"Those best can bear reproof, who merit praise."

We have sometimes laughed at homœopathy, animal magnetism, and the humoral pathology; but, we have felt no cold-blooded envy at the honours of their several champions, — nor would we,

could we, pluck a laurel from their brows. It may, indeed, be yet our destiny, in being turned adrift by the anatomicals, to enjoy from one a prolongation of life,—from another to obtain a foretaste of futurity,—and from the last, a purgation of all our bad *humours*. Hoping, therefore, but little from the stern philosophy of the first, we shall throw ourselves upon our other friends for all benefits in the foregoing particulars.

Let us remember the admonitions of history, that, when nations have begun to trample upon the past, to reject its experience, and and to strike out new systems of observing nature, it has been the most certain presage of approaching imbecility, and of that ultimate fall to which all are destined. When the great revolution shall have reached the genius of philosophy,—"το κρατιστον της φιλοσοφιας," the last phial of wrath is emptied and that nation is irretrievably gone. This is humiliating to pride, and may have been designed as one of its correctives. But, since *it is so* in the great plan of Providence, it must be sufficiently obvious, that, as a nation approaches its chaotic state, those who may be in the ascendency are bound neither to counteract the order of nature, nor to suffer their own prosperity to be blighted by the mildew.

Ambition must follow the beaten path of philosophy. The denunciation of past experience is the ambition of egotism, which erects its innovations upon error, and imbues them with superstition, and absurdities.

"The wisdom of each age *is chiefly a derivation* from all preceding ages, not excepting the most ancient; just as a noble stream, through its whole extent and its widest overflowings, still holds communication with its infant springs, gushing out, perhaps, in the depths of distant forests, or on the heights of solitary mountains." (1) Or as Horace has it,—"*labitur et labetur in omne volubilis ævum.*"

And thus spake Hippocrates of *his* predecessors,—a fitting commentary upon impracticable systems in medicine, the accuracy of which has been illustrated by all subsequent history; with which, also, we would connect his manly concession to the past, as quoted in our first volume, page 331:

"Medicina autem jam ab antiquo existit, et principium, et via inventa, per quam inventa et multa et probe habentia comperta sunt, per multum adeo tempus, et reliqua deinceps invenientur, si quis sufficiens sit, et jam inventorum gnarus, ex his ad perquirendum procedat. Quicunque vero his rejectis, ac

(1) Channing's Character, &c. of Milton.

omnibus reprobatis, alia via, aliaque forma inquirere conatur, et quid invenisse gloriatur, falsus est, et fallitur. Impossibile enim id est. Verum ob quas necessitates impossibile sit, ego demonstrare canabor, docens ac demonstrans, quod ars existat. Ex hoc autem palam fiet, impossibile esse aliquo alio modo inveniri."

"Nihil enim libentius quisque recordatur, quam quæ sibi ipsi accidisse audit. Si quis autem idiotarum sententia destituetur, et andientes non afficiet, ille etiam veritatis compos non fiet, et propterea, neque fundamento, neque scopo opus habet. Etenim Ars medica neque inventa ab initio fuisset, neque quæsita." "Nunc autem ipsa necessitas homines coegit Medicinam inquirere, ac invenire." (1)

(1) De Vet. Med. v. 18—37.

Whilst we are now quoting this remarkable man for the last time, we may say that he cousidered medicine as the highest of all human pursuits, and to which all other sciences may be made subservient. In urging upon his son Thesalus the study of Geometry and Mathematics, he says,—

"Non enim solum vitam tuam illustrem, mi fili, et ad multa commodam in humanarum rerum statu efficient, sed etiam animam acutiorem, et clariorem reddent ad omnium, quorum usus in medicina expetitur, ultilitatem consequendam." (*a*)—And again:—"Quapropter singula prædicta suscipere oportet, et sapientiam transferre ad medicinam, et medicinam ad sapientiam, *Medicus enim philosophus est Deo æqualis.*"—He also enjoins upon the physician "alienitatem a superstitione," because it is impossible to be superstitious and see the truth. (*b*)

And so Democritus:—"Sapientiæ cognitionem medicinæ sororem ac contubernalem esse puto." (*c*)—Medicine, indeed, has been considered by nearly all eminent physicians as an attainment which involves the whole range of philosophy; and therefore a science which has the broadest and deepest foundation in nature. Baglivi says of it,—

"Ad medicinam secundam, sive curativam morborum, fateor sane plurimum lucis allatura esse aliarum scientiarum studia, et præsertim illarum, quæ cognationem aliquam habet cum medicina." "Studia mathematicæ, meteororum, astronomiæ, eloquentiæ, philosophiæ moralis, medicis tantopere necessaria." (*d*)

From Hippocrates, the first, to Paul, the last of the Greeks, through the lapse of a thousand years, we find that they only were distinguished in medicine whose works are imbued with all the knowledge of their times; and what was physick in Rome till she received the literature of Greece? Arts and sciences, from the age of Paul, fell into neglect; and, till their general revival, medicine presents, throughout Europe, nothing but a chaos of absurdities.

Blackstone advises physicians to study law, "to complete their character of general and extensive knowledge; a character which their profession, beyond others, has remarkably deserved." (*e*) (See Bacon, Johnson, Laplace, Lacroix, Condorcet, pp. 582, 588.)

M. Sainte-Maria has written an Essay to prove that physicians are all the better for being poets; and "the instance of Darwin is alone sufficient to prove that the most exact practical knowledge may be united with the most poetic fancy." (*f*) Bacon has a happy comment upon this subject:—Varia vita et subtilis corporis humani compositis et fabrica efficit, ut sit instar organi musici, operosi, et exquisiti,

(*a*) Ep. ad Thessalum. (*b*) De Decent. Ornatu, v. 51—57. Also, de Medico; and, de Arte.
(*c*) Epis. de Nat. Human. v. 3. (*d*) Prax. Med. l. 1. c. 5. s. 5.
(*e*) Commentarics, &c. vol. i. (*f*) London Med. Repository and Rev. vol. xxv: p. 472.

Let us hear another philosopher of universal esteem, and of acknowledged impartiality: —

"While it is undeniable that our science has made more advances in modern times," says Dr. Brown, "yet it appears to me that the principal improvement

quod harmonia sua facile excidit. *Quare apud poetas summa ratione musica cum medicina in Appolline conjungitur.*" Whilst, therefore, he infers the greatest difficulties in the science of medicine, he argues that it is as harmonious as the harp of Apollo. (*a*) True, in speaking of the erudition of physicians, he admonishes them to apply their learning to professional objects, and remarks, — "Invenies etenim inter eos, poetas, antiquarias, criticos, rhetores, politicos, theologos, atque in iis artibus magis, quam in professione propria eruditos." But this applies only in a specific, not in a general sense; and it is generally true, in principle, of every other human pursuit. Each mind knows its own aptitudes. It is this consciousness which determines the course of ambition. "Bona spe (επ αγαθη ελπιδι) fretus lætatur, quemadmodum qui feras venantur, spe capiendi perlubenter laborant." (*b*)

"I will allow," says Zimmermann, "that in many cases, the most learned physician is of little consequence. A physician, who has no other erudition than what depends on memory, may know much, and yet be very stupid; and, as the practice of physic depends wholly on genius, it will of course happen, that a stupid man of learning will be a very indifferent physician." (*c*) "A connoisseur in painting," says Magendie, "when his sight is very bad, experiences in seeing a beautiful painting, a hundred times more pleasure than an indifferent judge, though he examine it with good eyes; and the connoisseur preserves the image of it long after the other has lost it." (*d*)

There is a broad distinction betwixt simple judgment and "*genius.*" Like most other things, this has been defined, and we will therefore appropriate it *as we find it.* "By genius we understand a high degree of refinement, sensibility, and penetration, or an extreme degree of perfection in all the faculties of the mind." "A man who possesses a just judgment sees the dependence of an idea when it is shown him; a man of genius finds it of his own accord." Or, as Cicero says, it is the privilege of genius, to see in its dreams, what humbler *men* ascertain only by laborious toil. "Genius," says another, "perceives and comprehends, in a given time, an infinite number of things, which others, who have less genius, can either not see at all, or require more time to see." Or, as our principal author has it, "genius is chance, inspiration; for what is inspiration but chance?" Judgment and patience make the mathematician; these united with sagacity, the politician; the physician requires them all, with the light of imagination. It is the opinion of Freind, that medicine requires a more penetrating genius than any other pursuit. In this Zimmermann agrees. "Every branch of physick," he says, "does not seem to require the same genius. Anatomy, botany, and the knowledge of medicines, seem to require more time than genius. Physiology, pathology, and semiology, require more genius than time. The *practice* of physick requires very little time, but a great share of genius." This is the reason why so many, "finding giants in zeal and knowledge to contend with for public favour, are daily quitting the ranks of the medical profession, from sheer inability to succeed, however hard they may have studied." It is one of the remarkable characteristics of cultivated genius, that it rarely wears out; and least of all is it inconsistent in its principles or inclined to superstition. Where either of the latter exist, there is no genius, nor can it be appreciated in others. On the other hand, ordinary minds

(*a*) De Argument. Scient. l. 4. c. 2.
(*b*) Xenophon Memorabilia, l. 2.
(*c*) On Experience in Physick, vol. ii. p. 17.
(*d*) Notes to Bichat, &c. p. 165

which has taken place is, that a great number of practitioners are now masters of the existing knowledge, and hence that a greater number of practitioners are not discoverers, but intelligent and industrious appliers of the discovered truths, both as to diseases and their remedies." "If we are not better acquainted than the ancients with the *proximate causes* of diseases, we are at least much better acquainted with their *effects*, from the study of morbid anatomy." "Disease appears to be as rife now as it was in former days, and our vaunted improvements in the materia medica have not made death less frequent than before, have not added to the average duration of human existence." (1)

And thus another, profound in the lore of antiquity: —

"We have been long advancing in medicine; and though we admit most fully the vast strides which have been made, still we must declare our firm conviction, that the study of the older authors is too much neglected, and that in them we shall find a treasury of knowledge, much of which we may think to be the production of our own times." (2)

Our principal author, however, for the purpose of abolishing the experience of the past, had endangered the stability of his own; but for the security with which he invests them by erecting the whole into general laws: Thus, —

are often in ruins long before there be any obvious failure in the organic functions. Celsus thinks that, there should be an element in a physician which can neither be defined, nor easily comprehended. Is it not that of which we have been speaking?

It has been well said, that there may be a great distinction between "erudition and true learning. Erudition, considered by itself, is a mixture of good and bad things, often contradictory to each other, and badly digested; which burthen the memory, at the expense of common sense, and render the simply lettered man rich in provisions that are useless, and poor in ideas; *great in minute things, and very little in great things.* A man of erudition, may at the same time, be a very great simpleton; whereas, a man of true learning must, necessarily, be a man of genius. He not only is acquainted with the sciences which depend on reasoning and memory; but he possesses a true spirit of philosophy, which forms, as it were, *the soul of learning.*" — And here we call, especially, the attention of our readers to the following remark of this profound philosopher, which is so appropriate to our text: —

"Filled with the blindest arrogance, or influenced by the meanest views, many modern practitioners, or those whom I call empyries, reject, with reason, that which would be able to unmask them. *They affect to despise learning, because they are deficient in it. They deny the erudition and discoveries of all ages, turn into ridicule all the laws of reason and analogy, that they may be the better able to persuade the ignorant public, who listen to them, that all that is good and useful originates with themselves.* The public honour in them their own prejudices, and their vile souls attribute to themselves, the public respect; as the ass in the fable did the homage paid to the statue of Isis, which he carried upon his back." (*a*)

Galen complains that the fine arts and philosophy were considered useless to the physician; and he ascribes the neglect of them to the corrupting patronage which awaited those who administered to the prejudices of the public, and spent their time in pursuits to which the public was addicted.

(1) William Brown, in Edin. Med. and Surg. Journ., Oct. 1839.

(2) Stokes; Theory and Practice of Medicine, p. 6. 1838.

(*a*) Zimmermann, on Experience in Physick, vol. i. pp. 44, 75.

151. "It is not true, moreover, as has been said too often, that FACTS do not become *old*." "The IMMENSE MAJORITY of them have become so; and, moreover those which we collect IN THESE TIMES, will, in like manner, in their turn, become old." "The best work is good only in relation to the epoch at which it appears." (1)

The foregoing doctrine is subversive of all physiology, and of all principles in medicine.

"Can length of years on God himself exact?
Or make that fiction which was once a FACT?" (2)

But, mark our author's consistency when other objects are contemplated:—

"A leaf of a tree ONCE well described may *always be recognized*; and *general principles of pathology*, ONCE clearly defined, *can always be verified* under circumstances similar to those in which the subjects were placed, *from whom* such general principles were derived. Thus, in truth, we can form *a class of* FACTS bearing sufficient resemblance, one to another, and from hence deduce LAWS which *every day's experience* verifies;" — quite as much so as "a leaf of a tree when once well described may always be recognized." And then follows one of those never ending affirmations that, — "Reasoning A PRIORI *is the habit of those physicians who oppose this method which is known as the numerical method.*" (3)

And again, our author having another point to carry, concludes "that *facts confirm facts*, and when one conclusion has been *rigorously* deduced, *every circumstance* which can be referred to it is a *new proof of the truth of it.*" (4)

Since, therefore, all principles and generalizations consist of facts alone, we must hold our author to his last conclusion, that "facts," do not become old.

Hope of distinction and immortality, in medicine at least, must rest in a great measure, hereafter, upon the development of facts that shall illustrate principles and laws already known; or upon something, like the discovery of vaccination, that shall bestow great benefits upon man. We are apt to esteem an isolated, practical result, more than fundamental principles; and he, therefore, who shall discover a remedy for hydrophobia, or consumption, or the cholera, or who may invent a machine that may greatly improve the condition of society, though he be otherwise a dolt, may go down to posterity with Hippocrates or Newton. Possibly, something of this nature may be yet gleaned from morbid anatomy; though it be a mere mechanical pursuit, and wholly subservient to a system of philosophy which reposes upon an intellectual basis. But, we must make up our minds that there can be no second "infancy," and no second "father of medicine." System after system, as in former times, that may depart from

(1) On Typhoid Fever, Advertisement, p. 9. (2) Cowper's Conversation.
(3) On Bloodletting, p. 57. (4) On Typhoid Fever, vol. i. p. 228.

the Hippocratic, must soon fail and be forgotten ; or, at most, will be remembered only as the dream of the visionary, or the project of the ambitious, or of the unscrupulous man.

Had we been honest, had we been discreet, had we been even prompted by the selfish motive that should have led us to apprehend that posterity may take our example and strike us from the roll of time, and have cherished our fathers as an important inheritance, then should we have gone on adding value to their labours, instead of bringing our science to a state of "*infancy*." "*Vires acquirit eundo*." It is greatly with medicine, for reasons which we have stated, as with the poetry and the oratory of the ancients ; and we may as truly say of the former as of the latter, that, —

"If any one, at this day, in the 18th century, takes upon him to decry the ancient classics ; if he pretends to have discovered that Homer, and Virgil, are poets of inconsiderable merit, and that Demosthenes, and Cicero, are not great orators, we may boldly venture to tell such a man, that he is come too late with his discovery."

"It may be said," continues Blair, "not altogether without reason, that, if the advancing age of the world bring along with it more science and more refinement, there belong, however, to its earlier periods, more vigour, more fire, more enthusiasm of genius." (1)

It was the opinion of Dutens, as expressed in his *Recherches sur les Découvertes attribuées aux Modernes*, that "nearly all the discoveries attributed to the Moderns were not only known to the Ancients, but ably illustrated and supported by them." Fenelon, in speaking of the past, says, "I urge it upon all who adorn the present age not to neglect what so many ages have admired." (2)

"At the same time, a just and high regard for the prime writers of antiquity is to be always distinguished from that contempt of everything which is modern, and that blind veneration for all that has been written in greek and latin, which belongs only to pedants." (3)

The principle is, that, the "*indocti discant, et ament meminisse periti ;*" and "Here at each step, imagination burns." As far back as Rhazes, it was said by this observer, that he would prefer a well-read physician who had never seen a patient, to one who should be ignorant of what has been said and written by the ancients.

"Whatever may be the resources of your own refined genius," says Freind in a letter to Mead, "*blush not to own the abundant harvest you have gleaned from the writings of our masters.*"

(1) Blair's Lectures on Rhetoric, vol. ii. pp. 249, 254. (2) Œuvres, t. 7. p. 206.
(3) Blair, ut cit. vol. ii. p. 258.

And, of this sentiment was the Baron Cuvier, who lamented, that, —

"Heaven had not given me the eloquence of heart which you so much admire in my venerable colleague. How would I then depict to you the difference between the individual without books, and the one who is so fortunate as to meet daily in his library the minds of the mighty dead!" (1)

"*Possum multa tibi veterum precepta refere.*" (2)

"Utilitate hominum nil debet esse homini antiquius." (3) "Quæ vero pro utilitate reipublicæ scribuntur, *eterna.*" (4)

What was true in Seneca's day, is not less so in ours: —

"Onerat discentem, turbat non instruit; multoque satius est paucis te authoribus tradere, quam errare multos." (5) Or, as Scaliger has it, *hyperbolically*, — "Inter tot mille volumina vix unum a cujus lectione quis melior evadat, immo potius non pejor." (6)

The very fact of the rejection of all antiquity, and the intervening periods, is an admission, on the part of the objectors, that they have departed from the Hippocratic mode of observing Nature; but which, having been continued in all other sciences, has constantly maintained their onward march. Bacon, in intellectual, Newton, in mechanical, and Davy, in chemical philosophy, did but carry out the Hippocratic system. They regarded Nature in her living operations, not when her processes were finished and she had come to a stand. But, medical inquirers, who cut off Hippocratic experience, renounce also the method. This is an unavoidable result; otherwise the experience of the past would still be available. Hence has arisen the declaration of our author, and his school, that "medicine is now in its infancy." And, with such a positive avowal, shall we not be justified in imputing to the ancients, who had brought medicine to a remarkable state of maturity, better habits of observation, more knowledge, and more practical wisdom, than the contemners of antiquity, who, by their own admission, have not yet advanced beyond the "infancy of medicine"? And so it has been at every age of the science. Paracelsus, Brown, and all others, who have cast away the past, have declared medicine "to be in its infancy." It would follow, therefore, by the most irresistible analogy, that, since all other sciences have been steadily progressive, and would now seem to have almost reached their zenith, — and, since the human mind is the same at all ages, — it would follow, that medicine is insusceptible of cultivation or advancement. If this conclusion be not admit-

(1) Discourse. (2) Virgil, Georg. l. 1, v. 176. (3) Baglivi, op. p. 27.
(4) Vegetius, de Re Militari. (5) De Tranquil. Animi, c. 9. (6) Epis.

ted, then, by the same analogy, it must be conceded that medicine has kept some sort of pace with those sister sciences which have been the product of the human understanding. We look with admiration upon the poetry, the history, the arts of refinement, the intellectual taste, the surpassing genius of our remote ancestors; nay, with a sense of mortification that in these respects, so open to the understanding, we have made little or no advancement beyond them. Blair thinks, that "for all that belongs to original genius, to spirited, masterly, and high execution, our best, and most happy ideas are, generally speaking, drawn from the ancients," and that "there are commonly none but the ignorant and superficial who undervalue them." The phenomena of disease were peculiarly attractive to such an order of mind; and, springing then, as now, from the great laws of organization, the latter were grasped as were the fundamental principles of taste, poetry, &c. Medicine was even more an object of solicitude to every class of society than at the present moment. Witness the daily records of disease, made in the public places by the common passenger. See the sovereigns, and princes, paying homage to medical science; laying at the feet of its professors, unmeasured wealth, and unlimited honours. (1) And see, on the other hand, that soft inspiration, which can belong alone to genius enlightened by the best philosophy, spurning the allurements as incompatible with the divine science, when cultivated in its purest and most lofty bearing. And witness, again, the same universal, deep-felt interest in its advancement, in those devotional monuments to the genius of medicine which were spread over the land. "*Primi inventores judicaverunt artem dignam quæ Deo adscriberetur, quemadmodum etiam receptum est.*" (2)

Franklin has drawn a vivid contrast between the progress of the sciences and arts before the 15th century. Of medicine, he says,—

"They had surgery without anatomy, and physicians without the materia medica; they gave emetics without ipecacuanha, drew blisters without cantharides, and cured agues without bark." (3)

If this be so, however, they "cured" their patients, "drew blisters," and had their "surgery." The former was done by ade-

(1) "Dabo igitur," says Artaxerxes, "ipsi auri quantum volucrit, et reliqua abunda, quibus opus habet, et ipsum ad nos mittito."—*Epis. Artax.*

(2) Hippocrates, De Vet. Med. s. 215. (3) Franklin's Works, vol. iv. p. 374.

quate means, and through an enlightened knowledge of pathology, the remote causes of disease, &c.; and, as to surgery, perhaps their want of our more exact anatomical knowledge was counterbalanced by the greater infrequency of surgical operations. Our object, however, in quoting the foregoing paragraph, is to say, that if we allow the conclusions to be right as to many of the most important sciences and arts, but utterly wrong in their spirit as to medicine, we know of no commentary which places the early progress of medicine in such forcible contrast with other human pursuits, as this paper by Franklin, on "Discoveries." The science of medicine has no special relation to the accidental discovery of a drug that may produce emesis, or a fly that may irritate the skin; and mere surgery is purely mechanical, having no connection with the profound principles of medicine. Surgical diseases, however, belong to the physician, and come, therefore, within the province of science.

It was a saying of Alonzo of Arragon, in commendation of "age," that "it appeared to be best in four things; old wood best to burn; old wine to drink; old friends to trust; and old authors to read." He was also wont to say of himself, that "*he was a great necromancer, for he used to ask counsel of the dead.*"

"*Spartam nactus, hanc orna.*" "This," says Verplanck, "was the law of our forefathers; under this we ourselves were born and bred. It is susceptible of indefinite improvement without losing its substantial excellencies. Let us then prune off its deformities; let us remedy its defects, whilst we reverently guard its substance.

"The wisest and the most efficient reformers, and those whose works last the longest, are they who build on the old foundations. Their works have not the systematical beauty of the wholesale reformer, but they prove far more convenient for all the varied uses of society.

"A great German poet, (Schiller,) has embodied this truth in noble and philosophical imagery. The path of mere power to its object, says he, is that of the cannon-ball, direct and rapid, but *destroying every thing in its course, and destructive even to the end it reaches.* Not so the road of human usages, which is beaten by the old intercourse of life; that path winds this way and that, along the river or around the orchard, and securely, though slowly, arrives at last to its destined end. 'That,' says he, 'is the road on which blessings travel.'"

As to our humble selves, we may be permitted to say, that we never read the most ancient writers on medicine without instruction, and never without wonder and enthusiasm. And, if what we have now said of the estimation in which the fathers of medicine have been held by all learned successors, should, in some measure, counteract the growing prejudice against this source of much of our best experience, and many of the best principles in science, we shall consider ourselves justified in having made this defence. There are treasures in Hippocrates which none can realize without exploring the mine. It is everywhere adorned with many of those gems which now decorate the brows of others; and there, too, you will find that he was not only great as a philosopher, but as a practical and virtuous man;—ever inculcating the rule that "severe diseases require severe remedies;" looking at all the relations of disease, whether in respect to its phenomena, or the contingent influences of climate, constitution, age, &c.; and ever yielding their meed of praise to others. (')

But, though the present generation may be too much occupied with the discoveries, or the hypotheses, that are everywhere in progress, to enjoy much of this great master of the healing art, he cannot remain long neglected. "*Time*," says Sophocles, "*both degrades and raises again all human things.*" (2) It is the ultimate disposition of man to prefer the substance to the shadow; and, since nothing is dearer to our race than what concerns the preservation of health and of life, the writings of this prodigy of the world will be the last that shall be lost in the wreck of time.

"Here then we rest; the *Universal Cause*
Acts to one end, but acts by *various laws.*"

(1) Hippocrates advises Crateva, that he should "cut up amongst other herbs, that weed, envy, by the roots, that it may never sprout again; for, in so doing, you shall not only cure quickly all diseases of the body, but of the mind also." Hippocrates often complains of having met with this "weed" in his walks. (*a*) (Vol. i. p. 310.)

"I have read," says Marcus Aurelius, "Greek, Hebrew, Chaldee Authors; I have consulted with many wise men, for a remedy for envy. I could find none, but to renounce all happiness, and to be a wretch, and miserable forever. Every other sin hath some pleasure annexed to it, or will admit of an excuse. Envy alone wants both. Other sins last but for a while; the gut may be satisfied; anger remits; hatred hath an end; envy never ceaseth."

(2) Ημερα κλινει τε καναγει παλιν
Απαντα τα ανθρωπεια.

(*a*) Epis. ad Cratevam, v. 12—14. See, also, Epis. ad. Senat. Pop. Abder. v. 21. Epis. ad Damagetum, v. 136, et passim.


FINIS.